Nature, May 30th 1878.

William Harvey.

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NATURE

A WEEKLY

ILLUSTRATED JOURNAL OF SCIENCE

VOLUME XVII.

NOVEMBER 1877 to APRIL 1878

"*To the solid ground*
Of Nature trusts the mind which builds for aye."—WORDSWORTH

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A WEEKLY ILLUSTRATED JOURNAL OF SCIENCE

"*To the solid grouna*
Of Nature trusts the mind which builds for aye."—WORDSWORTH

THURSDAY, NOVEMBER 1, 1877

THE SUN'S DISTANCE

A MOST interesting state paper has just been issued; we refer to the Report by the Astronomer-Royal on the Telescopic Observations of the Transit of Venus of 1874, made by the Expeditions sent out by the British Government and the results deduced from them. The Astronomer-Royal suggests that another report may be called for when the photographs of the transit have been completely measured and worked out, if possible in combination with the results of similar observations made in the expeditions organised by other governments.

It will be seen from the present Report that the plan of operations actually pursued has been very nearly that proposed by the Astronomer-Royal in his communication to the Royal Astronomical Society on December 11, 1868, when for the third time directing attention to the arrangements which it would be necessary to make for the efficient observation of the transits of 1874 and 1882. The method of absolute longitudes was to be applied for observations both of ingress and egress; it being therefore essential that the longitudes of the observing-stations should be determined with precision; and the longitudes recommended to be fixed by Great Britain were Alexandria, stations in New Zealand and in the Sandwich Islands, Kerguelen's Land, and Mauritius or the two islands of Rodriguez and Bourbon.

The stations eventually selected for observations by the British expedition were fixed upon "entirely by consideration of the influence which their positions would have in determining with accuracy the necessary alteration of parallax." They were: Egypt, the Sandwich Islands, the Island of Rodriguez, New Zealand, and Kerguelen's Land. It was intended to adopt in each of these districts one fundamental station, the longitude of which was to be independently determined, for conversion of local times into Greenwich times, and subordinate to this primary station, other stations were proposed to be selected at such distances that advantage might be taken of different states of weather that might possibly prevail.

In Egypt his Highness the Khedive rendered every possible assistance, tents being supplied with military guards for the protection of the observers and their instruments, and telegraph wires erected. The Astronomer-Royal acknowledges the obligations of the expedition to the liberality of the Eastern Telegraph Company, in affording the means of determining with extreme accuracy and great facility the longitude of the principal station Mokattam. Greenwich was easily connected with Porth Curno, in Cornwall, whence there is an uninterrupted line to Alexandria, the longest submarine line in the world; Alexandria was connected with Mokattam by aid of the special line constructed by the Khedive from Cairo to the station. It is further stated that time-communication was also made from Mokattam through Cairo to Thebes, and to Suez by the ordinary telegraph, Thebes and Suez being the other Egyptian stations where the transit was observed.

In the Sandwich Islands much assistance was received from King Kalakaua and members of the reigning family. The principal station was at Honolulu, the longitude of which was determined partly by meridian-transits of the moon and partly by transits of the moon observed with the Altazimuth instrument. Waimea, in the island Kauai, where observers were also placed, was connected with Honolulu by means of chronometers carried in H.M.S. *Tercdos*. At the Island of Rodriguez the longitudes were determined in the same manner as for the Sandwich Islands stations, for three positions, viz., Point Venus, the Hermitage, and Point Coton; and communication was further made with the Mauritius and with Lord Lindsay's expedition with the aid of H.M.S *Shearwater*, the preliminary results being stated by Sir George Airy to agree closely with those given by the lunar observations. At Kerguelen's Land, again, the operations were similar; Supply Bay and Thumb Peak being the stations chosen.

In New Zealand unfavourable weather much interfered with the observations, and Sir George Airy had at first been led to suppose that all useful observation had been lost; it subsequently appeared, however, that this was not the case, one phase of the transit being well seen at Burnham, the longitude of which was fixed by meridian transits of the moon.

The Report is divided into three sections or tables.

In the first are given the descriptions of the various phenomena, in the words of the observers, with the Greenwich sidereal times of the different phases, obtained from accurate reduction of the observations for longitude here particularised; where such longitudes depend upon lunar observations the places of the *Nautical Almanac* were carefully corrected by observations on nearly the same days at Greenwich, Paris, Strasburg, and Königsberg. In studying these original descriptions, Sir George Airy was led to infer that it was "possible to fix upon three distinct phases for the *Ingress* and four for the *Egress*," though it might have been supposed that Egress and Ingress would exhibit the same number of distinct phases in inverse order; this was not the case in practice. The first phase, a, utilised in the calculations is the appearance of the planet just within the sun's disc, but the light between the two limbs being very obscure. After an interval of about twenty seconds "the light begins to clear, and the observers generally think that the contact is passed;" this is phase β. About twenty seconds later, the light which at phase β was not equal to that of the sun's limb, is free from all shadow, and the phase is called γ. Sir George Airy finds that of these phases β is the most exact, observers, even in the presence of clouds of moderate density, agreeing within three or four seconds, though for other phases much greater discordances are exhibited. Similarly at the Egress, the first appearance of a fine line or faint shadow is called δ, this becoming definite, or a "brown haze" appearing, is called ϵ. When most observers record "contact," the shadow having reached a maximum intensity, the phase is called ζ, and in this phase there is an agreement amongst observers, much closer than in other phases at Egress. The "circular" contact at Egress is called η.

In the second section of the Report, or Table II., these "adopted phases are massed for each district in which the parallax-factor is nearly identical," and several of the details of reduction are included. With the longitudes determined as above, the recorded times of the various phases of the transit were converted into Greenwich sidereal times. With the calculated apparent places of the sun and Venus in the *Nautical Almanac*, as deduced from Leverrier's Tables, an ephemeris was prepared exhibiting the predicted geocentric places for every tenth second of Greenwich sidereal time throughout the transit, and from these numbers the apparent positions of sun and planet at each station were computed. Calculations were further made, showing how the predicted places would be affected by alteration of the local longitude, by change in the tabular places of the sun and Venus, and by alteration of their tabular parallaxes; the first two alterations were not essential in these reductions, but the determination of alterations of the third class, as it is remarked, constituted "the special object of the expedition." The form of the reductions was "entirely determined by the consideration that such alterations must be made in the parallaxes as will render the observations of the same phenomena in different parts of the earth consistent with each other." In Table III. we have "the mean solar parallax deduced from every available combination." Thus Ingress accelerated at the Sandwich Islands is compared with Ingress retarded at Rodriguez and with Ingress retarded at Kerguelen's Land; Egress retarded at Mokattam and Suez with Egress retarded at Rodriguez, and likewise with Egress accelerated at the two stations in Kerguelen's; and again the retarded Egress at Thebes is compared with Egress retarded at Rodriguez and with Egress accelerated at Kerguelen's. The greatest separate value of the solar parallax resulting from these different comparisons is 8″·933 and the least 8″·407. Weights are given to the various determinations depending, firstly, upon the number of observations and the magnitude of the parallax-factor; and secondly, upon the particular phase a, β, γ, δ, ϵ, and ζ being included. Thus it is found that all the combinations for *Ingress* give the mean solar parallax 8″·739, weight 10·46, and all the combinations for *Egress* give 8″·847, weight 2·53, whence the general result is 8″·760, from which Sir George Airy finds the mean distance of the sun equal to 93,300,000 miles. The New Zealand observations were not included in these calculations; their mean result is 8″·764, almost identical with the above. It is remarked that many persons may perhaps consider that the more closely-agreeing phases β and ζ should be employed in deducing the value of the parallax to the exclusion of the others. If this be done we shall have from the *Ingress* 8″·748, and from the *Egress* 8″·905, or with their due weights a mean value 8″·773.

In this outline of the details contained in the Astronomer-Royal's first Report upon the observations of the transit of Venus, and the conclusions to be drawn from them we have adhered closely to his own words. Pending the appearance of the deductions to be made from the complete measuring of the photographs, the results before us are perhaps to be regarded as provisional ones only, or we have not yet learned all that may be done from the work of the British expeditions, so laboriously organised by Sir George Airy. Many astronomers we can imagine will regard with some suspicion so small a parallax as 8″·76, which is a tenth of a second less than has been given by the most reliable previous investigations, upon different principles. In illustration we may quote the separate results from which Prof. Newcomb obtained his value of the parallax, now adopted in most of our ephemerides:—

From meridian observations of Mars, 1862	8″·855
From micrometric observations of Mars, 1862 ...	8·842
From parallactic inequality of the moon	8·838
From the lunar equation of the earth	8·809
From the transit of Venus, 1769 (Powalky's reduction)	8·860
From Foucault's experiments on light	8·860

To these may be added Leverrier's value subsequently deduced from the planetary theories, which is also 8″·86. Newcomb's mean figure, taking account of weights corresponding to the probable errors is 8″·848, which, with Capt. Clarke's measure of the earth's equator, implies that the mean distance of the sun is 92,393,000 miles. Sir George Airy's 8″·760 would similarly place the sun at a mean distance of 93,321,000 miles.

It is well known that some astronomers have not expected our knowledge of the sun's distance to be greatly improved from the observations of the transit of Venus, regarding such an opportunity as is presented by a close opposition of Mars as affording at least as favourable conditions, and the result of Mr. Gill's expedition to

Ascension to utilise the late opposition will be on this account awaited with much interest. Nevertheless, whatever degree of opinion might be entertained by competent authorities, it appears to have been felt by those immediately responsible for action, in different civilised nations where science is encouraged, that so rare a phenomenon as a transit of Venus could not be allowed to pass without every exertion being made to utilise it, and this country may lay claim to an honourable share in the great scientific effort, thanks mainly to the long-continued and admirably-directed endeavours of the Astronomer-Royal to secure this result.

Several of the stations occupied during the transit of 1874 will be available for the transit of 1882, Kerguelen's Land in particular, where at Ingress the sun will be at an elevation of 12°, the factor of parallax being 0·98. In that year there will also be the advantage of observations along the whole Atlantic sea-board of the United States and Canada, where, as pointed out by the Astronomer-Royal in 1868, the lowest factor is 0·95, and the smallest altitude of the sun 12° for observing the retarded Ingress; and for observing the Egress as accelerated by parallax, the factors are about 0·85, the sun's elevation varying from 4° at Halifax, to 32° at New Orleans, or Jamaica. Australian and New Zealand stations are important for retarded Egress.

As is well known, the transit of Venus on December 6, 1882, will be partly visible in this country.

PARKER AND BETTANY'S "MORPHOLOGY OF THE SKULL"

The Morphology of the Skull. By W. K. Parker, F.R.S., and G. T. Bettany, M.A. (London: Macmillan and Co., 1877.)

IN the minds of most of those who have paid no special attention to the subject the skull is regarded as a bony case formed to contain the brain, together with the face. There is also a constancy in the number and position of these bones which lead to the apparently necessary conclusion that occipital, sphenoid, parietal, and other elements are fundamental cranial structures; so that an exhaustive study of their relationships and variations might be thought entirely to cover the subject of skull structure.

That such is not the case has dawned upon us since the elaborate researches of Rathke and other able embryologists, among the foremost of whom must be placed Profs. Huxley and Gegenbauer, who have been followed by Mr. Parker, the author of the work under consideration, who on account of his peculiar aptitude for manipulation, his untiring zeal and his immense experience, has placed the subject of cranial morphology upon a footing infinitely more satisfactory than it has previously been. His numerous memoirs in the *Transactions* of the Royal, Zoological, and Linnean Societies form a mine of biological facts, so beautifully supplemented by their accompanying illustrations. The perusal of them all, in their proper sequence, is however a task only to be undertaken by the specialist, and it is on this account that we have no small degree of pleasure in being able to give a notice of "The Morphology of the Skull," a work of less than four hundred pages, in which is collected, condensed, and digested the mass of information spread through the larger memoirs.

The work consists of a series of chapters on the skulls of carefully-selected types of the five classes of the Vertebrata. Those chosen are:—

1. The Dog-fish and Skate.
2. The Salmon.
3. The Axolotl.
4. The Frog.
5. The Common Snake.
6. The Fowl.
7. The Pig.

These are each described in all stages from their earliest appearance in the blastoderm to their adult condition. Following each chapter is a brief *résumé* of the peculiarities which have been observed in other members of each group, in such a manner that the student of any particular form can learn almost all he may require with reference to any special member of the sub-kingdom.

The primitive trabeculæ cranii, together with the parachordal cartilages and the branchial arches are traced from their earliest development until ossification in and around them has reached the limits of the different types. The insufficiency of our data for the determination of the cranial segments is prominently brought forward, although the moniliform constrictions of the anterior extremity of the notochord in the fowl and in the urodeles is stated, and thought to suggest a segmentation. On the subject of the vertebral theory of the bony skull, Mr. Parker tells us that "only one bony segment, the occipital, can be said to be clearly manifest in the skulls of fishes and amphibians. And in these forms there are no good grounds for assigning to the cranial bones special names indicating a correspondence to particular parts of vertebræ. From the study of adult structures in the mammalian groups skull-theories have been devised, lacking the basis of embryology; and granting that they express some of the truth respecting the highest forms of skull, there is only injury to knowledge in arbitrarily interpreting the lower forms by them. In reptiles the skull becomes much more perfect, but with wide variations in the different groups, such that they cannot be merely subordinated to and explained by the mammalian type. A careful study of the growth of the bird's skull, again, will show that it is impossible to express its composition on a simple formula derived from vertebral structures. But from the lower to the higher forms of vertebrates we can discern a growing away from the primordial type of skull towards and into a loftier development." This result of the extensive investigation upon which it is based is somewhat paradoxical. The "loftier development" of the highest types results in a skull some of whose components may be compared in detail with some expression of truth to vertebræ, whilst in the lower forms a similar comparison cannot be said to hold. And yet true vertebræ themselves, fully developed as far as their essential details are concerned, are found in forms far from high in the scale.

Mr. Parker's invaluable investigations besides their importance in a comparative anatomical point of view, have done much to demonstrate the degree of stress which must be laid on facts of cranial structure in problems relating to classification. His labours have led him to elaborate the instructive classification of birds

promulgated by Prof. Huxley in 1867, and so to bring out many points of special interest in avian cranial osteology, demonstrating most clearly the principle which may be arrived at from the study of any special organ or single structure, that a fact which is of the greatest significance in determining the relationships of some one collection of species or genera, may be valueless in attempting to classify others. As an instance of this we may take the skull of the woodpeckers and wrynecks, the peculiarities of which have led Mr. Parker to place them in a division by themselves of primary importance, whereas there is nothing more certain than that their differences from the Toucans and Capitonidæ are only just sufficient to separate them as a family from either. And yet among almost all other orders of birds the cranial structure is invaluable in the determination of their affinities.

The uniformity of the nomenclature and the absence of any laxity in the expression of the mutual relations of parts, greatly increases the facility with which the great number of facts brought forward by the authors can be grasped, and no doubt it is Mr. Bettany whom we have in great measure to thank for the general selection and classification of those which have been chosen to form "The Morphology of the Skull."

In conclusion we feel certain that all who read the work under consideration, the very nature of which makes it almost impossible for us to discuss the details with reference to any of the points which it brings forward, will realise how important an addition it is to biological science, and no thinking student will lay it down without recognising how much scope there is for still further investigation in the same field, especially in that direction which leads to the explanation of the reason why cartilages grow and bones form in certain definite directions and situations and in them alone; in other words, the next book of the kind required is one on the dynamics of the development of the skull.

THOMSON'S "SIZING OF COTTON GOODS"

The Sizing of Cotton Goods. By Wm. Thomson. (Manchester: Palmer and Howe.)

IN weaving cotton cloth it is necessary that the warp, which has to withstand a considerable strain in the process of manufacture, should be artificially strengthened by "sizing," that is, by dressing the thread with some adhesive material so as to enable it to resist the pulling and wearing action of the healds and shuttle. In the earlier days of cotton manufacture the weaver contented himself with the use of a mixture of flour-paste and tallow; the first ingredient gave the thread the desired extra strength, the second removed the harshness which the use of flour alone would have given. But the manufacturer soon discovered that by a judicious selection of the components of his "size," and by alterations in the mode of applying it, he could confer upon the cloth the appearance of being fuller and stouter than it actually was, judging from the amount of cotton contained in it. The great scarcity of the raw material during the cotton famine which sprung out of the American civil war had a powerful effect in developing the ingenuity of a certain set of manufacturers, and there is no doubt that their machinations have had a lasting influence upon the mode of manufacture of grey cloth. As the weight of a piece of calico is one of the chief elements in determining its value, attempts were quickly made to increase that weight by mixing such bodies as powdered heavy-spar, or, worse still, of deliquescent salts like the chlorides of magnesium and calcium, with the sizing material. Occasionally the manufacturer in thus attempting to palm off water or a worthless mineral in lieu of good cotton over-reached himself and a just retribution overtook him in the shape of heavy damages for mildewed or rotten goods.

The results of many of these attempts afford excellent illustrations of the proverbial danger of a little knowledge; the manufacturer somehow acquired the information that chloride of calcium, an almost worthless bye-product in many chemical operations, was an excellent absorbent of atmospheric moisture; its advantages as an ingredient of the sizing mixture were therefore obvious; unfortunately he knew nothing of *oidium oranteacum* or *puccinia graminis*, and had probably never heard of *pencilium glaucum*, or he might have known that he was preparing a mixture specially suited to the development of these fungi. Silicate of soda or water-glass doubtless appeared at first sight to be an excellent substance for dressing warp, but a painful experience was needed to teach some manufacturers that these alkaline silicates rapidly absorb carbonic acid, and that the resultant products, namely, free silica, and sodium carbonate, together occupying a larger volume than the original silicate, exerted a disruptive action upon the hollow cotton-fibre and made the cloth rotten and useless. Mr. Thomson does not altogether shirk the consideration of the moral aspects of the question of sizing; he makes no secret of the fact that the operation is often done with fraudulent intention. He expresses his opinion distinctly enough that the introduction of an undue amount of size into goods intended for the home trade can serve no useful purpose, but we think he will find it difficult to convince ordinary or unbiased people that a composition consisting, to the extent of half its weight, of a mixture of putrid flour, or British gum, China clay, barytes, or magnesium chloride, tallow, or palm-oil, with a sufficient amount of chloride of zinc or carbolic acid to prevent the whole from running into absolute nastiness, is a fit material to clothe even the patient Hindoo or the prudent Chinaman. Mr. Thomson, however, takes this business of sizing as a fact which, of course, cannot be ignored, and he tries to make the best of it. In the outset he shows that, as it now stands, the process is one of the clumsiest, most unscientific, and least understood of all the operations with which the manufacturer has to deal, and he points out, clearly and concisely, wherein it is faulty, and how it may be amended.

The book is, of course, designed primarily for the use of grey-cloth manufacturers, calico-printers, and generally of those whose business it is to buy and sell calico; and the subject is mainly treated from the point of view of a chemist perfectly familiar with the objects sought to be gained by legitimate sizing. In plain and albeit scientific language he describes the various pieces of apparatus employed in ascertaining the value of the different ingredients in size; he points out the qualities, good and bad, of the materials employed to give adhesive and softening qualities to the size; how the

size is to be applied to the yarn; to what diseases or modes of decomposition it is liable; and how it may be preserved from mildew or mischievous changes. The book has every right to be regarded as the only important treatise on the subject which has yet appeared, and, as such, we would recommend it to all who are interested in the production of one of our chief staples. T.

OUR BOOK SHELF

Physiological Tables for the Use of Students. Compiled by Edward B. Aveling, D.Sc., F.L.S. (London: Hamilton, Adams, and Co.)

WE are at a loss to find any excuse for the publication of these tables, which no one, we presume, would attempt to justify except on the plea that they may be useful in cramming students so as to pass the multifarious superficial examinations which are a blot upon our educational system.

They are unphilosophical in their plan, and altogether unreliable in their details. Some idea of the nature and value of the information which is here put up, as it were, into separate pigeon-holes for the use of the unwary, may be gathered from the following quotations. Nervous tissue, we are told, contains 15 per cent. of fats, thus classified:—

Fats, 15 per cent. in white, 5 per cent. in gray.	Oleo-phosphoric acid. Olein; margarin; palmitin. Cholesterin.

Would Dr. Aveling like to write a short essay upon oleo-phosphoric acid? Has he never heard of such bodies as glycerin-phosphoric acid and its derivative lecithin?

Or to quote from Table IV., where Dr. Aveling writes on the causes of the circulation:—

CAUSES OF CIRCULATION.
- Impulse of heart.
- Elasticity of arteries.
- Capillary Force. Proofs. ... ...
  1. Alterations in diameter of capillaries.
  2. Alterations of velocity of blood flowing through them
  3. Movement of blood after excision of heart in cold-blooded amimals.
  4. Emptying of arteries after death.
  5. Secretion after death.
  6. First movement of blood in embryo towards, not from, the heart.
  7. Fœtus without heart has organs developed.
  8. Degeneration of heart during life without much alteration in the circulation.
  9. Heart working well, and yet circulation through some part ceases.
  10. Asphyxia.
- Muscular pressure on veins.

Would it not be an admirable exercise to set the above lines to intending candidates in physiology and ask them to criticise them? Our readers will do so for themselves.

In the table referring to the sense organs we are confidently told that the nerve centres for the special sense of touch are the *thalami optici*, that the centres of the special sense of smell are the olfactory lobes, that the centres of sight are the corpora quadrigemina, the corpora geniculata, and the thalami optici.

But the above examples are more than sufficient to prove how dangerous a catalogue of mistakes Dr. Aveling has presented us with.

If science is to be used as a discipline in education, let it be fully and accurately taught; let us not imitate the old scholastic routine which forced unpalatable jargon in the form of "propria quæ maribus," &c., upon the unwilling student, and refuse to follow it in that which is its merit—its accuracy. A. G.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Indium in British Blendes

IT will be a matter of some interest to English mineralogists and chemists to know that certain blendes of Durham and, I believe, of Cumberland contain Indium in appreciable quantities. This fact has been made out by a very skilfully-conducted analysis by Dr. Flight in the laboratory attached to this department.

The work in the laboratory has, through the past two years, been almost exclusively devoted to the analysis of minerals selected from the division of the collection which is in process of being catalogued, and for which the crystallographic work has long been in progress.

When I gave the particular blendes in question to Dr. Flight for analysis, the grounds for their selection were that they were British, and that one of them in particular resembled certain foreign blendes which contain the rare metals found in association with this mineral.

The object of this letter is to secure a prompt announcement of Dr. Flight's having found Indium in the blende in question. He will in due time communicate further details of the analysis of the blende and of an elegant process by which he at once separates the Indium Sulphide from the blende.

NEVIL STORY MASKELYNE

Mineral Department, British Museum, October 30

The Radiometer and its Lessons

WILL you allow me to make a few remarks in reply to to Dr. Carpenter's letter on "The Radiometer and its Lessons," published in the last number of NATURE, and to try to show that I had good grounds for the opinion I expressed at the late meeting of the British Association in reference to his article on the same subject in the *Nineteenth Century*?

Nearly the whole of the first three columns of Dr. Carpenter's letter is devoted to proving that he "was not influenced, when writing on the radiometer, by any *animus* arising from [his] personal antagonism to Mr. Crookes on another subject." As I never in any way charged him with being thus influenced, I do not think that this part of his letter calls for any further remark on my part than an expression of my sincere regret that it should have been possible for him to think that I intended to make such a charge.

Dr. Carpenter devotes the rest of his letter to showing that he had "adequate justification" for "making it appear that Mr. Crookes had put a wrong interpretation on his own results," and thus proves very conclusively that I had "adequate justification" for supposing it possible that he may have intended to make this appear in his article in the *Nineteenth Century*.

In order to make out his "justification," Dr. Carpenter sets himself to prove (1) that Mr. Crookes puts forward the "direct impact of the waves" as affording "a definite interpretation" of the motion of the radiometer, and (2) that he claimed "the discovery of a 'new force' or 'a new mode of force.'"

With regard to the first of these points, I think that few persons can have read or heard Mr. Crookes's accounts of his investigations without having observed how careful he was to reserve his judgment as to the cause of the remarkable effects he had discovered, and neither to give out as conclusive any explanation of his own, nor to adopt any of those suggested by others until, chiefly through his own further experiments, one of them had been shown to rest on sufficient evidence. It is true that on one occasion he uses the following words (quoted by Dr. Carpenter):—"My own impression is that the repulsion accompanying radiation is directly due to the impact of the waves on the surface of the moving mass, and not secondarily through the intervention of air-currents, electricity, or evaporation and condensation," and that, in several places in his earlier papers, he shows a leaning towards the same hypothesis; but this is a very different thing from having adopted this view as a "definite interpretation" of the phenomena. Even Dr. Carpenter does not attempt to show that Mr. Crookes ever, in so many words, committed himself to this theory, but concludes that he held it

from considerations which, for fear of misrepresentation, I must give in Dr. Carpenter's own words :—

"After pointing out that 'there is no real difference between heat and light, all we can take account of [I presume he means physically, not physiologically] being difference of wave-length,' he [Mr. Crookes] thus continues : 'Take, for instance, a ray of definite refrangibility in the red. Falling on a thermometer it shows the action of *heat;* on a thermopile it produces an *electric current;* to the eye it appears as *light* and *colour;* on a photographic plate it causes *chemical action;* and on the suspended pith *it causes motion.*' Now (1) this motion being elsewhere spoken of as due to *the impetus given by a ray of light,* (2) a set of experiments being made to determine *the mechanical values of the different colours of the spectrum,* (3) an observation being recorded on *the weight of sunlight* (without the least intimation that he was 'speaking figuratively' as Mr. Crookes says that he did to his audience at the Royal Institution), (4) the term *light-mill* being used by himself as a synonym for 'radiometer,' and (5) no hint whatever being given of the dependence of the result (as argued by Prof. Osborne Reynolds) on a 'heat-reaction' through the residual vapour, I still hold myself fully justified in attributing to Mr. Crookes the doctrine of the *direct mechanical action of light.*"

Taking these points in order and using Dr. Carpenter's numbers for reference, I may observe as to (1) that this seems to refer to Mr. Crookes's statement of an "impression" in a passage already quoted; with regard to (2) that Mr. Crookes having found that "every ray from the ultra-red to the ultra-violet" produced a mechanical effect under the circumstances of his experiments, it was very natural that he should hope to get some clue as to the nature of the action by finding what rays produced the greatest effect; of Dr. Carpenter's arguments (3), (4), and (5), it is difficult to speak with the seriousness befitting their author's many valuable services to the cause of science, and the "due consideration of . . . *his* and *my* relative positions." To conclude that Mr. Crookes must have held a particular theory from the fact that, when he had constructed an apparatus which spun round on exposure to light, he called it a "Light-mill;" from his having neglected to give warning that he was "speaking figuratively" when he talked of "weighing a beam of sun-light," or from his having given no hint that he had adopted a rival theory, is certainly not to exemplify the "strict reasoning based on exact observation" which Dr. Carpenter recommends in the paragraph with which he concludes both his article and his letter to this Journal.

A few sentences before the passage I have quoted, Dr. Carpenter refers to the "whole phraseology" of Mr. Crookes's papers of January 5 and February 5, 1876, as indicating "that he *then* considered [the motion of the radiometer] as directly due to the impact of the waves upon the surface of the moving mass." This again seems to me a very unsound conclusion. The effect to the elucidation of which these papers were devoted was unquestionably due to the incident radiation, but whether as a primary or as a secondary effect, was still a matter for discussion. In my opinion the phraseology used in them implies no more than this : it indicates a relation of cause and effect, but, for the most part, leaves the question as to *how* the latter follows from the former, entirely untouched. If, however, Dr. Carpenter will refer to § 195 of the paper of February 5, as it is printed in the *Phil. Trans.* for 1876, he will see that Mr. Crookes did not *then* attribute the motion to *direct impact of the rays upon the surface* of the moving body, but rather to an elevation of its temperature, and a consequently increased *radiation of heat from its surface.* At the same time he will see that this suggestion is put forward in a tentative and entirely undogmatic way.

Dr. Carpenter next undertakes to show that Mr. Crookes laid claim to the discovery of a "new force" or a "new mode of force," finding his proof of this in a passage included in the quotation from his letter that I have given above. Commenting on this passage in the *Nineteenth Century* (p. 248), he says : "To the *three* attributes of radiation universally recognised by physicists, Mr. Crookes proposes (in the passage already cited) to add a *fourth,* the power of producing an electric current in a thermopile; and a *fifth,* the power of producing mechanical motion when acting on light bodies freely suspended in a vacuum." Again, if Dr. Carpenter had consulted the *Philosophical Transactions* for 1876 (p. 361), he might have done Mr. Crookes more justice and might have given him credit for the discovery of a *sixth* attribute of radiation—(Mr. Crookes there mentions one more effect which the same ray can produce : "concentrate it on the hand by a lens, it raises a blister accompanied with pain"),—and, if he had read a few lines further, he might have spared himself the trouble of explaining to Mr. Crookes that the electric current of a thermopile is not directly excited by the incident radiation, for he would have found that this action, in common with the pain and the blister and the motion of the mercury in a thermometer, is there spoken of as being an effect of *heat.* I think it must be evident to any one who will read this passage attentively with its context (either in *Proc. Roy. Soc.* [February 10, 1876], from which apparently Dr. Carpenter quotes, or in the *Phil. Trans., loc. cit.*), that it has nothing at all to do with either one or more new forces, but that the whole gist of it is to assert that, whatever may be the mode in which radiation produces mechanical force, the result is to be attributed to it *as a whole* and not to a particular constituent assumed for the purpose.

As though with the object of covering a retreat, Dr. Carpenter says, near the end of his letter, that "Prof. G. Carey Foster will doubtless be able to pick out *points of detail* in my article, as to which faults may be found by a severe critic." I may therefore point out that I have so far carefully confined myself to what he himself singles out as the "main issues" of the question between us, and that, in my further remarks, I shall treat the matter from a still more general point of view.

In speaking (in my address at Plymouth) of the "tendency" of Dr. Carpenter's article, I meant to indicate that I referred in what I said about it to what seemed to me to be its general drift and tone, rather than to any particular passage or passages. And my judgment of the drift of the article was formed not only from what I found in it, but also from what I did not find there. For example, if Dr. Carpenter had thought as highly as I do of Mr. Crookes's work he would almost inevitably have pointed out more emphatically than he did the really astonishing number, variety, and laboriousness of his experiments ; he would also, I think, have pointed out that (with the important exception of Dr. Schuster) scarcely one of the numerous investigators, who, in consequence of his researches, have occupied themselves more or less with the radiometer, had obtained any significant experimental result which Mr. Crookes himself had not anticipated ; and he would have shown that the discovery of the radiometer, while affording a remarkable illustration of the importance of following up unexplained though apparently trivial phenomena, illustrates no less forcibly the truth that scientific discoveries are not chance revelations, coming now to one and now to another, but that they are made only by those who have eyes to see a clue when it is offered them, and patience and skill to follow where it leads.

Turning to what the article did contain, I think it is not incorrect to say that it tended to produce the impression that Mr. Crookes, more or less obstinately, and on insufficient grounds, rejected a satisfactory explanation of his results. I will therefore try to state, as shortly as I can, what seems to me to be the true state of the case in relation to this point.

Prof. Reynolds (in his paper read before the Royal Society on June 18, 1874) undoubtedly showed that a mechanical reaction, such as might account for the results obtained by Mr. Crookes, might arise when heat is communicated from a solid surface to a vapour or gas, but he did not (then at least) show that in Mr. Crookes's vacua there was enough residual gas to produce the results he ascribed to it. Mr. Crookes, without disputing the possibility of the action pointed out by Prof. Reynolds, made experiments from which he concluded that it was insufficient to explain the movements he had observed. (I must here remark that Mr. Crookes did not say, as Dr. Carpenter asserts that he did, that the explanation offered by Prof. Reynolds was one that "it is impossible to conceive." His words were : "It is impossible to conceive that in *these* experiments sufficient condensable gas or vapour was present to produce the effects Prof. Osborne Reynolds ascribes to it. After the repeated heating to redness at the highest attainable exhaustion, it is difficult to imagine that sufficient vapour or gas should condense on the movable index to be instantly driven off by a ray of light, or even the warmth of the finger, with recoil enough to drive backwards a heavy piece of metal."—*Phil. Trans.*, 1875, p. 547. But although Prof. Reynolds is unquestionably entitled to the credit of having originated the fundamental idea and worked out many of the details of the explanation that seems now to be generally adopted, his explanation not only rested on a somewhat slender experimental basis, but was theoretically incomplete, and in particular it did not show clearly why so high a degree of rarefaction should be needed for the production of the phenomena in question. An important step towards supplying this deficiency

was taken by Profs. Tait and Dewar (July, 1875), who showed how the increase, resulting from rarefaction, in the mean length of the path of the gaseous molecules would favour the action, but the explanation in the form which they gave to it required that the rarefaction should be carried far enough to make the mean length of path of a molecule of gas great as compared with the dimensions of the inclosing vessel. It has, however, been pointed out by Prof. Zöllner (*Pogg. Ann.*, February, 1877), and more recently by Mr. Tolver Preston (*Phil. Mag.*, August, 1877), that, in the majority of cases, this condition is far from being fulfilled. On the other hand, the residual-gas theory of the action of the radiometer received very important experimental support from Dr. Schuster's beautiful demonstration (February, 1876) that the force exerted on the discs was correlative with an equal opposite force exerted upon the glass envelope. The complete proof that the action was due in some way to the presence of residual gas was furnished by Mr. Crookes's own discovery (June, 1876) that it rapidly diminishes when the exhaustion is carried beyond a certain point depending on the nature of the gas. The outstanding defect in the theory was removed by Mr. Johnstone Stoney, who (*Phil. Mag.*, March and April, 1876) showed that the observed phenomena might arise at a degree of rarefaction at which the mean length of path of a molecule was still much below the distance from the discs to the envelope, it being sufficient that this distance should not be too great to allow the warming of the discs to cause a sensible increase in the velocity with which the molecules struck the glass. Mr. Stoney's form of the theory answers to all the facts of the case, so far as I am acquainted with them, and it has been confirmed and illustrated by Mr. Crookes with a numerous series of remarkably beautiful and ingenious experiments.

My object in thus tracing the chief stages in the growth of the accepted theoretical explanation of the radiometer has been to point out that the quality of mind which led Mr. Crookes to reject the various suggested explanations of the phenomena he had observed, so long as they were only approximate and did not account for *all* his facts, was merely a further exemplification of the quality which led him to the original discovery. If he had been content to disregard a seemingly trivial fact he would never have made this discovery at all, and if he had disregarded slight defects in the explanations that were offered he would have missed some of its most important consequences. I think that this also might have been suitably included among the "Lessons of the Radiometer." G. Carey Foster

University College, London, October 27

Has Dr. Carpenter allowed himself to become possessed by a "dominant idea?" From his letter in Nature (vol. xvi. p. 544), I infer that he *might* have taken the trouble to reply to my article in the July number of the *Nineteenth Century*, had he not thought that my assertions "were well known in the scientific world to be inconsistent with fact."

Some remarks, however, made by Prof. G. Carey Foster at the British Association seem to have forced upon Dr. Carpenter the conviction that he may have underrated my character for veracity, and that the "scientific world," at all events, is not unanimous in regarding my "assertions" as falsehoods. Dr. Carpenter therefore seeks in your columns to justify the statements contained in his article on "The Radiometer and its Lessons," in the *Nineteenth Century* for April last.

When Dr. Carpenter declares my "assertions (1) . . . (2) . . . (3)" to be false, I have a right to demand that Dr. Carpenter give my identical words, and not his own interpretation of my words—an interpretation which is "inconsistent with fact."

To show Dr. Carpenter's inaccuracies in small things as well as great, I may point out that he does not even quote correctly the title of my article in the *Nineteenth Century*. His carelessness in more important matters is of deeper consequence. In order to enforce one of his dominant ideas "yet more fully and emphatically," he tells us that he applied himself to a "careful reperusal of" my papers "with the most earnest desire to present a true history of the whole inquiry." A most laudable determination! And where, will it be believed, did Dr. Carpenter, a Fellow of the Royal Society, go for information? To the *Philosophical Transactions*, where my papers are printed at full length? No! He only referred to the "*Proceedings* of thd Royal Society," a record, as every one knows, that contains brief, and therefore imperfect abstracts of what is published in full in the *Transactions*.

In his "justification" Dr. Carpenter quotes a passage from a lecture I delivered in 1874, on The Repulsion Accompanying Radiation, commencing, "my own impression is," &c. Had Dr. Carpenter quoted the next paragraph, which is necessary to a correct interpretation of the sentence he did quote, your readers would have been enabled to judge how far I advanced theories of my own. My words were these: "I do not wish to insist upon any theory of my own. . . . The one I advance is, to my mind, the most reasonable, and, as such, is useful as a working hypothesis, if the mind must have a theory to rest upon. *Any* theory will account for *some* facts, but only the true explanation will satisfy *all* the conditions of the problem, and this cannot be said of either of the theories I have already discussed." My next paragraph concludes with the following quotation from Sir Humphry Davy:—"When I consider the variety of theories which may be formed on the slender foundation of one or two facts, I am convinced that it is the business of the true philosopher to avoid them altogether. It is more laborious to accumulate facts than to reason concerning them; but one good experiment is of more value than the ingenuity of a brain like Newton's."

With regard to my having "theorised on the subject," I have never denied having done so, although I have on five or six occasions specially stated that "I wished to keep free from theories," and "unfettered by the hasty adoption" of theories. But I do deny that I ever stated that my results were definitely explained by the direct mechanical action of light. Your readers will understand that an experimental research is necessarily and slowly progressive, and that the early provisional hypotheses have to be modified, and perhaps altogether abandoned, in deference to later observations. Until my experiments confirmed the explanation given by Mr. Johnstone Stoney, I adopted no definite theory, and I contend that a trained physicist would fail to gather from my published papers that I desired my first impressions to be regarded as final.

Dr. Carpenter again attributes to me the terms "a new force," or a "new mode of force," as applied to the repulsion accompanying radiation. Unless Dr. Carpenter can point these words out in my published papers, he has no right to place them between inverted commas.

But the chief burden of Dr. Carpenter's song is that "Mr Crookes has another side to his mind, which makes Mr. Crookes the spiritualist almost a different person from Mr. Crookes the physicist." I fail to see how the investigation of certain phenomena called spiritual can make a man a spiritualist, even if he comes to the conclusion that some of the phenomena are not due to fraud. My position in this matter was clearly stated some years ago, and I ask your permission to quote the following passages from an article I published in 1871:—"I have desired to examine the phenomena from a point of view as strictly physical as their nature will permit. . . . I wish to be considered in the position of an electrician at Valentia examining, by means of appropriate testing instruments, certain electrical currents and pulsations passing through the Atlantic cable; independently of their causation, and ignoring whether these phenomena are produced by imperfections in the testing instruments themselves, whether by earth currents or by faults in the insulation, or whether they are produced by an intelligent operator at the other end of the line."

From this stand-point I have never deviated. Can Dr. Carpenter say that his position and mine, in respect to the investigation of the phenomena ascribed to spiritualism, are so very different? He asserts that he has shown beyond doubt that it is all imposture. But I would ask if this was proved to his satisfaction twenty years ago, why does he still waste valuable time in interviews and sittings with so-called mediums? If I am to be censured for having devoted time to this subject, such censure must be doubly applicable to a man who commenced the investigation when I was a child, and who cannot let the subject drop whenever a new "medium" comes in his way. Does he regard the subject as his own special preserve, and may his demonstrations against other explorers in this domain of mystery be looked upon as the conduct of a gamekeeper towards a suspected poacher?

To impress on the world that he has no "*animus*," Dr. Carpenter says he "cordially" and "personally congratulated" me. His words bring vividly to my mind the conversation, of which, by the by, he has omitted an important part. It was at the annual dinner of the Fellows of the Royal Society on November 30, 1875, when the royal medal was awarded to me. Dr. Carpenter accosted me with great apparent cordiality, and said,

"Let us bury the hatchet! Why should scientific men quarrel?" I signified my full acceptance of the offered peace, and great was my surprise soon after to find that, unmindful of the understood compact, he had exhumed his hatchet and was dealing me unexpected and wanton strokes, tempered by a certain amount of half praise which reminds me of the sort of caressing remonstrance of Majendie in the pre-anæsthetic days, to the dog which he had on his operating table—"*Taisez vous, pauvre bête!*"

In all seriousness, however, I must again ask, what is the meaning of the "personal antagonism," and the persistent attacks which Dr. Carpenter, for the last six years, has directed against me? In his recently published book, in the *Nineteenth Century*, and in his last letter to you, the key-note struck in the *Quarterly Review* six years ago is sustained. We have the same personalities, the same somewhat stale remark about my double nature, and the same exuberance of that most dangerous and misleading class of averments, half truths. Dr. Carpenter, indeed, condescends to admit that I have pursued "with rare ability and acuteness a delicate physical investigation in which nothing is taken for granted without proof satisfactory to others as well as to himself," and that I have "carried out a beautiful inquiry in a manner and spirit worthy of all admiration;" but, after granting so much, he dissembles his love and proceeds to "kick me down stairs." I am damned with faint praise, and put to rights in such a school-masterly style, that I could almost fancy Dr. Carpenter carries a birch rod concealed in his coat-sleeve. He admits that in an humble and subordinate sphere I have done useful work, only I must not give myself airs on that account. Dr. Carpenter reminds me of Dr. Johnson defending Sir John Hawkins, when he was accused of meanness. "I really believe him," said Johnson, "to be an honest man at the bottom; but to be sure he is penurious, and he is mean, and it must be owned he has a degree of brutality, and a tendency to savageness, that cannot easily be defended." In the same magnanimous spirit Dr. Carpenter allows that I have contributed a trifle to science, but he does not forget to add that I am the victim of cerebral duplicity, and I am again held up to illustrate the sad result of neglecting to train and discipline "the *whole* mind during the period of its development," &c.

I have, it appears, two allotropic personalities, which I may designate, in chemical language, Ortho-Crookes and Pseudo-Crookes. The Ortho-Crookes, according to Dr. Carpenter, has acquired "deserved distinction as a chemist." He carries out a "beautiful inquiry in a manner and spirit worthy of all admiration." He has shown "ability, skill, perseverance, and freedom from prepossession." He pursues "with rare ability and astuteness a delicate physical investigation." He evinces the "spirit of the true philosopher," and he has "deservedly" received "from the Royal Society the award of one of its chief distinctions."

But Pseudo-Crookes, whose career Dr. Carpenter has evidently watched almost from his cradle—as he professes to know the details of his early education—unfortunately took a "thoroughly unscientific course," and developed into a "specialist of specialists." He had "very limited opportunities" and "never had the privilege of associating" with scientific men, although he displayed "*malus animus*" "towards those with whom he claims to be in fraternity." He is "totally destitute of any knowledge of chemical philosophy, and utterly untrustworthy as to any inquiry" not technical. His "assertions" are "well known in the scientific world to be inconsistent with fact." He enters on inquiries "with an avowed foregone conclusion of his own." He has "lent himself to the support of wicked frauds." He has "prepossessions upon which clever cheats play." His "scientific tests" are not "worthy of trust." He is a believer in "day dreams," and the supporter of a "seething mass of folly and imposture;" whilst, to crown all, he actually thinks that the radiometer is driven "by the direct impetus of light." In short, this Pseudo-Crookes is a compound of folly and knavery such as has rarely, if ever, previously been encountered.

William Crookes (The Ortho-Crookes?)

London, October 29

Mr. Wallace and Reichenbach's Odyle

I am amazed that Dr. Carpenter should think it necessary to make public, with such haste, Prof. Hoffmann's statement that Baron Reichenbach's facts and theories are not accepted by the body of scientific men in Germany. Of course they are not. But how this affects their intrinsic accuracy I fail to see. Less than twenty years ago the scientific men of all Europe utterly disbelieved in the co-existence of man with extinct animals; yet the facts adduced by Freere, Boué, McEnery, Godwin Austen, Vivian, and Boucher de Perthes, are now admitted to have been trustworthy and deserving of the most careful examination. The whole history of scientific discovery from Galvani and Harvey to Jenner and Franklin, teaches us, that every great advance in science has been rejected by the scientific men of the period, with an amount of scepticism and bitterness directly proportioned to the novelty and importance of the new ideas suggested and the extent to which they run counter to received and cherished theories. Rejection is one thing, disproof is another; and I have in vain searched for anything like disproof, or even rational explanation, of Reichenbach's facts: his theory, or "Odyle-doctrine," I have never "attempted to rehabilitate," as Dr. Carpenter, with his usual misconception, says I have done. In my review of Dr. Carpenter's lectures (*Quarterly Journal of Science*, July, 1877, p. 396), I adduce five tests employed by Reichenbach, and also the independent and simultaneous confirmation of Dr. Charpignon in France; and the only reply I get is: "All men of science disbelieve them." With the facts of history above alluded to in my mind, and believing that human nature is very much the same in the nineteenth century as it was in the eighteenth, I can only say, "so much the worse for the men of science."

Dr. Carpenter's reference to the believers in a flat earth, as a parallel case, is unfortunate, because the two cases are really of a totally different nature. Those who maintain the earth to be flat do not deny the main facts which we rely on as proving it to be round, but they attempt to give other explanations of them. The dispute is on a question of reason and inference; and every intelligent and fairly educated man is able to decide it for himself. But in Reichenbach's case it is the *facts* that are rejected without disproof or adequate explanation. The two cases are therefore quite distinct, and Dr. Carpenter's attempted parallel, as well as his setting up of scientific disbelief as a conclusive reply to evidence, is in conformity with his whole treatment of this subject.

I trust that such of the readers of Nature as may feel any interest in the questions at issue between Dr. Carpenter and myself will read my article above referred to, and not allow themselves to be influenced by Dr. C.'s repeated appeals to authority and to prejudice.

Alfred R. Wallace

I have to request your insertion of a post-card I have this morning received, for two reasons; *first*, because, as it is anonymous, and as the writer of it is obviously a reader of Nature, no other way is open to me for replying to it except that which your columns may afford; and *secondly*, because it is a very curious example of the misconceptions into which men are apt to fall who allow themselves to become "possessed" by "dominant ideas."

"If Mr. A. R. Wallace has to choose between being either 'a fool or a knave,' there is *at all* events *no choice* left for *the man* who deliberately and maliciously makes incorrect assertions and suppresses the truth to further his own views. I dare say you know what most people would call such a man. Yours,

"One who was at Plymouth"

Now, in the first case, it must be perfectly obvious to any one who is capable of reasoning logically, that nothing which I said of Mr. Wallace in your last number can be twisted into the implication that he is either "a fool or a knave." John Hampden is continually saying this of Mr. Wallace and of everybody who upholds the rotundity of the earth. And I mildly suggested whether, in putting himself in opposition to the whole aggregate of scientific opinion on the value of Reichenbach's Odylism—not because he had himself repeated them, but because he believes in Reichenbach—Mr. Wallace is not assuming an attitude in some degree similar, that is, setting himself up as the one wise and honest man who duly appreciates Reichenbach, and therefore implying that everybody else is either stupidly or wilfully blind to the evidence he presented. If anyone thinks it worth while to read Mr. Wallace's review of my lectures on "Mesmerism, Spiritualism," &c., in the last number of the *Quarterly Journal of Science*, he will be able to judge whether I have or have not wronged Mr. Wallace in this matter.

The writer's appreciation of my own character, which has fre-

quently been expressed to me before in the same manner and in the like terse and elegant language, is now enforced by what he deems to be Prof. Carey Foster's judicial opinion, delivered at the Plymouth meeting; and I find myself, therefore, fully justified in my opinion that by his introduction of the word "intentionally" Prof. Carey Foster made his judgment legitimately bear a meaning, which, as he has stated, he would consider insulting to my character. And I cannot but believe that Prof. G. Carey Foster will regret having thus given a new handle to a man who obviously *wishes* to insult me on account of my antagonism to spiritualism. As the writer of the post-card continues to use Prof. G. C. Foster's authority, *after* that gentleman's explicit disavowal of the offensive meaning here attached to it, and as I may, of course, expect that he will continue to avail himself of that authority, I should like him to know through your columns that it is scarcely worth while for him to trouble himself to repeat these attacks, since they have long since ceased to do anything else than amuse me, and will only furnish me with materials for amusing other people.

It seems much to be regretted that neither spiritualism nor attendance at the meetings of the British Association, nor even the reading of NATURE seems able to teach this person to behave like a gentleman. WILLIAM B. CARPENTER

October 29

Potential Energy

YOUR correspondent "X." has described some of his troubles respecting potential energy. Many a learner could describe similar troubles respecting force and energy in general. They who earnestly contend for definiteness and accuracy do not always teach with definiteness and accuracy. For example: in his "Treatise on Heat," p. 137, Dr. Tyndall tells me that by raising a weight from the floor I have conferred upon the weight potential energy. Presently he tells me that this energy is derived (not from me, but) from the pull of gravity. He next tells me that we might call the energy with which the weight descends, moving force, *i.e.* he teaches me to confuse force and energy; and after all this he bids me remember that "exactness is here essential. We must not now tolerate vagueness in our conceptions."

Take another example. In his lecture on "Force" (NATURE, vol. xiv. p. 462), Prof. Tait teaches that force is a mere name, and that it has no objective existence; he also teaches that the product of this non-existence by its displacement has an objective existence. Few learners would say that is a very lucid statement. Again, in the same lecture he says "there is no such thing as centrifugal force, and accelerating force is not a physical idea at all;" but in his "Nat. Phil." he speaks of both these forces, and describes their effects (Nos. 185, 187, 598, 248).

When teachers deservedly eminent make statements like the foregoing, so likely to mystify and confuse a novice, it is no wonder that there is a good deal of smattering in popular science.

Prof. Tait says "the so-called accelerating force is really no force at all, but another name for the kinematical quantity acceleration." I venture to entirely disagree with this statement, and for the following reason:—$\frac{dv}{dt}$ is a *number*, and may be that number of units of *force*, or that number of units of *acceleration*. When it is called accelerating force it is the representative of $m\frac{dv}{dt}$, when $m = 1$, and m does not appear in the expression; and it means $\frac{dv}{dt}$ units of *force*. When it is called acceleration it means $\frac{dv}{dt}$ units of *acceleration*. Accelerating force is just as real as moving force, for it is, in fact, the mth part of the moving force. In like manner v may mean either v units of velocity, or v units of momentum; in the latter case it is the representative of mv, when $m = 1$, and means the momentum of a unit of mass which has v units of velocity. In like manner m may mean either m units of mass, or m units of momentum, or m units of kinetic energy; in the two latter cases it is the representative of mv or of mv^2 when $v = 1$, and means the momentum, or the *vis viva* of m units of mass moving with unit of velocity.

A few simple definitions would remove the difficulties respecting force. Thus: If a mass of m units of mass is at any instant receiving an acceleration of a units of acceleration in any given direction, the force which is acting on it at the given instant in the given direction is ma units of force. The force acting on the mass in the *direction of its motion* is called the moving force. The force in the normal to the direction of its motion and towards the centre of curvature is called the centripetal force. An equal and opposite force is called the centrifugal force. The mth part of the moving force is called the accelerating force, which is the moving force acting on a unit of mass.

In the case of a planet's orbit it is too common to give the name centrifugal force to two forces which generally differ both in magnitude and in direction, one of them being in the direction of the normal, the other in the direction of the radius-vector. This is the last instance which I shall give of sins against definiteness and accuracy. E. G.

Bardsea

Hartlaub's "Birds of Madagascar"

THE excellent review, exhibiting traces of a master's hand, of the above-named useful work, which appeared in NATURE (vol. xvi. p. 498) prompts me to offer some remarks on the ornithology of Madagascar and its neighbouring islands, and to take exception on two points therein laid down.

The first of these is propounded by your reviewer and seems to me absolutely contrary to fact. He says:—"Compared with Madagascar itself the appendent island groups are poor in species, although in every case there are many interesting forms among their winged inhabitants. The Comoro Islands muster only some forty-four species of birds, Mauritius about sixty, of which fifteen or sixteen have been introduced by man's agency, and Bourbon about the same number, while Rodriguez appears to have only about twenty-five species now existing in it, of which four or five are certainly recent introductions."

Now twenty years ago my friend, Mr. Sclater, in that remarkable paper of his on the geographical distribution of birds (*Journ. Linn. Soc.* Zoology, ii. p. 130), which so happily laid the true foundation for our present researches into the subject, showed that the proper mode of comparing the wealth or poverty of one fauna with another was to state the proportion which the number of species composing it bears to the area over which they range. The same view was adopted very shortly after by Mr. Wallace, who took occasion (*Ibis*, 1859, p. 449) to question certain of Mr. Sclater's results, and its correctness seems to have been since generally admitted. Yet, applying this test to Madagascar and its neighbouring islands, we find a state of things to exist very different from that which your reviewer has alleged. The area of Madagascar is said[1] to be 10,751 German square miles, that of the Comoros collectively 38·57, of Mauritius 34·76, of Bourbon 42·05, and of Rodriguez 5. It will be sufficient for my purpose to compare the first and last of these. Your reviewer is willing to allow twenty indigenous species to Rodriguez; then—

Area of Rodriguez.		Area of Madagascar.		Species in Rodriguez.		Species in Madagascar.
5	:	10,751	: :	20	:	x

$$x = \frac{10{,}751 \times 20}{5} = 43{,}004.$$

But instead of an avifauna of 43,004 species, or about four times the number known to exist throughout the whole world, Dr. Hartlaub gives it 218, and your reviewer generously adds two more, making 220! Suppose (an extravagant supposition) that future explorations enable us to double the last number, it is Madagascar that will still be out of all proportion "poor in species" compared with "the appendent island groups," and not these with Madagascar.

The next point to which I must demur is that "the individuality of the fauna of Madagascar is so unique that even that of New Zealand can hardly be compared with it." I will leave to fitter hands than mine to show that this is not the case generally, and shall only remark here that it is not so with birds. Of the sub-class *Ratitæ* there have been until lately *five* strongly-marked groups, each of which is equivalent to an "order" among the *Carinatæ*. Now *two* of these groups were peculiar to New Zealand, and one (*Apterygidæ*) is so now, while the other (containing the families *Dinornithidæ* and *Palapterygidæ*) is but recently extinct. Willingly granting that *Æpyornis*, when we

[1] Behm und Wagner, "Areal und Bevölkerung der Erde" (Petermann's *Geogr. Mittheilungen*, Ergänzungsheft, November 20, 1876).

know more about it, may prove to form a sixth group, the balance of "individuality," if I understand the meaning of the word, will still be on the side of New Zealand. Turning to the Carinate birds, *Harpagornis* stands alone, while *Cnemiornis* will certainly count for as much as the *Dididæ*. The extraordinary Mascarene Rails (*Miserythrus* and *Aphanapteryx*) are well represented by *Ocydromus*, which so much resembles them, and *Strigops* is undoubtedly a more abnormal form than, so far as we can judge, either *Lophopsittacus* or *Necropsittacus*; just as *Nestor* is more aberrant than *Coracopsis*, and *Heterolocha* than either *Fregilupus* or *Necropsar*. But there is no need to continue the list, and in conclusion I will only declare that I think far too highly of the fauna of Madagascar and of the Mascarene Islands to wish that its extraordinary peculiarities should be undervalued, though I do not want them to be unduly magnified at the expense of those of the fauna of New Zealand.

ALFRED NEWTON

Magdalene College, Cambridge, October 27

Eucalyptus

HAVING read with great interest the article in your journal (vol. xvi. p. 443) on the *Eucalyptus* I take the liberty of sending you a pamphlet on the same subject, in which I have endeavoured to unite all the arguments likely to persuade and convince the Italians of the immense utility of the above-named tree, the cultivation of which would be of the greatest importance for the *Agro Romano*.

As is well remarked in the article in NATURE, the *Eucalyptus* is extensively cultivated in France, Spain, and Portugal. But in Italy, where it prospers almost all over the country and might be cultivated with facility, in spite of the most earnest efforts on my part during my residence here for the last ten years, in spite of its being recommended in Parliament by one of the most influential members, it has not been adopted.

In my gardens on the Lake Maggiore, I cultivate forty different varieties of the *Eucalyptus*. Of these the *amygdalina* and the *globulus* have attained, in eight years, the height of 17 metres. It is to be remembered that the temperature has sometimes been as low as 7° C. below zero without injury to these plants.

If you consider it probable that these few words could be of interest to your readers I willing authorise you to publish them in your estimable journal. PRINCE PIERRE TROUBITZKOY

Villa Troubitzkoy, near Intra, Lago Maggiore, October 15

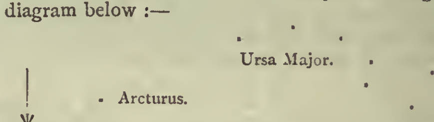
THESE trees are now attracting so much attention that even the small amount of experience I may be able to offer may not be unacceptable to your readers. Considerable stress is laid upon their influence in dissipating malaria; but I have not found this to be the case in Queensland, one of the head-quarters of the tree. I have personally suffered from malaria in the very heart of a forest extending for many miles in every direction, and composed mainly of all the varieties of *Eucalyptus*, and not by any means remarkable for the extent of swampy ground, and have known many instances of febrile attacks among shepherds and stockmen in the locality. Moreover I was told on inquiry that these attacks were not confined to any particular year, but that every year some cases might be expected. I was greatly surprised at reading in your "Notes" (NATURE, vol. xvi. p. 557) that the mosquitoes had disappeared with the introduction of the "gum" trees into Algeria. This would not be the experience of any one who has lived in Australia, I believe. I have found these pests so intolerable on high land, where almost the only tree to be found was one variety or other of *Eucalyptus*, and sometimes all, that sleep was impossible while camping out at night, and life a burden in the day by reason of these pests. The gums emit a most decided odour, especially in strong sunlight. When riding across the great Queensland plains and approaching wooded spurs I have (*Scotticé*) "felt" the characteristic smell of the gums at a considerable distance. These plains—ten miles in breadth—are not crossed in a short time, and the resinous odour of the gums, omnipresent in the forest and scarcely noticed there, strikes one forcibly when approaching the trees after the olfactory organs have been for some time deprived of it. Whether this odour has any effect or whether it is *the* preservative against malaria, I do not know. The growth of these trees in South America is very rapid. When in the Banda Oriental some years ago I examined a plantation of red and blue gums, then eight years old. The trees were at least forty feet high, and many of them measured thirty-six inches in circumference at three feet from the ground. They had a profusion of foliage such as I have never seen on the same trees in Australia. This was right out on "pampa" land, in deep alluvial soil. These trees had fought their way up, in spite of the black ants so destructive to foliage—the owner told me that they had at first stripped the young trees—and the tremendous gales which sweep over this open country. Those to the westward and south-westward of the plantation were far inferior in size to those on the east and north. This was the only grove of *Eucalypti* in the Banda, and it demonstrates the possibility of covering the naked pampas to any extent with forest. English settlers in the River Plate countries should note this fact, and I am sure the enlightened owner of the Estancia "Sherenden" would supply any of his countrymen with seed.

ARTHUR NICOLS

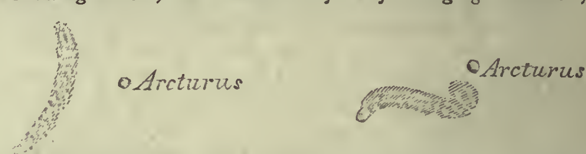
Meteor of October 19, 6.15 P.M.

THE large meteor described by two correspondents (NATURE, vol. xvi. p. 551) was observed also by several persons in this district, but most of the accounts are so meagre and doubtful as to possess little scientific value. The meteor appears, however, to have been well seen by Mr. W. Watkins Old, of The Parade, Monmouth, and his notes are so interesting that I beg to transcribe them. He says:—

"The meteor fell at 6.15 exactly. It appeared to me to descend perpendicularly some degrees from and to the west of Arcturus (which was shining brightly), and it disappeared behind a bank of dark cloud above the horizon at a point in a line projected beyond Arcturus, half the distance between that star and the last of those in the tail of Ursa Major, as roughly shown in the diagram below:—



Thus it remained stationary, like a dazzling white wand, while I counted twenty, during which time I could perceive the vapour, of which the trail was composed, as it were in ebullition. It then gradually curved towards the north as depicted in the following sketch; and drifted slowly away during *eight* minutes,



until it lay almost horizontal though still brightly illuminated, while the clouds gradually rose and covered it from my view. Altogether I observed it over eight minutes by my watch. There was much twilight in the west and the moon was shining brightly from which one may judge the extreme brilliancy of the meteor. I should add that when it appeared there was simultaneously a sensible rent or flip, like one sometimes hears with a sharp flash of lightning, and which may possibly be due to the appulse of light, as it could scarcely be the sound of explosion if there was any. It was too simultaneous to be the report of the descent of the meteor through the air, but it was sufficiently loud to be pronounced and caused some people standing near me, with their backs to the west, to inquire what it was, though they evidently saw nothing of the meteor nor even turned towards its direction. I listened but heard no further sound."

Ashleydown, Bristol, October 26 W. F. DENNING

Curious Phenomenon during the Late Gale

YOUR correspondent, "G. A. M." (vol. xvi. p. 551), may be interested to know that the "ball of fire" he saw descend on the evening of the 14th inst. was seen here by me, and by those who accompanied me, at precisely the same time (6.50 P.M.) that he mentions. We were walking in a south-easterly direction, and it seemed to fall from about half-way between that point of the compass and the moon, which was due south of us, and shining brightly. The ball itself appeared to us luminous white, while the "wake" left in its passage through the air, was bluish green. It was visible, I should say, for twenty seconds.

Occurring, as it did, at a time when thousands were wending their way to church, it must have been very generally observed.

Harrow, October 26 A. W. B. J.

Singing Mice

WHEN at school a friend and I used to keep tame mice, and amongst our large stock was one of the so-called singing mice. The mouse in question was not one we bred ourselves, but was bought from a London dealer, so we had no opportunities of knowing whether it had ever been kept near a singing bird or not; but it was not at all averse to performing in broad daylight, and would chirp whilst a knot of boys were standing round it as freely as when the cupboard was closed.

As M. Brierre describes it (vol. xvi. p. 558), the mouse used to sit with its snout more or less elevated, but not at all to an uncomfortable height, and its throat used to throb like that of a bird whilst singing, the fur of the one being ruffled like the feathers of the other; and the song was something between that of a wren and that of a shrew mouse, and rather pleasing than otherwise.

At first we were inclined to attribute the noise to disease of the lungs or throat, but were unable to hold that opinion long, as there never seemed to be any pain or gasping connected with it, but the noise was always produced at periods of greatest rest, and chiefly when the mouse came out of its sleeping place to wash its face and paws, at which times it generally chirped at intervals. It never had the power of imparting the art to others, nor did any of its numerous progeny inherit its powers. Neither was it all short lived, but rather the contrary, and its death was caused by an accident. We were unable to consider the power of emitting the sounds at all the result of weakness or disease. HENRY H. SLATER

Sound-Producing Arthropods

I HAVE read with much interest the brief abstract given in NATURE (vol. xvi. p. 567) of Mr. Wood Mason's announcement to the Entomological Society of the discovery of stridulating organs in association with scorpions; reference being made at the same time to his recognition of similar sound-producing structures among other Arthropoda, including certain Crustacea. In this latter case no mention is made of the particular types with which these sound-organs have been observed, and I therefore hazard the relation of an instance that has recently fallen under my own observation with the chance of its proving a newly-recorded example.

The crustacean in question, which I have ascertained to possess sound-producing properties to an eminent degree, is a species of *Spheroma*, belonging to the Isopodous order of the class. I have not as yet ascertained the exact method in which sound is produced nor whether the animal has organs specially adapted for the purpose; on numerous occasions, however, my attention has been attracted to the glass jar of which, with the exception of microscopic Copepods and Protozoa, a single specimen of the species is the sole animal occupant, by a little sharp tapping sound produced three or four times consecutively with intervals of about one second's duration, and which I can almost exactly imitate by gently striking the side of the jar with the pointed end of a pipette. On being approached the little creature always endeavours to elude notice by passing to the opposite side of the stalk of seaweed, upon which it usually reposes in the same way that a squirrel dodges round the branch of a tree, and on no occasion so far have I been able to catch the little fellow *flagrante delicto*, or in the act of producing the sound which it most undoubtedly emits. The character and intensity of the sound produced associated with the small size of the animal, scarcely one quarter of an inch in length, induces me to believe that it is caused by the sudden flexion and extension of the creature's body. A more prolonged observation will no doubt clear up this point, but Mr. Wood Mason may possibly be in a position to throw further light upon the subject by means of the evidence he has collected in reference to other crustacean types.

Among the higher Decapodous crustacean order one species, *Alpheus ruber*, frequently collected by me in Guernsey, produces a snapping noise beneath the water by the sudden extension of the terminal joint of its larger claw that can be heard at a considerable distance, and that at once betrays its lurking place to a practised ear. The large sea crayfish (*Palinurus quadricornis*) again, often emits when handled what may be fitly described as a shrill squeaking sound by the rubbing together of the spinous abdominal segments. It would seem indeed that a closer study of the life habits of the aquatic Arthropoda is likely to reveal among its members as infinite a variety of sound-producers as has hitherto been determined to exist among their more familiar terrestrial congeners. W. SAVILLE KENT

St. Heliers, Jersey, October 27

Insects and Flowers

IN reference to the question whether insects are most attracted to flowers by scent or colour, may I mention that while staying at the hotel at Cettinge lately I was amused by the behaviour of some humming-bird sphinx moths. My room was roughly stencilled with a "spotty" pattern of purplish brown on the dull white plaster. Every morning these moths, with their probosces extended, used to attack the dabs of colour, hovering before them, just as though they were real flowers, but starting back with apparent amazement on finding that they were not. This seems the more remarkable because the wonderfully abundant aromatic herbs of that region, which must have supplied their usual food, have all, so far as I know, very inconspicuous flowers.

Notting Hill, October 27 A. J. H.

FRANCIS VON ROSTHORN

FRANCIS VON ROSTHORN, who died June 17, 1877, was the son of Matthew Rosthorn, of Lancashire, who went to Vienna in 1765, at the invitation of the Empress Maria Theresa, to establish the manufacture of metal buttons. He constructed the first rolling-mills in Austria; one at Vienna, another (in 1792) at Fahrafeld, in Lower Austria. Matthew von Rosthorn was ennobled by the Emperor Joseph II. in 1790, and died at Vienna January 3, 1805, leaving five sons. The youngest of these, born April 18, 1796, at Vienna, is the subject of this notice. These five brothers joined in creating extensive metallurgic establishments; the first (1817) at Oed; and another (1823) in Carinthia, for smelting zinc (then high in price) out of the Raibl and Bleyberg ores, by means of brown coal. Having purchased (1826) the state demesne of Wolfsberg, in Carinthia, with extensive metallurgical works, they constructed there a large rolling-mill, together with a puddling furnace. Francis von Rosthorn, having prepared himself for his practical career by attending the Mining Academy of Schemnitz, in Hungary (1814 to 1818), soon became acquainted with several eminent geologists, and obtained the patronage of the late Archduke John. He made several scientific tours in Carinthia, Carniolia, Styria, Salzburg, and the Hungarian border; in 1827 with Prof. Keferstein, in 1828 with Archduke John, in 1829 with Escher and Schrötter, and in 1832 with Dr. Boué. His annual visits to Archduke John at Gastein (1829 to 1836) were always connected with Alpine exploration. His later travels (1842 to 1847) were chiefly southward. In 1832 he communicated the results thus obtained to the Meeting of German Naturalists at Vienna; and in 1836 to the meeting at Freiburg. In 1848 he was elected into the Legislative Assembly ("Landtag") of Carinthia; and from 1852 to 1870 held the office of President of the Commercial and Industrial Board of that province. Francis von Rosthorn's constitution was exceptionally robust, so that up to his seventy-sixth year he was able to undertake arduous Alpine ascents. His conversation with persons of any social station was unaffectedly amiable; but he could be sarcastic when he met with affectation or baseless pretensions.

SPECTRUM OF AURORA AUSTRALIS

AS I believe no account of spectroscopic observations of the Aurora Australis have as yet been published, I venture to send this description of two auroræ observed during the stay of H.M.S. *Challenger* in high southern

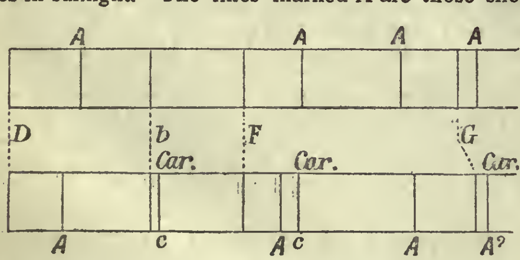
[1] Obituary Notice by Prof. E. Suess ("Report of the Imperial Geological Institute, Vienna," August 31, 1877).

latitudes. The opportunities of observing were not frequent, either from the rarity of the phenomena (which is very possible) or because the dense mass of cloud which is the prevailing feature of those regions prevented their being seen except when exceptionally bright.

Altogether four appearances were noted. The first was 1.30 on the morning of February 9, 1874, in lat. 57° S. and long. 75° E., bar. 29·0 in., ther. 35°. There were brilliant streaks to the westward; no spectroscopic observations were taken. The second was on February 21 at 9.30 P.M., lat. 64° S., long. 89° E., bar. 28·8, ther. 31°; one bright white curved streamer extended from Jupiter, which appeared to be near the focus, through Orion and about as far beyond. Under this was what appeared to be a black cloud, but the stars were visible through it. Real cumulus clouds hid great part of the remainder, but there were two vertical flashing rays that moved slowly to the right (west), generally the aurora was still and bright.

On examining the streamer with the spectroscope I found the usual three prominent lines, namely, one yellow-green, one green, the third blue or purple. I looked for the red line but could not find it.

The third aurora was seen on March 3, lat. 53° 30′ S., long. 109° E., bar. 29·1, ther. 36°, after some days wet and stormy weather. Soon after 8 P.M. the sky began to clear and the moon shone out. Noticing the light to the southward to be particularly bright I applied the spectroscope and found the distinguishing auroral line. About midnight I was called as there were very brilliant auroral clouds. The sky was almost clear, but south were two or three brilliant light clouds, colour very white yellow, shape cumulus stratus; from about west to near south extended a long feathery light of the same colour, parallel with the horizon, and between south and west there appeared occasionally brilliant small clouds, the upper edges seemed hairy, and gave one the idea of a bright light behind a cloud. The forms changed, but I did not notice any particular order, perhaps because my attention was particularly directed to examining the light with the spectroscope, and the great cold, for my fingers seemed almost frozen, and the motion of the ship made my task rather difficult. I could trace four lines, three bright, and one rather faint, and by reference to the moon, which was shining brightly, roughly determined their places. They must have been exceedingly bright to show so plainly in full moon. The spectroscope used was one of Grubb's single prism with long collimator. A needle point in the eye-piece marked the position of the lines, and a corresponding needle point carried on a frame with the point in the eye-piece and moved by a coarse thread screw, scratched the lines on a plate of blackened glass. I took two plates;—on the first I scratched the auroral lines and the telluric lines visible in the moonlight; on the second I scratched the auroral lines, the telluric lines shown by the moon, and the lines given by carbon in the flame of a spirit lamp; the next morning I verified the lines in sunlight. The lines marked A are those shown



by the aurora, those marked D, *b*, F, and G are the telluric lines, and those marked *car.* were given by the carbon in the spirit lamp.

The spectrum has been magnified five times from the plates. I cannot account for the different position of the auroral lines in the two plates, as the prism was not moved during the observations that I am aware of.

The fourth aurora was a slight one seen to the southward on March 6 at 8 P.M. It would be worth investigating whether the low barometer has anything to do with the absence of red in the spectrum, the normal state of the barometer is an inch lower in those regions than in more temperate latitudes.

I may as well add that on February 9 the aurora was preceded by a watery sunset, and the day broke afterwards with high cirrus clouds and clear horizon. On February 21 the aurora preceded a fine morning, cumulus stratus clouds. On March 3 there was a brilliant sunset followed by a fine morning; and on March 6, after the slight appearance of aurora, the clouds changed to high cirrus.

J. P. MACLEAR

ABSOLUTE PITCH

AT the present time the question of absolute pitch is attracting attention in consequence of the discrepancy between König's scale and the numbers determined by Appunn's tonometer. This instrument is founded upon the same idea as Scheibler's fork tonometer, and consists of a series of sixty-five harmonium reeds, bridging over an entire octave, and so tuned that each reed gives with its immediate neighbours four beats per second. The application to determine absolute pitch, however, does not require precision of tuning, all that is necessary being to count with sufficient accuracy the number of beats per second between each pair of consecutive reeds. The sum of all these numbers gives the difference of frequencies of vibration between the first reed and its octave, which is, of course, the same as the frequency of the first reed itself.

The whole question of musical pitch has recently been discussed with great care by Mr. Ellis, in a paper read before the Society of Arts (May 23, 1877). He finds by original observation with Appunn's instrument 258·4 as the actual frequency of a König's 256 fork, and Prof. Preyer, of Jena, has arrived at a similar result (258·2). On the other hand, Prof. Mayer in America, and Prof. Macleod in this country, using other methods, have obtained numbers not differing materially from König's. The discrepancy is so considerable that it cannot well be attributed to casual errors of experiment; it seems rather to point to some defect in principle in the method employed. Now it appears to me that there is such a theoretical defect in the reed tonometer, arising from a sensible mutual action of the reeds. The use of the instrument to determine absolute frequencies assumes that the pitch of each reed is the same, whether it be sounding with the reed above, or with the reed below; and the results arrived at would be vitiated by any mutual influence. In consequence of the ill-understood operation of the wind, it is difficult to predict the character of the mutual influence with certainty; but ("Theory of Sound," §§ 112-115) there is reason to think that the sounds would repel one another, so that the frequency of the beats heard when both reeds are sounding, *exceeds* the difference of the frequencies of the reeds when sounding singly. However this may be, in view of the proximity of consecutive reeds and of the near approach to unison,[1] the assumption of complete independence could only be justified by actual observation, and this would be a matter of some delicacy. If the mutual influence be uniform over the octave it would require a difference of one beat per minute only to reconcile König's and Appunn's numbers.

As to the amount of the influence I am not in a position to speak with confidence, but I may mention an obser-

[1] It must not be forgotten that the vibration of the tongue involves a transference of the centre of inertia, so that there is a direct tendency to set the sounding-board into motion.

vation which seems to prove that it cannot be left out of account. If two sounds of nearly the same pitch are going on together, slow beats are heard as the result of the superposition of vibrations. Suppose now that a third sound supervenes whose pitch is such that it gives rapid beats with the other two. It is evident that these rapid beats will be subject to a cycle of changes whose frequency is the same as that of the slow beat of the first two sounds. For example, in the case of equal intensities of two sounds there is a moment of silence due to the superposition, of equal and opposite vibrations, and at this moment a third sound would be heard alone and could not give rise to beats. The experiment may be made with tuning-forks, and the period of the cycle will be found to be sensibly the same whether it be determined from the slow beat of the two forks nearly in unison or from the rattle caused by the simultaneous sounding of a third fork giving from four to ten beats per second with the other two. In the case of forks there is no fear of sensible mutual action, but if it were possible for the third sound to affect the pitch of one of the others the equality of the periods would be disturbed. The observation on Appunn's instrument was as follows:—The reeds numbered 0 and 64 being adjusted to an exact octave, it was found that the beats arising from the simultaneous sounding of reeds 0, 63, and 64 were by no means steady, but passed through a cycle of changes in a period no greater than about five seconds. In order to work with greater certainty a resonator of pitch corresponding to reed 64 was connected with the ear by a flexible tube and adjusted to such a position that the beats between reeds 0 and 64 (when put slightly out of tune) were as distinct as possible, indicating that the gravest tone of reed 64 and the octave over-tone of reed 0 were of equal intensity. By *flattening* reed 64 (which can be done very readily by partially cutting off the wind) the beats of the three sounds could be made nearly steady, and then when reed 63 was put out of operation, beats having a 5 seconds' period were heard, indicating that reeds 0 and 64 were in tune no longer. It would appear, therefore, that when reed 63 sounds the pitch of reed 64 is raised, but in interpreting the experiment a difficulty arises from the amount of the disturbance being much in excess of what would be expected from the performance of the instrument when tested in other ways.[1]

I come now to an independent determination of absolute pitch, which it is the principal object of the present communication to describe. The method employed may be regarded as new, and it appears to be capable of giving excellent results.

The standard fork, whose frequency was to be measured, is one of König's, and is supposed to execute 128 complete vibrations in a second. When placed on its stand (which does not include a resonance box) and excited by a violin bow, it vibrates for a minute with intensity sufficient for the counting of beats. The problem is to compare the frequency of this fork with that of the pendulum of a clock keeping good time. In my experiments two clocks were employed, of which one had a pendulum making about $1\frac{1}{2}$ complete vibrations per second, and the other a so-called seconds' pendulum, making half a vibration per second. Contrary to expectation, the slower pendulum was found the more convenient in use, and the numerical results about to be given refer to it alone. The rate of the clock at the time of the experiments was determined by comparison with a watch that was keeping good time, but the difference was found to be too small to be worth considering. In what follows it will be supposed for the sake of simplicity of explanation that the vibrations of the pendulum really occupied two seconds of time exactly.

The remainder of the apparatus consists of an electrically maintained fork interrupter, with adjustable weights, making about $12\frac{1}{2}$ vibrations per second, and a dependent fork, whose frequency is about 125. The current from a Grove cell is rendered intermittent by the interrupter, and, as in Helmholtz's vowel experiments, excites the vibrations of the second fork, whose period is as nearly as possible an exact submultiple of its own. When the apparatus is in steady operation, the sound emitted from a resonator associated with the higher fork has a frequency which is determined by that of the interrupter, and not by that of the higher fork itself; nevertheless, an accurate tuning is necessary in order to obtain vibrations of sufficient *intensity*.[1] By counting the beats during a minute of time it is easy to compare the higher fork and the standard with the necessary accuracy, and all that remains is to compare the frequencies of the interrupter and of the pendulum. For this purpose the prongs of the interrupter are provided with small plates of tin so arranged as to afford an intermittent view of a small silvered bead carried by the pendulum and suitably illuminated. Under the actual circumstances of the experiment the bright point of light is visible in general in twenty-five positions, which would remain fixed, if the frequency of the interrupter were exactly twenty-five times that of the pendulum. In accordance, however, with a well-known principle, these twenty-five positions are not easily observed when the pendulum is simply looked at; for the motion then appears to be continuous. The difficulty thence arising is readily evaded by the interposition of a somewhat narrow vertical slit, through which only one of the twenty-five positions is visible. In practice it is not necessary to adjust the slit to any particular position, since a slight departure from exactness in the ratio of frequencies brings all the visible positions into the field of view in turn.

In making an experiment the interrupter is tuned, at first by sliding the weights and afterwards by soft wax, until the interval between successive appearances of the bright spot is sufficiently long to be conveniently observed. With a slow pendulum there is no difficulty in distinguishing in which direction the pendulum is vibrating at the moment when the spot appears on the slit, and it is best to attend only to those appearances which correspond to one direction of the pendulum's motion. This will be best understood by considering the case of a conical pendulum whose motion, really circular, appears to be rectilinear to an eye situated in the plane of motion. The restriction just spoken of then amounts to supposing the hinder half of the circular path to be invisible. On this understanding the interval between successive appearances is the time required by the fork to gain or lose one complete vibration as compared with the pendulum. Whether the difference is a loss or a gain is easily determined in any particular case by observing whether the apparent motion of the spot across the slit (which should have a visible breadth) is in the same or in the opposite direction to that of the pendulum's motion.

In my experiment the interrupter *gained* one vibration on the clock in about eighty seconds, so that the frequency of the fork was a thousandth part greater than 12·5 or 12·51. The dependent fork gave the ninth harmonic, with a frequency of 125·1. The beats between this fork and the standard (whose pitch was the higher) were 180 in sixty seconds, so that the frequency of the standard was as nearly as possible 128·1, agreeing very closely with König's

[1] The value of my instrument has been greatly enhanced by the valuable assistance of Mr. Ellis, who was good enough to count the entire series of beats, and to compare the pitch with that of the tuning-forks employed by him in previous investigations. Mr. Ellis, however, is not responsible for the facts and opinions here expressed. It may be worth mentioning that the steadiness or unsteadiness of the beats heard when three consecutive reeds are sounding simultaneously is a convenient test of the equality of the consecutive intervals. The frequency of the cycle of the four a second beats is equal to the difference of the frequencies of either of the actual extreme notes and that which, in conjunction with the other two, would make the intervals exactly equal.

[1] This tuning is effected by prolonging as much as possible the period of the beat heard when the dependent fork starts from rest. This beat may be regarded as due to an interference of the forced and natural notes.

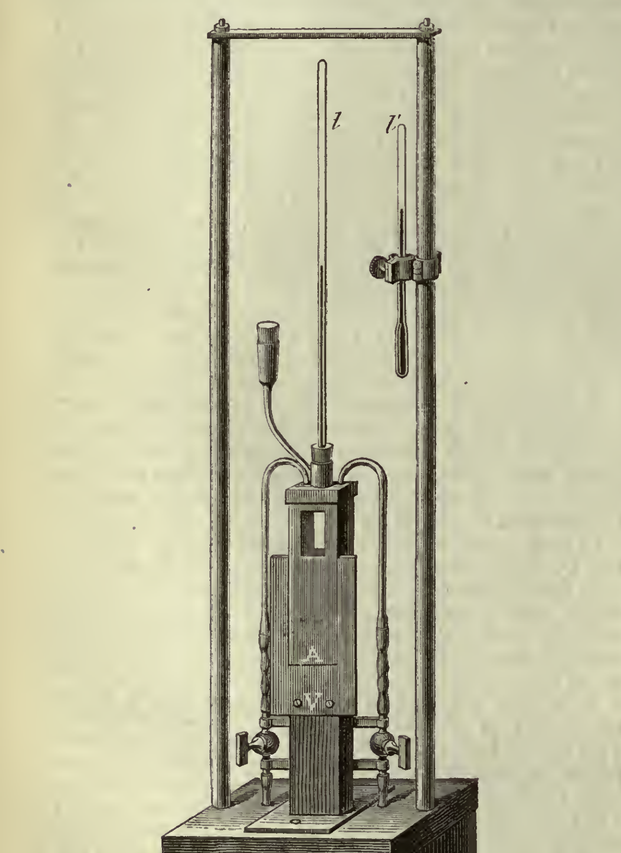
scale. The error of the determination may amount to ·1, but could not, I think, exceed ·2.

I ought to add that the *approximate* determination of the frequency of the interrupter must be made independently, as the observation on the pendulum does not decide *which* multiple of $\frac{1}{2}$ nearly coincides with the frequency of the fork. Also the relation between the two auxiliary forks was *assumed*, and not determined; but as to this there can be no doubt, unless it be supposed that König's scale may be in error to the extent of a whole tone.

RAYLEIGH

A NEW CONDENSING HYGROMETER

A NEW apparatus of this kind, invented by M. Alluard, and described by him in *La Nature*, is distinguished from all those hitherto employed by the two



Alluard's Condensing Hygrometer.

following points:—(1) The part on which the deposit of dew is to be observed is a plane well-polished face A, of silver or gilt brass; (2) This plane face is set in a plate of silver or brass V, itself gilt and polished, which does not touch it, and which, never being cooled, always preserves its brightness. It results from this arrangement that the deposit of dew is observed with the greatest facility, in such a manner that there is scarcely any difference between the temperatures of the instants when the dew commences and ceases to appear on the instrument properly cooled by the evaporation of ether.

The form of the instrument is that of an upright prism with square base. Its height is eight centimetres and the side of its base eighteen millimetres. Three small copper tubes pass through the upper lid; the first reaches the bottom, and the two others, one surmounted by a funnel for introducing the ether, open only above. Two small windows enable us to judge of the agitation of the ether by the aspiration or driving back of the air intended to produce coolness by the evaporation of the volatile liquid; it is best to work with an aspirator, the aspiration of which we can regulate as we wish. A central tube permits the introduction of a thermometer, *t*, which, placed in the middle of the evaporating liquid, gives the temperature at which the deposit of dew occurs. A small sling thermometer, fixed on the side of a brass support, enables us to determine with precision the temperature of the air whose hygrometric condition we wish to ascertain.

Daniell's condensing hygrometer was formerly modified by M. V. Regnault. He made it an instrument of precision; but his apparatus has not been much used on account of its delicate construction. The deposit of dew, being made on a cylinder of polished silver, is difficult to observe. In the plane face hygrometer of M. Alluard this deposit is very easily seen by contrast, even at some metres distance, especially if care is taken to observe in such a manner as to avoid all reflection from the gilt faces, when they will appear a beautiful ebony black. Its employment being very simple, without losing anything of its precision, there is no reason why it should not come into general use.

Since meteorological observations have multiplied on all sides, the hygrometer has assumed an importance which it had not before. The psychrometer is at present almost exclusively employed. But all physicists know that below zero we cannot trust the results which it gives; it is the same when the air is much disturbed. And yet, almost everywhere, it continues to be employed on these conditions. We hope that the plane face hygrometer, furnished during the winter cold with an aspirator filled with glycerine, will be able to yield accurate results to all who do not fear to devote a few minutes to its working.

OUR ASTRONOMICAL COLUMN

EARLY OBSERVATIONS OF THE SOLAR CORONA.—Referring to Mr. Dreyer's letter in NATURE (vol. xvi. p. 549), the note in this column relating to the solar eclipse of 1605 was by no means intended to imply that it afforded one of the earliest observations of the corona, nor can the eclipse of Stiklastad, as it has been usually called, on August 31, 1030, be so characterised. Prof. Julius Schmidt, of Athens, had called attention in 1870 to a record of the eclipse of December 22, 968, in Corfu, where he found a reference to the corona, but a much earlier date is assigned by Prof. Grant for the first mention of this phenomenon. It occurs in Philostratus' "Life of Apollonius of Tyana," Book VIII., chap. xxiii., in the Leipzic edition, and runs thus:—*Περὶ δὲ τὸν χρόνον, ὂν ἐν τῇ Ἑλλάδι ἐνεσπούδαζεν, ἐπεῖχε τὸν οὐρανὸν διοσημία τοιαύτη. τὸν τοῦ Ἡλίου κύκλον περιελθᾶν στέφανος, ἐοικως Ἴριδι, τὴν ἀκτῖνα ἠμαυροῦ.* Prof. Grant considers that "the words here quoted refer beyond all doubt to a total eclipse of the sun, and that the phenomenon seen encompassing the sun's disc was really as well as verbally, identical with the modern corona." He also points out that Plutarch,

who was contemporary with Apollonius, refers to a total eclipse of the sun which had *recently* occurred, and remarks of total solar eclipses in general that "a certain effulgence is seen round the circumference," so that although the sun may be wholly covered by the moon "still the eclipse is deficient in duration as well as in amplitude," this surrounding effulgence not allowing of a very intense shadow. These remarks of Philostratus and Plutarch Prof. Grant thinks may probably apply to the same eclipse, and afford "the earliest allusions to the corona recorded in history." Several attempts have been made to discover the date of the phenomenon, but so far as we know without success.

The earliest distinct and more accurate account of the corona is that given by MM. Plantade and Capiés, who observed at Montpellier on the occasion of the eclipse of May 12, 1706.

THE OUTER SATELLITE OF MARS.—Our ephemeris of the satellite of Mars is here continued ten days further from the elements employed last week, though much greater difficulty must now attend observations than when the discovery was first announced. In the middle of August the distance of the planet from the earth was less than 0·4; on November 12 it will have increased to 0·68. At the next opposition in 1879, the least distance of Mars will be 0·482, at a north declination of upwards of 18°, so that observations may be made at many observatories in this hemisphere, probably without greater difficulty than about the late opposition; at the following one in December, 1881, the planet will attain a declination of 27° N., but its distance from the earth will be at no time less than 0·602.

Prof. Asaph Hall's complete discussion of the observations of the satellites of Mars, made in the present year, will be looked for with much interest; it is only fitting that this investigation should be left in the hands of their discoverer, who has made the year 1877 a very notable epoch in the history of practical astronomy.

The following positions of the outer satellite are for 8h. G.M.T.

Nov. 3 ... Pos.	358° ... Dist.	21″	Nov. 8 ... Pos.	27 ... Dist.	25″
,, 4 ... ,,	69 ... ,,	52	,, 9 ... ,,	75 .. ,,	48
,, 5 ... ,,	122 ... ,,	25	,, 10 ... ,,	150 ... ,,	20
,, 6 ... ,,	236 ... ,,	42	,, 11 ... ,,	243 ... ,,	46
,, 7 ... ,,	272 ... ,,	37	,, 12 ... ,,	285 ... ,,	28

DE VICO'S COMET OF SHORT PERIOD.—The year to which we drew attention some time since (1876·9-1877·9) as one which might possibly witness the re-discovery of De Vico's comet of 1844 is drawing to a close without its having been remarked, and the chance of detecting it at this season if the perihelion passage be not already passed, is small. We must therefore probably place the comet in the class which, though undoubtedly moving in elliptical orbits of small dimensions when under observation, are now "lost." Whether in the case of De Vico's comet this arises from a larger error in the determination of the mean motion in 1844 than at present appears admissible, or whether the action of the planet Mars, to which allusion has been made in this column, may explain it, or again, whether the comet has encountered one of the minor planets, and thereby been deflected or disintegrated, cannot be at present ascertained. It was hardly to have been anticipated that the laborious investigations of Prof. Brünnow relating to the motion of this comet from 1844-55 would not have resulted in its re-observation.

METEOROLOGICAL NOTES

MEAN ATMOSPHERIC PRESSURE OF EUROPE.—A great contribution to this very important subject has been made by Dr. Buys Ballot in the second volume of the "Nederlandsch Meteorologisch Jaarboek voor 1872," which has just been published. The first 130 pages of the volume are occupied with a very careful and in certain directions exhaustive discussion of the barometric observations made at about 110 places situated in different parts of Europe from 1774 to 1874. The method of discussion is identical with that adopted by Dr. Buys Ballot in his recently published paper on the Meteorology of Holland (NATURE, vol. xvi. p. 89). This method consists in accepting as the normal mean atmospheric pressure at Greenwich, Vienna, and Palermo, the arithmetic means of the observations made at these places which embrace periods of 100, 101, and 84 years respectively. The normal values for the other stations have been determined by the process of differentiation, that is, by a comparison of the means of all the observations made at the place with the corresponding means of one or more places at the nearest available stations whose normals have been already determined, and thereafter applying the necessary correction. Thus the normals which have been arrived at in this very laborious manner are substantially the averages which would have been obtained if the observations at each of the stations had been made during precisely the same terms of years. The thirty years' averages should probably have been accepted as the best normals for Stykkisholm in Iceland, instead of correcting these averages from the Greenwich and Christiania observations, seeing that a low average barometer at Stykkisholm is frequently coincident with a high barometer at either or both of these stations, and *vice versâ*. The resulting differences, however, are but slight. This work of Dr. Buys Ballot, particularly when looked at with reference to future discussions, may be said to take a place at once as a classic of meteorology. The next step to be taken in this field of European meteorology is the discussion of all good barometric observations made in Europe during the meteorological lustrum ending with 1875. To the results of this discussion corrections could be applied from Du Buys Ballot's normals, which are sufficiently numerous for the purpose, and thus a graphic representation could be made of the closest possible approximation to the true mean atmospheric pressure of Europe. In this way, by disclosing the striking, and in a large measure still unrecognised, influence of large masses of land and water on the barometric pressure, much light would be thrown on the origin and history of those great atmospheric currents which, flowing or sweeping over this continent, are mainly instrumental in determining the climates of its different regions.

METEOROLOGY OF NEW YORK, U.S.—The "Annual Report of the New York Meteorological Observatory for 1876" gives, in addition to the individual observations made daily, and their monthly and annual averages, a more than usually full statement of rain and wind observations. On pp. 39-88 are given the details of the amount of rain and snow-water which fell each hour from 1870 to 1876, together with the hourly averages of each month for these seven years. These hourly means show maximum amounts during winter, from 11 A.M. to 3 P.M.; during spring, from 9 P.M. to 1 A.M.; during summer, from 5 to 10 P.M.; and during autumn, from 3 to 8 A.M. The irregularity of these periods and the irregular occurrence of secondary maxima indicate that seven years is too short a time for the determination of the hourly curve of the rainfall at New York. There appears, however, a tendency to a double maximum varying considerably with season. Extended observation alone can give this curve. The influence of the daily fluctuation of temperature and of the sea breeze which sets in very decidedly from south-east during the hot months on the rain-curve, can then be studied. During the same seven years the duration of each shower has been noted in the number of minutes, the average result of which is that the minimum time of fall, a small fraction less than two days, occurred in June; from this time it slowly but steadily rose to 3 days

17 hours in January, fell a little in February, and rose to $4\frac{1}{3}$ days, the annual maximum in March, from which it rapidly declined to the minimum in June. On a mean of the past forty-one years the monthly averages are in excess from May to August inclusive, August and May being decidedly the months of maximum rainfall, whilst January and February are the months of least rainfall. From 1836 the annual amounts show with some interruptions a decided increase in the rainfall up to 1868, since which year there has been as decided a decrease. This result is generally corroborated by the rainfall at Washington, Philadelphia, and Providence, which Mr. Draper adds to his Report. A valuable table of the monthly amounts from 1836 to 1876 is printed at p. 6. In accordance with the suggestion thrown out by Mr. Hill (NATURE, vol. xvi. p. 505) the amounts for the winter months have been picked out, averaged for the eleven-years sun-spot period, and bloxamed. The results, thus worked out, are in inches these, beginning with the first year of the cycle :—22·57, 22·26, 22·92, 23·31, 22·24, 21·03, 21·98, 21·05, 21·14, 22·18, and 23·56.

METEOROLOGY IN RUSSIA.—The St. Petersburg Agronomical Society has appointed a special committee for the purpose of elaborating, in accord with other Russian scientific bodies, a scheme for establishing throughout Russia an extensive network of meteorological stations. Owing to the interest manifested in the subject by a great number of agriculturists, it is expected that the plan will soon be put into execution.

NOTES

WE much regret to have to announce the death, on Sunday last, of Mr. Robert Swinhoe, F.R.S., a naturalist whose numerous contributions to our knowledge of the mammalia and birds of the Chinese Empire have proved invaluable to zoological science. We hope, next week, to give an account of Mr. Swinhoe's work.

THE International Committee for the erection of a monument to Liebig at Munich, having now at their disposal a sum of 120,000 marks, invite sculptors of all nations to send in models for their acceptance. A prize of 2,000 marks will be given to the model which takes the first place, and 1,500 to the second. The model of the statue should be forty centimetres, and of statue and pedestal about one metre in height. Models should be addressed to the "Castellan der königlichen Akademie der Künste, 38, Unter den Linden, Berlin," where they will be received from June 1 to 15, 1878, to be exhibited first at Berlin and then at Munich. The Committee bear all the expenses of transport.

IT has been noted in the French papers *à propos* of the recent colliery explosion, that M. Leverrier, when presiding at the meeting of the French learned societies at Easter, proposed to extend the telegraphic warnings of the International Meteorological System to the several French pits. The question of the illumination of mines by electricity has been revived by these terrible tragedies, and a number of interesting communications connected with that important topic will be presented and fully discussed at the next meeting of the French Academy of Sciences.

IT was stated by one of the speakers at the last quarterly meeting of the French Academies that M. Thiers had written a complete work on Spherical Trigonometry when quite a young man.

WE regret to record the death of M. Cazin, Professor of Physics at one of the Paris Lycées, and an active member of the Paris Physical Society. M. Cazin was sent to the Island of St. Paul by the Academy of Sciences under the command of Capt. Mouchez to make physical observations during the last transit of Venus; he there contracted the germ of the illness which has proved fatal at the early age of forty years. He had been admitted to the Observatory by M. Leverrier to execute a series of delicate researches on magnetism, which have been left unfinished.

THE Harveian Oration at the Royal College of Physicians of London will be delivered in 1878 by Dr. J. Burdon Sanderson, F.R.S.

AN anthropological exhibition will be opened at Moscow in 1879, in connection with the society of Friends of Natural Science. Many objects of great scientific value, almost exclusively of Russian origin, are already in the hands of the organising committee.

MR. TUCKWELL, recently head-master of Taunton College School, has issued a circular addressed to head-masters, giving an account of his connection with the school whose reputation he did so much to raise, and which has treated him so ungratefully. Our readers are already familiar with the details of this unhappy matter, and we are sure will all wish with us that Mr. Tuckwell may soon find a field for the exercise of his powers as a successful teacher unfettered by the narrowness of uneducated and narrow-minded directors. Mr. Tuckwell gave Taunton School a status and a name; the Council of the school have undone all his work, and left the school nowhere.

THE winter session of the Chester Society of Natural Science opened on October 25 with a lecture on "The Arctic Regions," by Mr. de Rance, of H.M. Geological Survey. The upper silurian, lower carboniferous sandstones, mountain limestone, and lias of the Parry Archipelago, as well as the oolites, cretaceous and miocene rocks of Greenland and Grinnel Land, were described as occupying hollows in the old Laurentian Mountains, and the existing cold climate was stated to have probably only prevailed since the last glacial epoch. The range of the northern mammals, and the discovery of remains of the Eskimo by Capt. Feilden, R.N., naturalist of the *Alert*, near Cape Beechey, far north of the present limit of human habitation, and further north than any previous discovery of man or his works, were commented on; and a large collection of Arctic fossils were exhibited by Sir Phillip Egerton, collected in Grinnel Land by his nephew, Lieut. Egerton, R.N., of the late British Arctic Expedition.

AN unusually interesting scientific *soirée* was recently held at the Bristol Museum and Library, which has been characterised as "the headquarters of scientific research in the west of England." Many of the most recent scientific experiments were shown, the most attractive probably being Prof. Graham Bell's exhibition of the wonders of the telephone. During the winter a course of lectures has been arranged for at the museum, mostly scientific, as follows:—November 19—A. R. Wallace, F.R.G.S., F.L.S., the Distribution of Animals as indicating Geographical Changes; November 29—Prof. Ball, F.R.S., a Night at Lord Rosse's Telescope, illustrated by the Oxy-hydrogen Lantern. December 10—Frederick Wedmore, Rembrandt; his Life and Work. January 14—Prof. Marshall, M.A., Principal of University College, Bristol, The Economic Condition of America. January 28—Prof. W. C. Williamson, F.R.S., Coal and Coal Plants. February 11—C. T. Hudson, M.A., LL.D., The Larger and Rarer Rotifers; illustrated with Transparent Diagrams. February 26—Prof. Rowley, M.A., of University College, Bristol, Francis Bacon: his Personal Character and Political Career. March 11—Dr. J. H. Gladstone, F.R.S., Fiery Meteors and Meteoric Stones. March 25—J. Norman Lockyer, F.R.S., Sun Spots in Relation to Indian Famines, with Spectroscopic Experiments and Oxy-hydrogen Lantern Illustrations.

The Royal Society of New South Wales, originated in 1821 as the Philosophical Society of Australia; after an interval of repose it was revived in 1850 as the Australian Philosophical Society, by which designation it was known until 1856, when the name was again changed to that of the Philosophical Society of New South Wales, and finally, about ten years ago, by the sanction of her Majesty the Queen, it assumed its present title. Judging by its present list of members it would seem to be prospering, but judging from the volume of its *Proceedings* (vol. x. for 1876) its scientific life might be higher, and we would venture to express the hope that future volumes may give us a larger number of memoirs treating of that vast quadrilateral of which Sydney is the acknowledged capital. Of the articles in this volume we would notice the following: On the Deep Oceanic Depression off Moreton Bay, by the Rev. W. B. Clarke, F.R.S.; On some Tertiary Australian Polyzoa, by the Rev. J. E. T. Woods. The species were with one exception derived from the Mount Gambier polyzoan limestone, South Australia, and are all described as new; ten are described and figured as belonging to the genus Eschara, two species of the genus Pustulipora are described, and one Tubulipora. On the formation of Moss Gold and Silver, and on a Fossiliferous Siliceous Deposit from Richmond River, is the title of a paper by Prof. Liversidge. The composition of this deposit shows that it answers to the common siliceous sinters or geyser deposits. The weathered surfaces are usually marked with the remains of ferns which stand out in relief, and more rarely through the mass are to be found the remains of certain fruits and seeds. These latter have been described by Baron Müller as belonging to a plant (*Liversidgea oxyspora*) allied to Capparideæ and Bixaceæ, the fruits are from two-thirds to nearly an inch in diameter, divided into four turgid lobes, placentas parietal; seeds turgid; oval towards one extremity and attenuated at the other; both fern and fruits are figured. In the discussion following the reading of a paper by the Rev. W. B. Clarke, F.R.S., On the Effects of Forest Vegetation on Climate, many interesting statements were made as to the condition of the forests in the neighbourhood of Sydney at the present time, and so long back as forty years ago.

Petermann's *Mittheilungen* for November contains a paper by Dr. G. Radde describing the journey of himself and Dr. Sievers from Erzeroum to the Bin-Gol-Dagh; the paper is full of details concerning the botany of the region traversed. Under the title of "Tekna and Nun," Dr. Rohlfs gives some valuable information on the part of the Sahara about the south-west of Morocco, showing that it is by no means so barren as is generally thought, and that even the most recent maps of the region are unsatisfactory.

One of the most interesting papers in the September number of the *Bulletin* of the French Geographical Society is an account by M. Brau de St.-Pol-Lias of his visit to the French Colonial stations recently established on the coast of Sumatra, in the province of Deli. The author gives many interesting observations on the people and the products of the part of the island which he visited, and speaks hopefully of the colony, which he considers an excellent basis for the exploration of the island. In the same number Dr. Harmand gives some account of recent journeys he made in Cambodia.

The first map showing the whole of Stanley's route from Bogomayo to the mouth of the Congo has been published by *L'Exploration* (October 21). In this map the course of the Congo is roughly shown as indicated in Stanley's letter, and that also of the Ogové according to the explorations of de Brazza, Lenz, and Marche. The trend of the Ogové to the south-east is shown, and its probable junction with the Congo by two arms indicated.

The Geographical Society of Paris has received letters from the French Consul at Zanzibar informing them that a road is being opened from Zanzibar to Tanganyika, for carting by oxen. It is expected that ere long explorers will be able to dispense with native porters.

A Geographical paper has been started at Lyons by M. du Mazet, one of the staff of the *Courrier de Lyon*. It will record the transactions of all the provincial geographical societies of France. The Lyons Geographical Society will have the advantage of a number of communications from the Roman Catholic missionaries who have an old-established special seminary and college in that city.

In the *Times* of Wednesday last week appeared a long story about the discovery of the remains of Columbus in St. Domingo. At Madrid, the *Times* Paris correspondent now states, the story is declared to be a hoax, inasmuch as "a Spanish squadron years ago escorted the remains to Havannah, where they lie in the Cathedral."

Under the title of "Pictorial Geography for Young People," Messrs. Griffith and Farran have published a neat little map intended to exhibit graphically the significance of the various terms used in geography—continent, island, river, lake, mountain, volcano, city, &c. It is necessarily exaggerated, but in the hands of a judicious teacher might be a valuable and attractive help to the teaching of the elements of geography.

Two severe shocks of earthquake were experienced at Lisbon at 6.45 A.M. of October 25. No damage was reported.

Under date October 17, it is reported from Smyrna, in Asia Minor, that there had been, during a few days, several earthquake shocks doing no further harm but cracking some walls.

It has been affirmed by P. Secchi of Rome, that iron heated red is transparent to light. This is denied by M. Govi of Turin, who, in a paper to the French Academy, describes some experiments on the subject, and shows how one may be deceived in studying the phenomena. If a mixture of borax and carbonate of soda be fused in a thin platinum crucible raised to a red heat, there will be seen on the exterior of the vessel the form of the liquid mass with all its accidents of rapidly varying form, indicated by a zone of less brightness than the upper portion of the metallic surface. At first sight it is natural to infer a transparence for light of the heated platinum, but (M. Govi points out) the case is really one of transparence for radiant heat; that is to say, a phenomenon connected with the good conductivity of platinum. The liquid, liberating carbonic acid, is less hot than the crucible, and is constantly borrowing heat from it. It is inevitable, then, that at every point where the liquid touches the metal, the latter relatively cooled, should appear less luminous than in the neighbouring region. M. Govi gives some other examples of the phenomenon.

"Shorthand for General Use" is the title of a little volume by Prof. Everett, of Belfast, published by Marcus Ward and Co. Prof. Everett's system claims several advantages over Pitman's, one being that the vowels can be written continuously with the consonants, and thus the word has not to be gone over a second time to insert the vowels. The system appears to us decidedly worth the attention of anyone wishing to learn shorthand.

We have received the eighth edition of Prof. Atkinson's translation of Ganot's "Physics." About sixty pages of additional matter, with an equal number of illustrations, have been added to this edition. Messrs. Longmans and Co. are the publishers.

Another scientific play is now being performed at the Cluny Theatre, Paris, under the title of the "Les 6 Parties du Monde." It is written by M. Figuier, the well-known scientific story-teller. The sixth part of the world is supposed to be the Antarctic

continent, where Dumont Durville is made to land. It is a masterly panorama of a number of climes and countries, enlivened by a well-constructed plot.

DR. HOEK, of Leyden, sends us the following additions to the list of dealers in zoological specimens given by Prof. Ray Lankester in a recent number of NATURE:—1. Hilmar Lührs, Fischer f. Zoologen und Aquarien, Helgoland (Unterland), for fish and invertebrates (alive and in spirits, specimens of all classes). 2. The Zoological Station of Dr. Anton Dohrn, Naples, for fish and invertebrates (spirit specimens).

THE additions to the Zoological Society's Gardens during the past week include three Tigers (*Felis tigris*), born in the Gardens, but did not survive; a Common Genet (*Genetta vulgaris*) from North Africa, presented by Mr. P. V. Carletti; two Hyacinthine Porphyrios (*Porphyrio hyacinthinus*) from West Asia, presented by Mrs. Henry Cobb; two All-Green Parakeets (*Brotogerys tiriacula*) from South America, presented by Miss Rowe; two Yellow-bellied Liothrix (*Liothrix luteus*) from India, presented by Gen. Breton; two common Marmosets (*Hapale jacchus*) from South-east Brazil, presented by Mrs. Clayton; three Darwin's Pucras (*Pucrasia darwini*), a Chinese Blue Magpie (*Urocissa sinensis*) from China, a Sun Bittern (*Eurypyga helias*) from South America, deposited; a Moose (*Alces machlis*) from North America, purchased.

AMERICAN SCIENCE

PROF. HENRY'S portion of the report of the Smithsonian Institution for the year 1876 has been printed in separate pamphlet form, in advance of the entire volume, and gives the usual record of operations for the period. It draws attention to the fact that it is the thirtieth of the annual series made by him, and that the policy advised at the first meeting of the board has been carried out with scarcely any modification. The original fund of 541,379 dols. has been increased to 714,000 dols., although a building costing nearly 500,000 dols. has been erected. There is a library of 70,000 volumes of the most valuable class of books, namely, the serial scientific publications of learned societies. The museum has grown until it now ranks among the best in existence. This embraces copious collections illustrating the ethnology and natural history of the world. The institution has published twenty-one quarto and forty-two octavo volumes of transactions and reports. It has carried on successfully a great system of meteorological observations (only intermitted on the successful operations of the Signal Service), the results of which have been issued by a number of stately volumes. It is now prosecuting a great system of international exchanges, for the benefit of the whole world. Its correspondence, both at home and abroad, requires a large number of clerks and specialists; and the name of Smithson is universally known in consequence.

Details have been recently published (*Proc.* Acad. Nat. Sci. Philadelphia, 1877, p. 255) of the exploration of a specially interesting mound at Coup's Creek, Macoupin County, Illinois. Four skeletons sat within it, considerably enveloped in a peculiar granulated but exceedingly tenacious earth. They were placed two and two, their arms crossed, the knees of one pair pressing sharply against the backs of the other, and the faces of all turned directly toward the east. Though the greatest care was taken, only one skull was removed comparatively perfect. The whole grave measured but six feet in length by three in width, and it contained in addition to the skeletons four large marine shells of *Pyrula* (*Busycon*) *perversa* (Luin.), each similarly placed in relation to the bodies. The smaller end of one shell was placed in the right hand of each individual, while the larger portion rested in the hollow above the left hip. But, still more remarkable, within each shell had been packed what appeared to be the bones of a child; the skull, crushed before burial, protruded beyond the aperture. The suggestion is made that these infants were sacrificial offerings in honour of the dead. The graves in these mounds are constructed of stone slabs from the locality, and hence they are known as stone graves. The builders give evidence of decided constructive ability, and of having been careful cultivators of the soil. The grave-mounds are found upon ridges, while others on which dwellings were supported are near streams. A systematic series of mounds of similar origin extends from the foot of Lake Michigan to the mouth of the Illinois river, a distance of two hundred and fifty miles. Unfortunately the remains are scarcely ever capable of being preserved, or even of being examined satisfactorily on exhumation.

The following are notes of papers in the October number of the *American Journal of Science and Arts*:—The nickel plates now largely used as anodes for nickel plating are prepared by fusing commercial nickel, generally with addition of charcoal, and casting in suitable form. From an analysis of several specimens of cast nickel by Mr. Gard, it appears that silica may be reduced and retained as silicon, and that a considerable amount of carbon may be present (*e.g.*, 1·9 and 1·8 per cent.). One experiment made with a view to ascertain how much carbon nickel may take up under conditions to which it is more or less exposed in the processes of manufacture and casting, was to pack half-a-pound of granular commercial nickel in layers with charcoal in a Hessian crucible, in which it was exposed to a full red-heat twelve hours. No fusion took place. The temperature was then raised till there was complete fusion. The resulting metal was strongly magnetic, quite soft, and to a considerable extent malleable. Its specific gravity was 8·04, and it had a fracture like that of fine-grained pig-iron, scales of graphite being plainly visible. It was found to contain of total carbon 2·105, 2·130; graphitic carbon, 2,030, 1·990; silicon, ·360. Mr. Gard also made some experiments on the deportment of nickel and cobalt towards hydrocarbon at a high temperature, the substances being placed in a platinum trough within a porcelain tube and treated with a slow current of pure dry marsh-gas at a full red heat. In one case thin plates of pure electroplate nickel (·8597 gr.) were found at the close to have gained 10·649 per cent.; in another 1·2697 gr. of cobalt gained 12·758 per cent.

Among other chemical contributions we note one on the iodates of cobalt and nickel, by Mr. Fullarton, who finds that the true normal iodates contain really six molecules of water of crystallisation, and that they are essentially different from the salts obtained by Rammelsberg. Several specific-gravity determinations follow (by students of Cincinnati University), including those of a series of chromates, by Miss Abbot. Pettersson has lately shown that selenates have molecular volumes exceeding those of the corresponding sulphates by six for each molecule of the acid radicle. On comparing the chromates with Pettersson's selenates, it is found that the two series of salts have approximately equal molecular volumes; the difference, if any exists, being very slightly plus for the selenates. If regularities of this kind can be thoroughly established, it will be easy (Prof. Clark suggests), having the density of a chromate, to calculate that of the corresponding sulphate or selenate, or *vice versâ*.

A preliminary catalogue of the reptiles, fishes, and Leptocardians of the Bermudas is furnished by Mr. Brown Goode, comprising 148 out of 163 known species. The Bermudan fauna shares with the West Indies 116 species (or 79 per cent.), of which 58 (or 40 per cent.) are peculiar to the West Indies, while many others have their centres of distribution in that region. With the Eastern United States Bermuda shares 47 species, and with the waters of the Pacific and Indian Ocean 32 species. Mr. Goode also gives a description of four species of fishes believed to be new.

Prof. Dana draws some lithological and orographic conclusions in his (continued) paper on the relations of the geology of Vermont to that of Berkshire, and the *Journal* also contains some information on the Archæan of Canada and the geology of New Hampshire, &c.

THE EARTHWORM IN RELATION TO THE FERTILITY OF THE GROUND

FROM observations extending over a number of years, M. Hensen is led to the conclusion that infertile undersoil is rendered valuable by the action of worms in two ways, viz., by the opening of passages for the roots into the deeper parts, and by the lining of these passages with humus. This will be more fully understood from the following facts regarding the life-habits of the worm (*Lumbricus terrestris*) given in M. Hensen's paper in the *Zeitschrift für wissenschaftliche Zoologie*.

It is known that the adult animals in wet weather come up to the surface by night, and, with their hinder end in their tube,

search the ground round about. They then draw whatever vegetable material they can find into their tubes—fallen stems and leaves and small branches. In the morning one then finds little heaps of plant-fragments projecting at various parts of the surface, and each of them penetrating the tube of a worm. On closer examination it is found that the leaves have each been rolled together by the worm, and then drawn into the tube in such a way that the leaf-stalk projects. The portion of the leaf in the tube is moist and softened, and only in this state are plants consumed by the worm. There are distinct indications that the worm gnaws them, and after some days the meal is ended. The food is never drawn deeper down into the ground. In digging the ground at various seasons it was only very rarely that plant remains were found in the subsoil, and probably they got there by accident.

With reference to the structure of the worm-tubes, some interesting facts were established in these researches. In humus their character is difficult to make out, owing to the looseness of the mass. In sand they proceed almost vertically downwards three, four, or even six feet, whereupon they often extend some distance horizontally; more frequently, however, they terminate without bending. At the end of the tube the worm is found with his head upwards, while round about him the tube is lined with small stones. On the sandy wall of the tube one observes more or less numerous black protuberances which make the sand fertile. These are the secretions of the worm, which, after being removed out of a tenanted tube, are found next morning replaced by fresh matter. They are observed after a few days, when a worm is put in a vessel with clean sand, and allowed to make a tube for itself. Older abandoned tubes are pretty regularly lined with the earth formed by the worm, and some passages are densely filled with black earth. This black substance appears to diffuse somewhat into the sand.

In about half of the tubes, not quite newly made, M. Hensen found roots of the plants growing at the surface, in the most vigorous development, running to the end of the tube and giving off fine root-hairs to the walls, especially beautiful in the case of leafy vegetables and corn. Indeed such tubes must be very favourable to the growth of the roots. Once a root-fibre has reached such a tube it can, following the direction of gravity, grow on in the moist air of the passage, without meeting with the least resistance, and it finds moist, loose, fertile earth in abundance.

The question whether all roots found in the under-soil have originally grown in the tubes of worms, cannot be answered with certainty. It is certain that the roots of some plants penetrate themselves in the sand, but not to great depths. M. Hensen is of opinion that the tap-roots, and in general such root-forms as grow with a thick point, can force a path for themselves, while the fine and flexible suction-roots have difficulty in obtaining a path into the depths other than what has been previously made for them. Roots of one year's growth especially can penetrate deep into the sub-soil, only where there are earth-worms.

A microscopical comparison of the earth deposited by the worm shows that it is like the two-year leaf-mould prepared by gardeners for the filling of flower-pots. Most of the plant-cells are destroyed; still there are present some cells and shreds of tissue, browned and friable, mixed with many sand grains and brown organic fragments. The chemical composition of the worm-earth shows much similarity to that of fertile humus ground. Its fertility, therefore, cannot be doubted, though direct experiments with it are wanting.

With regard to the numerical value of this action of the earthworm, the following observations by M. Hensen afford some information.

Two worms were put into a glass pot 1½ foot in diameter, which was filled with sand to the height of 1½ foot, and the surface covered with a layer of fallen leaves. The worms were quickly at work, and after 1½ month many leaves were down 3 inches deep into the tubes; the surface was completely covered with humus 1 cm. in height, and in the sand were numerous worm-tubes partly fresh, partly with a humus wall 3 mm. thick, partly quite filled with humus.

Counting, when an opportunity offered, the open worm-tubes in his garden, M. Hensen found at least nine in the square foot. In 0·15 square metres two or three worms were found in the deeper parts each weighing three grammes: thus in the hectare there would be 133,000 worms with 400 kilos. weight. The weight of the secretions of a worm in twenty-four hours was 0·5 grammes. While these numbers are valid only for the locality referred to, they yet give an idea of the action of this worm in all places where it occurs.

The assertion that the earthworms gnaw roots is not proved by any fact; roots gnawed by worms were never met with, and the contents of the intestine of the worms never included fresh pieces of plants. The experience of gardeners that the earthworm injures pot plants may be based on the uncovering or mechanical tearing of the roots.

"Let us take a retrospective glance," concludes the author, "over the action of the worm in relation to the fertility of the ground. It is clear that no new manure material can be produced by it, but it utilises that which is present in various ways. 1. It tends to effect a regular distribution of the natural manure material of fields, inasmuch as it removes leaves and loose plants from the force of the wind and fixes them. 2. It accelerates the transformation of this material. 3. It distributes it through the ground. 4. It opens up the undersoil for the plant roots. 5. It makes this fertile.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—The University Commissioners are at present occupied in taking evidence on the subject of University requirements. The Dean of Christ Church, the Master of Balliol, the Master of University, the Librarian of the Bodleian, Profs. Clifton, Bonamy Price, Bartholomew Price, Stubbs, and others have appeared, or are to appear during the present week, before the Commissioners.

Mr. Lazarus Fletcher, B.A., of Balliol, has been elected to the vacant Fellowship at University College. Mr. Fletcher obtained a first class in the School of Mathematics in 1875, a first class in that of Natural Science in 1876, and the senior mathematical scholarship in 1876.

It is proposed to found a high school for the City of Oxford, the mayor, aldermen, and citizens having long felt it a reproach that, being the site of one of the most ancient and famous of the Universities of Europe, it has been absolutely without any recognised grammar school available for the sons of the citizens.

LONDON.—Prof. W. K. Clifford, F.R.S., is at present delivering at University College a very interesting course of Lectures on Quaternions. The main object of the course is to bring the physical applications of quaternions as much as possible within the reach of mathematicians of moderate attainments.

A requisition is in course of signature to the chairman of Convocation of London University, Dr. Storrar, asking that an extraordinary meeting of that body may be convened for the purpose of considering and discussing the following resolutions, and for deciding with reference thereto in such manner as to Convocation may seem fit:—"That it being manifestly inexpedient that frequent application should be made to the Crown for new and additional charters, it is desirable that provision should be made in any such charter for all changes in the constitution of the University, either at the time urgent or likely to be soon required; and that it being probable that initiative measures will be shortly taken towards procuring such a new or additional charter, the following proposals require the serious consideration of Convocation and the Senate:—'(1) An enlargement of the powers directly exercised by Convocation; (2) An increase in the proportion of senators to be nominated or elected by Convocation, and the limitation of the tenure of office in the case of all senators to a term of years; (3) The encouragement of mature study and original research among the members of the University, by the establishment of University lectureships, of limited tenure, in different departments of learning and science; (4) The introduction into the constitution of the University of such modifications as may remove all reasonable ground of complaint, on the part of any of the affiliated colleges, with respect to the absence of means for expressing opinion and giving advice to the Senate on the examination regulations, and on the changes proposed to be made therein from time to time. And that a Special Committee of ten members of Convocation be appointed to consider the above-mentioned proposals, and to report thereon to Convocation as speedily as possible.'"

The Entrance Science Scholarships in St. Thomas's Hospital have been awarded this year as follows:—The Scholarship of 60*l*. to Mr. Wansborough Jones, B.A. Oxon., and B.Sc., London; and that of 40*l*. to Mr. A. E. Wells.

BRISTOL.—A well-printed and well-arranged Calendar of University College has been published. It extends to upwards of sixty pages, and contains all the information usually found in such publications, including full details as to the Medical School.

Dean Stanley's address on Education, at University College, on Saturday, attracted an audience of about 1,700 people, who listened with the closest attention.

SCIENTIFIC SERIALS

Kosmos, Part 2 (May) opens with an article by L. Overzier, on "Heredity" (Part 1), aiming at the discovery of the real cause of inheritance.—Prof. Jäger, commencing a series of articles on "The Origin of Organs," deals with the development of the eye, showing how the laws of optics and the properties of living substance mutually influence one another.—Hermann Müller, treating on "The Origin of Flowers," considers the first metasperm (or angiosperm) to have been diclinous and fertilised by the wind, that is, supposing the metasperms to have originated from a single stock.—W. O. Focke deals with "The Conception of Species in the Vegetable Kingdom," especially in relation to the genus Rubus. He shows how far the different species are from being of equivalent value and that the term variety has no definite significance. He exposes the futility of much botanical "research," owing to imperfection of methods and lack of comparative study; Darwin has few imitators. Such work requires an entire devotion of time and complete botanical gardens, for the multiplication of which the author calls.—A. Lang, on Lamarck and Darwin (I.), expounds Lamarck's conceptions of natural history.

Kosmos, Part 3 (June).—L. Overzier continues his discussion of heredity, reviewing Darwin's theory of pangenesis, Haeckel's perigenesis, and Jäger's chemical theory; he considers the latter to be of great value.—Carl du Prel, on the needed remodelling of the nebula hypothesis.—Prof. Jäger treats of the origin of the organ of hearing, tracing it from the simplest condition where spicules diffused through the entire protoplasmic body of an animal serve to gather up and conduct vibrations of sound. He brings forward the remarkable theory that in animals possessing nerve fibres, the organs of hearing is but a specialisation from the general tactile sense.—W. von Reichenau, on the colours of bird's eggs, makes the generalisation that birds having open nests have coloured eggs, while those with covered or concealed nests have white ones; further, that in open and ground nests the colour of the eggs has a protective object.—A. Dodel-Port, on the lower limit of sexuality in plants, gives an account of the sexual processes in *Ulothrix zonata*, but appears not to have heard of the researches of Dallinger and Drysdale on the monads. —A. Lang, on Lamarck and Darwin, expounds Lamarck's "hydro-geology."

SOCIETIES AND ACADEMIES

PARIS

Academy of Sciences, October 22.—M. Peligot in the chair. —The following papers were read:—M. Leverrier's tables of Uranus and Neptune, by M. Tresca.—On some applications of elliptic functions (continued), by M. Hermite.—*Résumé* of a history of matter (first article), by M. Chevreul. This is an extract from a work commenced about the end of last year, and occupying 418 pages of the *Memoires de l'Académie*, t. xxxix. A sketch of the principles of alchemy is given.—On one of the causes of red coloration of the leaves of *Cissus quinquefolia*, by M. Chevreul. This cause is sunlight. The green colour is retained in the leaves that are shaded by others.—On the order of appearance of the first vessels in the shoots of some Leguminosæ, by M. Trécul.—Modifications in the conditions of maxima of electro-magnets by the state of more or less complete saturation of their magnetic core, by M. Du Moncel. The law of proportionality of the attractive forces to the squares of the intensities of the current is true only within certain limits, and under certain conditions; and electro-magnets through which the current is interrupted at very short intervals, are (more or less) not subject to it. When the forces are proportional to (say) the cubes of the electric intensities, the helices must always be less resistant than the exterior circuit. In the case of multiplied interruptions, the resistance of electro-magnets must always be less the shorter the duration of closures of the current; and for this reason (also because of defective insulation and extra currents) telegraph electricians reduce considerably the resistance of electro-magnets applied to long circuits. Reverting to the question in the title, the thickness of the magnetising spiral may be increased in case of defective saturation of the magnetic core; becoming double the diameter of this if the force increases as the cube of the intensities.—Preparations of sulphide of carbon brought to the solid state by means of gelatine, by M. Cassius. 100 grammes of gelatine are dissolved in 1,000 grammes of water, and sulphide of carbon (25, 50, or 75 per cent.) is mixed at a temperature of 15° to 20°, and the mixture let cool. M. Cassius thinks the preparation might be useful in viticulture. The sulphide is liberated slowly, the time varying according to the proportion of sulphide absorbed.—Experiments on the formation of artificial ultramarine, by M. Plicque. He finds (in opposition to some German authors) that ultramarine does not contain nitrogen. Blue ultramarine, properly so called, is formed by an oxygenated compound of sulphur, and it is probable that this compound is fixed both by sodium and by aluminium.—On the catechines and their constitution, by M. Gautier.—On acid acetates, by M. Villiers. The increase of weight of some neutral acetates, dried and placed, in a summer month, under a bell jar with crystallisable acetic acid, was, in the case of acetate of soda, 404 per cent., or nearly six equivalents of acetic acid; acetate of potash, 264 per cent; of baryta, 179 per cent.; of lead, 134 per cent, &c. The solutions of neutral acetates in crystallisable acetic acid, have much less tension of vapour than that of acetic acid.—Researches on butylene and its derivatives, by M. Puchot.—Note on the cause of anthrax, by M. Klebs.—On the structure of the blood corpuscle, and the resistance of its envelope to the action of water, by MM. J. Bechamp and Baltus. The demonstration of the membrane (by action of soluble fecula) is here given in the cases of the frog, the ox, the pig, and the sheep. Water does not destroy the globules; it merely renders them invisible, and they may always be discovered with the aid of picrocarminate, even in extremely dilute media, and after several weeks of contact. The blood of sheep (like that of the hen in M. A. Bechamp's experiments) contains globules of more delicate structure than those of the other bloods examined.—Researches on the functions of leaves of the vine, by M. Macagno. Glucose and tartaric acid are formed preferably in the upper leaves of the fruit-bearing vine-branch; this production of sugar progresses with that of the grape, and is much reduced (even to disappearance) after the vintage. The green branches are conductors of glucose. These facts explain the evil of "pinching" or removing the tops of the grape-bearing branches, with too great zeal. Where there is an abundant production of grapes, a sufficient quantity of leaves should be left for preparation of the necessary glucose.—Reply to a recent note of M. Buys Ballot, on the division into time and into squares of maps of nautical meteorology, by M. Brault.

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THURSDAY, NOVEMBER 8, 1877

EXPLOSIONS IN MINES

AFTER the occurrence of great colliery explosions such as those which took place recently in Pemberton and Blantyre collieries, one very general and pertinent question presents itself to most minds, namely, What has been done or attempted with the view of preventing these disasters? It would be impossible to condense into an article like the present all that could be said in reply to this question, but I shall endeavour to give a brief outline of the subject, and point out, as well as I can, what appear to be its most prominent features.

Before the invention of the safety-lamp, the only means of guarding against the ignition of firedamp consisted in the employment of an apparatus called the "steel mill." The light obtained by its aid was feeble and uncertain, and Mr. Buddle informs us that explosions were known to have been caused by the sparks emitted by it. When Davy made his brilliant invention in 1815–16, the steel mill was laid aside for ever, and it was then imagined that colliery explosions had also become phenomena belonging to a past order of things. So confident, indeed, was Davy in the efficacy of his lamp, that he believed it could be safely employed for carrying on work in an explosive atmosphere; and he even went so far as to propose to make use of the firedamp itself as the light-giving combustible. These fond expectations were soon roughly dispelled, as one explosion followed another in an apparently unaccountable manner; and at length they were succeeded by a feeling of positive distrust, which found expression in the report of a select committee appointed, in 1835, to inquire into the nature of accidents in mines.

In 1850 Mr. Nicholas Wood made a series of experiments, which proved that when a Davy lamp is subjected to an explosive current travelling at the rate of eight or nine feet per second, the flame soon passes through the wire gauze. This was corroborated about 1867 by experiments conducted by a committee of the North of England Institute of Mining Engineers.

Lastly, in 1872–73, the writer demonstrated, also by experiment, that when a lamp burning in explosive gas is traversed by a violent sound-wave, such as that produced by a blasting shot, the same result follows, that is, ignition is communicated to the outside atmosphere. These are weak points inseparable from the construction of the ordinary Davy and Clanny lamps; but as it is now a thoroughly-recognised maxim that work must never, under any circumstances, be continued in an explosive atmosphere, they are seldom put to the test.

The atmosphere of part of a mine may, however, become explosive before the men can escape, either by the sudden influx of a quantity of firedamp from some natural cavity in which it had existed in a state of tension, or by a partial or total cessation of the ventilating current; and I propose in the next place to consider how such an event could produce an explosion supposing all the men to be provided with safety lamps.

This will happen, firstly, if the inflammable gas passes over a furnace at the bottom of the upcast; secondly, if it is carried against a Davy or Clanny lamp at a greater velocity than seven feet per second, or if the lamp is traversed by a sound-wave; thirdly, if a blasting shot is fired directly into it; and lastly, if it reaches a safety lamp that has been opened by one of the men.

The means that have been provided for guarding against these contingencies are as follow:—1. Furnaces have to a large extent been replaced by ventilating fans in fiery collieries. 2. Davy and Clanny lamps are still almost universally employed, and little importance seems to be attached to their known imperfections by those who are supposed to be capable of deciding the question. 3. Shot-firing having been found to originate many explosions, although probably in a manner not yet understood by most people, is now carried on under certain restrictions which it could easily be shown are still insufficient. 4. Much nonsense has been talked and written about miners opening their lamps. That they sometimes do so is beyond a doubt; but why should this state of matters be allowed to continue when it can be easily put an end to? The present flimsy pretence for a lock is not a necessity but a cheap convenience; and who is responsible if say a hundred men are killed through its being opened by one? Is there no responsibility attaching to the owners or the legislature for placing the lives of ninety-nine innocent men in danger? I think surely there is.

The influence of changes of weather on the internal condition of mines has been remarked since the remotest times, and for the last fifty or sixty years at least many have asserted that firedamp is more prevalent when the barometer is low than in the opposite case. The explanation of these phenomena is easily found by anyone who has an elementary knowledge of the physical properties of gases. On the other hand, when vigorous artificial means of ventilation are employed, and ordinary skill practised in distributing the air, the effects of changes of weather become much less perceptible.

Hence if a large proportion of explosions can be shown to occur simultaneously with, and therefore, presumably, in consequence of, those atmospheric changes that would tend to augment the amount of firedamp in the workings, there is a strong argument in favour of the supposition that they are preventible, and cannot therefore be considered as accidents in the true sense of the term. With this object in view diagrams have been made from time to time by Mr. R. H. Scott and myself, and also by one or two others, showing the connection that exists between the two classes of phenomena, and an examination of these is sufficient to convince unbiased persons that there is a striking coincidence between the explosions and the favourable atmospherical conditions. As might, perhaps, be expected, some persons engaged in mining either fail to see the connection, or possibly they do not understand it. Nevertheless a general rule was inserted in the Coal Mines' Regulation Act (1872) making it compulsory for mine-owners to place a barometer and thermometer at the entrance to every mine in the coal-measures.

It has always been difficult, and sometimes impossible, for mining men to give an adequate reason for the extent of great explosions, and more especially when it is known that, immediately beforehand, little or no inflam-

mable gas has been present in the workings. The reports of the Inspectors of Mines bear ample testimony to the correctness of this statement. It has therefore been customary in the absence of any other tenable hypothesis to assume that a large volume of firedamp had been suddenly poured into the workings. But these so-called "outbursts of gas" are entirely unknown in some localities in which great explosions have occurred; and therefore it is much to be marvelled at that some other explanation was not at least sought for.

In September, 1844, before the appointment of inspectors of mines, Lyell and Faraday were sent to Haswell Colliery by the Home Secretary to report on an explosion that had just taken place there. I am unable to quote from their official report, but I am firmly convinced that the following sentences taken from their article on the subject in the *Phil. Mag.* 1845, is the true key to a solution of the problem as regards both the mode of occurrence and means to be used for the purpose of avoiding great explosions in future; and, moreover, I believe that it has been highly unfortunate, both for the cause of the miner and his employer, that these two philosophers were not induced to prosecute their investigations further than they did.

The sentences referred to are these:—"In considering the extent of the fire for the moment of explosion, it is not to be supposed that firedamp is its only fuel; the coal-dust swept by the rush of wind and flame from the floor, roof, and walls of the works, would instantly take fire and burn, if there were oxygen enough in the air to support its combustion; and we found the dust adhering to the face of the pillars, props, and walls in the direction of, and on the side towards, the explosion, increasing gradually to a certain distance as we neared the place of ignition. This deposit was in some parts half an inch, and in others almost an inch thick;[1] it adhered together in a friable coked state; when examined with the glass it presented the fused round form of burnt coal-dust, and when examined chemically, and compared with the coal itself reduced to powder, was found deprived of the greater portion of the bitumen, and in some cases entirely destitute of it."

About three years ago M. Vital, Ingénieur des Mines in France, showed that a flame resembling that produced by a blasting shot which blows out the tamping is greatly lengthened in an atmosphere containing a cloud of coal-dust; and soon afterwards the writer ascertained that air containing a small proportion of fire-damp (less than one per cent. by volume) becomes highly inflammable when coal-dust is mixed with it.

These discoveries complete what Lyell and Faraday began, and show how explosions of any conceivable magnitude may occur in mines containing dry coal-dust. A blasting shot or a small local explosion of firedamp, or a naked light exposed when a cloud of coal-dust is raised up by a fall of roof in air already containing a little firedamp is sufficient to initiate them, and, when once they are begun, they become self-sustaining.

These remarkable facts are either not yet sufficiently well known or their true significance is not yet fully appreciated. In conclusion I may state that out of many hundred collieries known to me there is not, to my knowledge, a single damp one in which a great explosion has happened; while, on the other hand, there is a considerable number of very dry ones in which explosions causing the deaths of from 12 to 178 men at a time have occurred.

W. GALLOWAY

[1] In the reports of the Inspectors of Mines, human bodies, timber, and coal, are described as being *charred* or *burnt* where they are covered with this deposit.—W. G.

THE SUN'S PHOTOSPHERE

DR. JANSSEN has just made a communication to the French Academy of Sciences, which will be received with interest, not only by students of solar physics, but by all who follow the various triumphs achieved by modern scientific methods. It seems a paradox that discoveries can be made depending on the appearance of the sun's surface by observations in which the eye applied to the telescope is powerless; but this is the statement made by Dr. Janssen himself, and there is little doubt that he has proved his point.

Before we come to the discovery itself let us say a little concerning Dr. Janssen's recent endeavours. Among the six large telescopes which now form a part of the equipment of the new physical observatory recently established by the French government at Meudon, in the grounds of the princely Château, there is one to which Dr. Janssen has recently almost exclusively confined his attention. It is a photoheliograph giving images of the sun on an enormous scale—compared with which the pictures obtained by the Kew photoheliograph are, so to speak, pigmies, while the perfection of the image and the photographic processes employed are so exquisite, that the finest mottling on the sun's surface cannot be overlooked by those even who are profoundly ignorant of the interest which attaches to it.

This perfection and size of image have been obtained by Dr. Janssen by combining all that is best in the principles utilised in one direction by Mr. De la Rue, and in the other by Mr. Rutherfurd. In the Kew photoheliograph, which has done such noble work in its day that it will be regarded with the utmost veneration in the future, we have first a small object-glass corrected after the manner of photographic lenses, so as to make the so-called actinic and the visual rays coincide, and then the image formed by this lens is enlarged by a secondary magnifier constructed, though perhaps not too accurately, so as to make the actinic and visual rays unite in a second image on a prepared plate. Mr. Rutherfurd's beautiful photographs of the sun were obtained in a somewhat different manner. In his object-glass he discarded the visual rays altogether and brought only the blue rays to a focus, but when enlargements were made an ordinary photographic lens—that is, one in which the blue and yellow rays are made to coincide—was used.

Dr. Janssen uses a secondary magnifier, but with the assistance of M. Pragmowski he has taken care that both it and the object-glass are effective only for those rays which are most strongly photographic. Nor is this all; he has not feared largely to increase the apertures and focal length, so that the total length of the Kew instrument is less than one-third of that in operation in Paris.

The largely-increased aperture which Dr. Janssen has given to his instrument is a point of great importance. In the early days of solar photography the aperture used was small, in order to prevent over-exposure. It was

soon found that this small aperture, as was to be expected, produced poor images in consequence of the diffraction effects brought about by it. It then became a question of increasing the aperture while the exposure was reduced, and many forms of instantaneous shutters have been suggested with this end in view. With these, if a spring be used, the narrow slit [1] which flashes across the beam to pay the light out into the plate changes its velocity during its passage as the tension of the spring changes. Of this again Dr. Janssen has not been unmindful, and he has invented a contrivance in which the velocity is constant during the whole length of run of the shutter.

By these various arrangements the plates have now been produced at Meudon of fifteen inches diameter, showing details on the sun's surface of less than one second of arc.

So much for the *modus operandi*. Now for the branch of solar work which has been advanced.

It is more than fifteen years ago since the question of the minute structure of the solar photosphere was one of the questions of the day. The so-called "mottling" had long been observed. The keen-eyed Dawes had pointed out the thatch-like formation of the penumbra of spots, when one day Mr. Nasmyth announced the discovery that the whole sun was covered with objects resembling willow leaves, most strangely and effectively interlaced. I here quote from Sir John Herschel.[2]

"According to his observations, made with a very fine telescope of his own making, the bright surface of the sun consists of separate, insulated, individual objects or *things*, all nearly or exactly of one certain definite size and shape, which is more like that of a willow leaf, as he describes them, than anything else. These leaves or scales are not arranged in any order (as those on a butterfly's wing are), but lie crossing one another in all directions, like what are called spills in the game of spilikins; except at the borders of a spot, where they point for the most part inwards, towards the middle of the spot, presenting much the sort of appearance that the small leaves of some water-plants or sea-weeds do at the edge of a deep hole of clear water. The exceedingly definite shape of these objects; their exact similarity one to another; and the way in which they lie across and athwart each other (except where they form a sort of bridge across a spot, in which case they seem to affect a common direction, that, namely, of the bridge itself), all these characters seem quite repugnant to the notion of their being of a vaporous, a cloudy, or of a fluid nature. Nothing remains but to consider them as separate and independent sheets, flakes, or scales, having some sort of solidity. And these flakes, be they what they may, and whatever may be said about the dashing of meteoric stones into the sun's atmosphere, &c., are evidently the *immediate sources of the solar light and heat*, by whatever mechanism or whatever processes they may be enabled to develop, and as it were elaborate these elements from the bosom of the non-luminous fluid in which they appear to float. Looked at in this point of view, we cannot refuse to regard them as *organisms* of some peculiar and amazing kind . . . . "

Here, then, was a discovery with a vengeance! and absolute endorsement from the man above all others who had a right to express an opinion. Nevertheless, the organisms have since disappeared, and the work of many careful observers has established that the mottling on the sun's surface is due to dome-like masses, and that the "thatch" of the penumbra is due to these dome-like masses being drawn, either directly or in the manner of a cyclone, towards the centre of the spot. In fact the "pores" in the interval between the domes are so many small spots, while the faculæ are the higher levels of the cloudy surface. The fact that faculæ are so much better seen near the limb proves that the absorption of the solar atmosphere rapidly changes between the levels reached by the upper faculæ and the pores.

These masses are in all probability due to a rapid increase of pressure in the portion of the solar atmosphere occupied by the photosphere; we know, or think we know, that they are not due to reduction of temperature.

Thus much presumed we now come to Dr. Janssen's discovery.

An attentive examination of his photographs shows that the surface of the photosphere has not a constitution uniform in all its parts, *but that it is divided into a series of figures more or less distant from each other, and presenting a peculiar constitution.* These figures have contours more or less rounded, often very rectilinear, and generally resembling polygons. The dimensions of these figures are very variable; they attain sometimes a minute and more in diameter.

While in the interval of the figures of which we speak the grains are clear, distinctly terminated, although of very variable size, in the interior the grains are as if half effaced, stretched, strained; for the most part, indeed, they have disappeared to make way for trains of matter which have replaced the granulation. Everything indicates that in these spaces, as in the penumbræ of spots, the photospheric matter is submitted to violent movements which have confused the granular elements.

In an article recently contributed by Dr. Hunter and myself to the *Nineteenth Century*,[1] the following passage occurs:—

"The spots may be taken as a rough index of solar energy, just as the rainfall may be taken as a convenient indication of terrestrial climate. *They are an index but not a measure* of solar activity; and their absence indicates a reduction, not the cessation, of the sun's energy. *Whether this reduction means one in a hundred or one in a thousand we do not know.*"

With the same idea in his mind Dr. Janssen points out that this fact throws light upon the forms of solar activity, and shows that that activity, in the photosphere, is always very great, although no spot appears on the surface.

We have already referred to the paradox that the sun's appearance can now be best studied without the eye applied to the telescope. This is what Dr. Janssen says on that point.

The photospheric network cannot be discovered by optical methods applied directly to the sun. In fact, to ascertain it from the proofs, it is necessary to employ glasses which enable us to embrace a certain extent of the photographic image. Then if the magnifying power is quite suitable, if the proof is quite pure, and especially if

[1] I have recently been making some experiments with a view of getting rid of the narrow aperture in general use, as it has appeared to me that the diffraction effects produced by it must be as injurious to definition as those due to a small object-glass. I have found that a circular aperture, allowing the whole beam to be flashed on the plate in conjunction with a plate of optically pure yellow glass nearly in contact with the photographic plate can be used without over-exposure.

[2] "Familiar Lectures," p. 87.

[1] "Sun-spots and Famines," *Nineteenth Century*, November, 1877, p. 584.

it has received rigorously the proper exposure, it will be seen that the granulation has not everywhere the same distinctness, that the parts consisting of well-formed grains appear as currents which circulate so as to circumscribe spaces where the phenomena present the aspect we have described. But to establish this fact, it is necessary to embrace a considerable portion of the solar disc, and it is this which it is impossible to realise when we look at the sun in a very powerful instrument the field of which is, by the very fact of its power, very small. In these conditions we may very easily conclude that there exist portions where the granulation ceases to be distinct or even visible ; but it is impossible to suppose that this fact is connected with a general system.

We have written enough to show that when the daily history of the sun comes to be recorded another method and another point of view have now been added as the first fruits of Dr. Janssen's labours in his new observatory.

J. NORMAN LOCKYER

FOWNES' "MANUAL OF CHEMISTRY"

Fownes' Manual of Chemistry. Vol. II. Chemistry of Carbon Compounds, or Organic Chemistry. Twelfth Edition. By H. Watts, B.A., F.R.S. (London: Churchill, 1877.)

ORGANIC chemistry is now progressing with such rapid strides, that a work on this subject becomes antiquated, at least in some parts, in the course of a few years. A new edition of a well known and favourite book must therefore be most welcome to students of this branch of chemical science, and more so when edited by a man whom we may justly call "the English Gmelin."

The old familiar, bulky Fownes has now been divided into two handy volumes, enabling the editor to devote the same space to the carbon compounds as to inorganic chemistry.

The arrangement of the subject is in principle almost the same as in the last edition ; organic compounds being divided into hydrocarbons, alcohols, ethers, amido-compounds, organo-metallic bodies, acids, &c., the compounds of each group being arranged in homologous series.

Physiological chemistry is omitted, and this must be considered as an improvement, as that branch of chemical science now requires special treatment in a separate work.

The name of the author is a sufficient guarantee for the soundness of the knowledge which this book imparts, and we hope to see it soon in the hands of numerous students who will find it a most useful and trustworthy guide, embracing as it does the most important recent researches. The book is singularly free from misprints, and the few which we have found can be easily corrected by a student who is accustomed to think for himself.

As a reviewer is expected to point out any faults, we will do so, but "sine iræ et studia," and only for the benefit of the students who will largely use this work.

Thus we miss an account of the normal sulphuric ethers, which are found by the action of sulphuryl chloride, or oxychloride on the alcohols and phenols. Perhaps these parts were written before the researches we allude to were published, and the same may be the case with phenyl-sulphuric acid, and its homologues, compounds which possess such interest both for the chemist and physiologist. To lactide, the author still assigns the old formula $C_3H_4O_2$, although Henry has proved, by determining its vapour density, that its molecular formula is $C_6H_8O_4$. On page 285 we find a statement which might lead a beginner in practical work to disappointment, it is there said that "crude acetyl chloride is purified by heating it with water and dilute soda solution." "Quandoque bonus dormitat Homerus."

We were much pleased to find that Mr. Watts has given particular attention to the study of isomerism, especially among the derivatives of benzene, and he justly says in the preface : "This part of the subject is here presented in a form in which it has not yet appeared in any English publication, except in the *Journal* of the Chemical Society."

Speaking of the disubstitution products of benzene, the following definition is given : "A di-derivative of benzene is para-, ortho-, or meta-, according as it can give rise to, or be formed from, one, two, or three tri-derivatives. This definition is, however, incomplete, and only holds good if in the di-derivative the substituting elements or radicals are the same. For it is easily seen that, to take the most simple case, a para-compound containing two different groups such as paranitrobromobenzene can give rise to or be formed from two different amidonitrobromobenzenes. The oversight is, however, a matter of small importance, and an attentive student will not be led astray by it.

The theory of structure or position which Mr. Watts treats so fully has been lately attacked by eminent chemists who seem to overlook or forget the great impulse which this theory has given to the progress of organic chemistry. The "modern chemists," as they sneeringly have been called, know well enough that the structural formulæ which they use do not pretend to give a picture of the real position of atoms in space, and do not mean more than the parallelogram of forces in mechanics, *i.e.*, they only express the manner in which the different forces of the atoms attract each other. They fully understand that their present theory, with the progress of science will have to undergo many modifications, and it is not a dogma, but will stand or fall on its own merits.

The opponents of the modern school remind us of the last followers of the phlogistic theory who got hold of any fact which the antiphlogistonists were not able to explain as a proof that the latter were in the wrong. We can easily imagine how pleased Priestley was when it was found that when heating certain metallic calces with charcoal an inflammable air was formed, whereas, according to Lavoisier's school, only carbonic acid could be produced. Just in the same way the opponents of the structural theory point out that the existence of four lactic acids is incompatible with it ; and Mr. Watts himself, although a strong adherent of the theory of structure, shirks the discussion of this point, and rusticates one of the four in a foot-note, in which he expresses his doubts as to its existence.

The recent researches of Wislicenus, however, hardly leave any doubt that four such acids exist. We must confess that we are not able to explain the difference between hydracrylic acid and ethenelactic acid, and quite agree with Mr. Watts that Wislicenus' explanation of the

cause of their isomerism is improvable and far-fetched. But there exist other isomeric compounds which, like these two acids, have apparently the same chemical constitution, and in some of these cases it has lately been shown that the bodies are not chemical isomerides but physical isomorphides, or differ from each other in exactly the same way as calcite differs from arragonite. We have not the least doubt that the cause of the isomerism of the lactic acids will, at no distant time, also find a satisfactory explanation, because we are convinced that organic chemistry is working in the right direction. Time will show whether we prophesy truly or not.

OUR BOOK SHELF

Transcaucasia and Ararat; being Notes of a Vacation Tour in the Autumn of 1876. By James Bryce. (London: Macmillan and Co., 1877.)

ALTHOUGH in this narrative Prof. Bryce takes the reader over pretty well-known ground, about parts of which, at least, much has been written, still even the best-informed readers will read his book with pleasure and profit. Prof. Bryce used his own eyes, and as he is a good and independent observer, there is an unusual freshness about his narrative. He journeyed down the Volga, crossed the southern steppe and the Caucasus to Ararat, which he ascended, thence to the shore of the Black Sea, sailing along the coast to Constantinople. Nijni Novgorod Fair, he thinks, has been much over-estimated in some respects, and he has a good word to say of the recently much-abused Cossack. Prof. Bryce is a good geologist, and his work abounds with interesting notes on the geology as well as the flora of the regions which he traversed. Perhaps the most interesting chapter in his book is that in which he describes his ascent of Mount Ararat. In a previous chapter he has collected much valuable information concerning the mountain, the legends connected with it, its geology, volcanic phenomena, meteorology, vegetation, and animals. Prof. Bryce, with a companion, six Cossack soldiers, and an interpreter, set out from Aralyk, a little to the north of the mountain, at 8 A.M., on September 11 last year, to attempt the ascent. About noon they were fairly on the side of Ararat, and at about 6,000 feet came upon a small Kurd encampment, some of the Kurds, with their oxen, being induced to act as baggage-bearers. At the well of Sardarbulakh they camped late in the afternoon, about 7,500 feet above the sea. About one A.M. they started again, thirteen in all, but as they proceeded, with many vexatious halts, the Cossacks dropped off one by one, and at last, at about 12,000 feet, Prof. Bryce resolved to take what he wanted in the way of food, and start at his own pace. Two Cossacks and a Kurd accompanied him to the height of about 13,600 feet, when they too dropped off, and Prof. Bryce resolved to accomplish the remainder of the 17,000 feet alone, a hazardous undertaking even for a trained Alpinist. Partly up a rocky slope which seems to extend considerably beyond the snow-line, and partly over the soft snow itself, and enveloped much of the time in cloud, Prof. Bryce continued his solitary and fatiguing climb, until about half-past two P.M., he became convinced that he was really on the top of Ararat, at least one of the tops, for there are two, one about thirty feet higher than the other, and he did not descend until he had set his feet on both. There were difficulties and dangers both in the ascent and descent, though they do not seem to be nearly so great, judging from Prof. Bryce's description, as those which attend the ascent of a moderate Alpine summit. Prof. Bryce reached his companions again in safety. Notwithstanding he had to make all haste to reach the summit, he had time to make several interesting notes of what he saw by the way, the evidences of volcanic action particularly attracting his attention. To show the superstitious awe with which the sacred summit is regarded in the region around, Prof. Bryce tells that when the Archimandrite of Etchmiadzin was told that the Englishman had ascended to the top of "Massis," the venerable man replied, smiling sweetly, "No, that cannot be. No one has ever been there. It is impossible." Prof. Bryce's is the sixth known ascent of Ararat, the first having been made in 1829 by Dr. Frederick Parrot, a Russo-German professor in Dorpat University.

Thermodynamics. By R. Wormell. (The London Science Class-books. Elementary Series. Longmans, 1877.)

THIS work is one of the earliest published of a series "adapted for school purposes," and "composed with special reference to use in school teaching," as we are told in the general preface.

We feel very strongly that no good can come of the introduction of such subjects as the dynamical theory of heat into school-teaching. That an average school-boy can be taught the elements of such subjects as astronomy, botany, and natural history, and that he will to a certain extent profit by such teaching, may probably be true; but only in so far as his powers of observation are concerned. We believe that it is a complete mistake in practical education to try to carry the process farther than the elements, even in the case of the comparatively easy subjects just named.

Some elementary experimental facts connected with heat might, no doubt, be added to the list. But it is simply the work of the crammer to stuff a school-boy's head with such utterly unassimilable materials as reversible engines, absolute temperature, and the kinetic theory of gases. This is education run mad.

This obvious consideration decides at once our opinion as to the value of the work before us. It is beyond the intelligence of schoolboys, and in the hopeless endeavour to sink it to *their* level it has been deprived of much that might have made it a serviceable work for more mature minds.

After what we have said, it would be superfluous to criticise the book minutely, for nearly all our objections would be mere repetitions in part of the first and general one. We note, however, a want of strictness, or at least of completeness, in some of the mathematical proofs. The first example we meet with may serve as a type. Thus (p. 4) it is assumed, without any attempt at explanation, in fact without a word to warn the reader that a distinct step has been taken, that in uniformly accelerated motion the mean velocity during any period is half the sum of the initial and final velocities—a truth, and a very important one, but most certainly not self-evident to the average schoolboy.

Simple Lessons for Home Use. (London: E. Stanford, 1877.)

THESE simple lessons are intended for younger children than those for whom the primers published by Messrs. Macmillan have been written, and they appear admirably adapted for the purpose they have in view. Mr. W. E. Forster, in his recent speech at Huddersfield, referred to the importance of teaching the elements of science in primary schools by means of appropriate reading books. The little books before us, so far as they go, meet the wish expressed by Mr. Forster. The print is clear, the language on the whole simple, and the price (threepence) places them within the reach of the humblest. Perhaps there is a little too great a tendency to moralise in parts of the otherwise capital little lessons on birds and money. The author of the last-named—the Rev. T. E. Crallan—tells in a simple and interesting way

how money grows, and writes for younger minds than does the Rev. G. Henslow, who contributes lessons on flowers, where too many technical terms are, we think, introduced, especially in the first chapter. Miss Fenwick Miller's lessons on the human body, and on ventilation, are excellent, and so are Mr. Philip Bevan's on food, and Dr. Mann's on the weather. Altogether, we congratulate the publisher on the subjects selected, and the authors he has chosen: no doubt the remainder of the lessons that are to be issued will confirm the high opinion we have formed of those already before us. W. F. B.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Appunn and Koenig.—Beats in Confined Air

In my letter published in NATURE (vol. xvi. p. 227), I stated that I should re-examine the question of the discrepancy between Appunn and Koenig, and inform you of the result. During the whole month of September I was engaged in very carefully counting and recounting Appunn's tonometer in the South Kensington Museum, the reeds of which had got a little out of order, a circumstance which did not interfere with the ascertainment of pitch, but disposed at once of any errors in Appunn's pendulum. I employed one of Webster's ship chronometers, which was rated to lose one second daily, and counted each set of beats repeatedly through one or two minutes. I ascertained by this means that the objections made by Koenig on the score of false pendulums and false counting were entirely groundless, and that the former determinations of the relative pitch of Koenig's forks and Appunn's reeds, made by Dr. Preyer and myself, were practically correct.

But as Lord Rayleigh pointed out in NATURE (vol. xvii. p. 12) the practical agreement of the results obtained by Professors Mayer and MacLeod, and by his own new method there described, with Koenig's, serves to show that there is a physical phenomenon to be accounted for. Mr. Bosanquet had drawn my attention to the subject several months ago, and my own experiments on the beating of disturbed consonances had led me to the same conclusion. Accordingly I had devised a series of experiments for ascertaining the fact, the nature of which I lately communicated to Lord Rayleigh; but as they required the use of two tonometers excited by separate bellows, there were difficulties in the way of making them, which I did not overcome till this week. To-day I made the first of these experiments, lasting four hours or more, and ascertained—

1. That the beats of the harmonium reeds in Appunn's tonometer *are* affected by taking place in a confined space of air.
2. That they are *accelerated*, and
3. That the acceleration, being roughly about one per cent., will probably, when completely ascertained, account for the discrepancy observed.

Details have been sent privately to Lord Rayleigh; they are too incomplete for publication. The experiments will require many weeks to complete with the necessary accuracy. But in the meantime I hasten to communicate an important acoustical fact which may bear upon many other phenomena besides the ascertainment of absolute pitch. ALEXANDER J. ELLIS

25, Argyll Road, Kensington, November 3

The Radiometer and its Lessons

As I now learn for the first time what are the grounds on which Prof G. C. Foster based his inculpation of me, I may ask for a *very* few last words. I fully admit that in giving a sketch of the history of the Radiometer, I *intended* to attribute to Mr. Crookes that he had in the first instance put a wrong interpretation upon his own results; because I believed that this was a simple fact, well known to everybody who had followed the history of the inquiry. And Prof. Carey Foster has not called in question the correctness of my statement of the general impression which prevailed among scientific men, alike when Mr. Crookes first exhibited his radiometer at the *soirée* of the Royal Society, and when its phenomena were discussed at the subsequent meeting. Having followed that discussion with the greatest interest, I cannot now recall one word that was not in harmony with the "direct impact" doctrine, or that suggested the idea of "heat reaction" through residual gas. If the question had been then asked, whether the rotation would continue to take place in an *open* vacuum (were such possible), or in a *perfect* vacuum,—so as to eliminate all "reaction," through residual gas, between the vanes and the containing flask,—I believe that the general, if not the unanimous, verdict would have been in the affirmative. Certainly I heard nothing from Mr. Crookes on the other side, he having previously spoken of the dependence of the "Repulsion resulting from Radiation on the presence of residual gas as 'impossible to conceive.'"

It is clear, then, that in referring to this then prevalent view, I no more wished to put Mr. Crookes in the wrong, than I wished to put in the wrong my very excellent friends among the other eminent Physicists who shared it; the special purpose of this part of my paper being to bring out, as strongly as I could, *the thoroughly scientific and philosophical method in which Mr. Crookes afterwards worked himself right.* If this is not expressed in as much detail as Prof. G. C. Foster would have approved, it surely afforded no adequate ground for his going out of his way to charge me with having "depreciated Mr. Crookes's merits." Yet this is the *only* ground that I can find in the whole of Prof. Carey Foster's statement, for what I could not but regard as a very grave imputation.

On Mr. Crookes's reply I shall make but a single remark, with reference to his perfectly correct citation of the latter part of my conversation with him, on the occasion of his receiving the Royal Medal. If I had not found, after the publication of my Lectures (in which I said nothing but what was respectful to Mr. Crookes), that he had himself been "digging up the hatchet" which I was quite disposed to keep buried, by giving his public attestation to the "spiritualistic" genuineness of what had been proved to be a most barefaced imposture, I should not have again brought his name into the controversy. But I felt that his greatly increased reputation as a Scientific man would do an increasing injury to what I honestly believed to be the cause of reason and common sense, not only in this country but still more in the United States.

Since the death of Prof. Hare, not a single scientific man of note (so far as I am aware) has there joined the Spiritualistic ranks; but the names of the "eminent British scientists," Messrs. Crookes and Wallace, are a "tower of strength" to the various orders of "mediums"—rapping mediums, writing mediums, drawing mediums, materialising mediums, test mediums, photographic mediums, trance mediums, healing mediums, and the like—*whose names form many columns* of the "Boston Trades' Directory." And the now notorious impostor, Eva Fay, has been able to appeal to the "endorsement" given to her by the "scientific tests" applied to her by "Prof. Crookes and other Fellows of the Royal Society," which had been published (I now find) by Mr. Crookes himself in the *Spiritualist* in March, 1875. Within two months of that date, as Mr. Maskelyne has publicly stated, an offer was made him (I have myself seen copies of the letters) by Eva Fay's manager, that for an adequate sum of money the "medium" should expose the whole affair, scientific tests and all, "*complicating at least six big guns, the F.R.S. people*," as she was not properly supported by the Spiritualists.

I have therefore felt it incumbent on me to show that in dealing with this subject Messrs. Crookes and Wallace have followed methods which are thoroughly *un*-scientific; and have been led by their "prepossession" to accept with implicit faith a number of statements which ought to be rejected as completely untrustworthy.

My call to take such a part—which I would most gladly lay aside for the scientific investigations which afford me the purest and most undisturbed enjoyment—seems to me the same as is made upon every member of the Profession to which I have the honour to belong, that he should do his utmost to cure or to mitigate *bodily* disease. The training I originally received, and the theoretical and experimental studies of forty years, have given me what I honestly believe (whether rightly or wrongly) to be a rather unusual power of dealing with this subject. Since the appearance of my Lectures I have received a large number of public assurances that they are doing good service in preventing the spread of a noxious *mental* epidemic in this country; and I have been privately informed of several instances, in which persons who had been "bitten" by this malady, have owed their recovery to my treatment. Looking to the danger which threatens us from

the United States, of an importation of a real spiritualistic *mania*, far more injurious to our *mental* welfare, than that of the Colorado beetle will be to our *material* interests, I should be untrue to my own convictions of duty if I did not do what in me lies to prevent it. That I do not take an exaggerated view of the danger, will be obvious to any reader of Mr. Home's book. I know too well that I thus expose myself to severe obloquy, which (as I am not peculiarly thick-skinned) will be very unpleasant to myself, and unfortunately still more so to some who are nearly connected with me. But I am content to brave all, if I can believe that my *exposé* will be of the least service either to individuals or to society at large. W. B. CARPENTER

THE high scientific position which Prof. Foster holds, as well as the decided manner in which his letter was written, must lead the otherwise unbiassed reader to the conclusion that not only has a satisfactory explanation of the action in question been found and generally adopted, but that this explanation turns upon certain considerations, and particularly on the mean length of the path of the gaseous molecules as influenced by the degree of rarefaction.

I feel my position, therefore, particularly unfortunate in having, for the sake of truth, to show that the explanation which Prof. Foster has adopted, and supposes others to have adopted, is, if judged by the statements in his letter, inconsistent with well-established laws.

Prof. Foster gives me credit for having originated the fundamental idea of the explanation, but states that my "explanation was theoretically incomplete; in particular it did not show clearly why so high a degree of rarefaction should be necessary for the production of the phenomenon in question;" and then he proceeds to explain how this asserted deficiency was supplied by other thinkers, who showed that "the increase, resulting from rarefaction, in the mean length of the path of the gaseous molecules, would favour the action."

It is this supposed completion of my explanation that is erroneous. It is contrary to the law of the diffusion of heat in gases that "the increase, resulting from rarefaction, in the mean length of the path of the gaseous molecules would favour the action," and so far from supplying any deficiency in my explanation it is incompatible with it. The only result from such an increase is to diminish the action—a result which rises into importance only when the rarefaction is carried so far that the mean length of the path of a molecule becomes comparable with the dimensions of the inclosing vessel.

In my first paper I gave a definite proof, which has nowhere been questioned, that according to the kinetic theory the force arising from the communication of heat from a surface to adjacent gas of any particular kind depends only on one thing, the rate at which heat is communicated, and to this it is proportional. If therefore the increased rarefaction increased the force it must increase the rate at which heat is communicated, but according to the law established by Prof. Maxwell the rate at which heat is communicated is independent of the density of the gas, whence it follows that the increase in the mean length of the path of the gaseous molecules, resulting from rarefaction, cannot favour the action which remains approximately constant until the gas becomes so rare that the law of diffusion no longer holds, after which it may easily be shown the communication of heat, and hence the action in question, diminishes but never increases.

The fact that in the radiometer the force caused by the communication of heat only causes motion when the surrounding gas becomes extremely rare is, as I pointed out in my first papers, fully explained by the action of what I have called convection currents, which action depends on the weight and density of the gas. The gas adjacent to the hot surface is hotter than that which is more remote, and hence the former rises forming an ascending column, to supply which the gas is drawn in laterally on all sides, and tends to carry the surface forward with it. With the same difference of temperature and surrounding circumstances the speed of these convection currents is the same whatever may be the density of the gas, and hence the force which they exert on the surface is proportional to the density of the gas.

This force is opposite in direction to that arising from the communication of heat to the gas, and since the former diminishes with the density while the latter is constant, there must be some density for which they balance, and below which the constant force will predominate, while above this point the convection currents will carry the surface with them. The fact that, starting from low densities, the motion of the vanes in the radiometer does not only diminish as the density increases, but is actually reversed at higher densities, requires explanation, and no other than this has yet been offered.

I have gone into the subject at considerable length, as I felt bound, when venturing to differ from so high an authority as Prof. Foster, to state my reasons. There is, however, nothing in what I have said here which I have not said elsewhere, in the same or other words; and however incomplete in theory the explanation given in my first papers may be, I can only say that it included all the facts known to me at the time these were written; it has led me to predict many of the experimental results which have since been obtained, and I have not been able to find one fact with which it is not in accordance, nor has it been, so far as I am aware, controverted in any particular.

OSBORNE REYNOLDS

Potential Energy

I HAVE reason to believe that the "grievous error" with which I charged "John O'Toole" in his reference to the clock is not meant by him to be his own view of the matter at all, but merely a legitimate deduction from the confused and inconsistent language of "the doctors." Such an erroneous view on his part is, indeed, obviously out of harmony with the extensive knowledge of the subject of energy displayed by him in letters which, without doubt, will convince "the doctors" of the necessity of adopting consistent and strictly logical phraseology.

G. M. MINCHIN

Royal Indian Engineering College, Cooper's Hill

Effects of Urticating Organs of Millepora on the Tongue

AN article by Mr. Moseley, in NATURE (vol. xvi. p. 475), reminds me of an experiment I made some years ago in Florida. In collecting corals on the reefs, I had of course become familiar with the disagreeable, though not very painful, effects of contact of the hands with Millepora. But the vulgar names of Pepper-coral or Sea ginger induced me to try the effect on the tongue, to find out how far the taste resembled those condiments. I accordingly broke off a fresh piece and applied it to the tongue. Instantly a most severe pain shot, not only through that organ, but also through the jaws and teeth. The whole course of the dental nerves and their ramifications into every single tooth could be distinctly and painfully felt. I can compare the sensation to nothing better than to the application of the poles of a pretty strong galvanic battery. The pain remained severe for about half an hour, then became duller, leaving a sensation still perceptible five or six hours later. The whole impression was much too violent to allow the distinction of any particular taste.

Such an experiment made with Physalia might be positively dangerous, considering the much more powerful urticating effects of its polyps. Indeed, a friend of mine once related to me that when a boy he had come in contact with one of the long tentacles of a Physalia, when bathing, and had to be carried out of the water almost fainting. L. F. POURTALES

Cambridge, Mass., October 22

Drowned by a Devil Fish

THE following account of the destruction of a human being by a cuttle fish at Victoria, in Vancouver Island, has all the appearance of authenticity about it. It occurs in the *Weekly Oregonian* of October 6, 1877. The *Oregonian* is the principal paper of Oregon, and is published at Portland.

The insertion of the account in NATURE may lead to further information on the subject. I know of no other authentic instance of the kind.

An account of the habits of the huge octopus of the Vancouver Island Sounds and also of the Indian method of hunting and killing the beasts for food is to be found in John Keast Lord's "Naturalist in Vancouver Island and British Columbia," vol. i. p. 192. Mr. Lord measured specimens which had arms five feet in length, with a thickness at their base as great as his wrist, and he once collected a detached sucker of one of these cephalopods as large as an egg cup in mistake for a huge actinia.

"BRITISH COLUMBIA

"*Drowned by a Devil Fish*

"VICTORIA, September 27.—An Indian woman while bathing was pulled beneath the surface of the water by an octopus or devil fish and drowned. The body was discovered the following day in the bottom of the bay in the embrace of the monster. Indians dived down and with their knives severed the tentacles of the octopus and rescued the body. This is the first recorded instance of death from such a cause in this locality, but there have been several narrow escapes."

Exeter College, Oxford H. N. MOSELEY

The Earthworm in Relation to the Fertility of the Soil

IN NATURE, vol. xvii., p. 18, there is an account under the above heading of M. Hensen's investigations of this subject, to which I wish to add a note. He says the assertion that the earth-worms gnaw roots is not proved by any fact ; roots gnawed by worms were never met with by him, and the contents of the intestines of the worms never included fresh pieces of plants. The experience of gardeners that the earth-worm injures pot plants may be based on the uncovering or mechanical tearing of the roots.

I should have thought that the universal experience of gardeners is that earth-worms never eat vegetable matter until it has decayed, and that their instinct leads them to draw the points of leaves as far as they can into their tubes for the purpose of setting up the decaying process, and likewise to sever the roots of pot plants with the same object. I can hardly understand how earth-worms have any mechanical means of severing the roots of plants except by gnawing.

But there is an omission in M. Hensen's account of the fertilisation of the subsoil by earth-worms which surprises me. He mentions but two ways in which this is effected, viz., by the opening of passages for the roots into deeper parts, and by the lining of these passages with humus.

I thought it was a well-known fact that worms, by means of their "casts," effect a complete *renversement* of the soil of meadow land down to a certain depth in the course of a few years. But whether well-known or not I met with a demonstration of this important fact in 1857. When putting down a considerable extent of iron fencing in the alluvial meadows near my house (consequent upon an exchange of land) I had occasion to cut a ditch two or three feet deep, and when the workmen had finished the ditch—a quarter of a mile long in all—I was astonished to see in one portion, of about sixty yards in length, a distinct and very even narrow line of coal-ashes mixed with small coal in the clean cut surface of the fine loam of the ditch face, perfectly parallel with the top sward. It immediately occurred to me that this was the work of the earth-worms, and upon inquiry I found that the farmer, who had occupied this land for many years, remembered having once, and only once, carted out some coal-ashes and spread it at this spot not many years before. I forget the exact number of years, but I believe it was about eighteeen. I have a distinct recollection, however, that the depth of the line of coal-ashes below the surface was at least seven inches, and that this seemed to confirm the general belief that the depth to which the earth-worm usually burrows is about that amount. I may add that the colour of the loam above the line of coal-ashes was decidedly darker than of that below.

HENRY COOPER KEY

Stretton Rectory, Hereford, November 2

IN NATURE, vol. xvii. p. 18, some details are given of observations made by M. Hensen on the relation of the earthworm to the fertility of the ground. He has observed, as everyone must have observed, that the earthworm during night draws into its tube or hole the loose leaves and fibres which may be lying about. But this operation of the earthworm has a significance in relation to the vegetable world of even a profounder kind than that of the fertilisation of the soil. Some months ago, in searching for young ash plants with three cotyledons, I found that in a great many cases the samara or seed of the ash had been drawn into a worm's hole, and had there found moisture and other essential conditions of growth ; while the same seeds lying dry upon the surface had not germinated. There can thus be no doubt that many seeds of all kinds are drawn under the surface of the ground, or covered by the earth thrown up by worms. They are thus preserved from birds and various enemies, and are placed in the proper position for germination. The dead plant is perpetuated from its fallen panicle by the earthworm. An ash tree, or a whole forest of ash trees, may have been planted by earthworms.

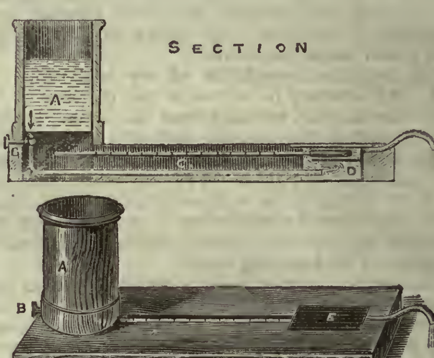
North Kinmundy, November 5 A. STEPHEN WILSON

M. Alluard's Condensing Hygrometer

THE notice of the above instrument in last week's NATURE (p. 14) is an excellent illustration of the necessity for increased communication between the scientific men of all countries. The labour which is at present wasted by repeating what has been done before is enormous, and until international intercommunication is improved it must be so.

I quite agree with you in your appreciation of M. Alluard's hygrometer, but I think it is desirable to state that it is not the first in which "the part on which the deposit of dew is to be observed is a plane well-polished face A, of silver or gilt brass." The annexed engravings represent the form of plane-faced hygrometer invented by Mr. G. Dines, F.M.S., described by him in the *Meteorological Magazine* for October, 1871, and exhibited at the Brighton Meeting of the British Association, 1872.

The action is extremely simple ; no ether is required nor any aspirator. Water colder than the dew point is the only requisite —it is poured into the reservoir A, passes through the regulating-tap B into the chamber D ; it is, by the black diaphragm, thrown past the bulb of the thermometer C, and then allowed to escape. The cooled plane surface E of silver or black glass, is excessively thin, and the space between it and the thermometer-bulb is wholly occupied by the effluent water, so that the great essential



of all hygrometers, a true indication of the temperature of the cooled surface, seems to be reached. The plate E can be kept within 0°·2 or 0°·3 for a length of time by adjusting the screw B, and as the condensation usually takes an elliptical form over the thermometer-bulb, and in the middle of E, the advantage of an adjacent bright surface is usually attained. I am, however, not sure that M. Alluard's surrounding plate might not be a convenience, although for the reason above given I have not found it necessary. G. J. SYMONS

62, Camden Square, N.W., November 2

Optical Spectroscopy of the Red End of the Solar Spectrum

NATURE, dated August 2 (vol. xvi. p. 264), containing Prof. Piazzi Smyth's communication on "Optical Spectroscopy of the Red End of the Solar Spectrum," reached me on the 21st ult., when I had no leisure to avail myself of the outgoing mail and reply immediately to the subject of his last paragraph. Inquiry is there made of "anyone" (besides the Royal Society), in association more or less with my name, whether *more recent particulars* have been published, of the spectrum in question, than "those (*i.e.* my) Indian observations," "printed in the *Philosophical Transactions* so long ago as 1874" (*i.e.* 1875).

2. The Astronomer-Royal for Scotland is presumably in a better position to reply for "any one," than myself, located in latitude N. 30°, longitude E. 78° ; and so far as the inquiry relates to the Royal Society, his penultimate paragraph in itself furnishes the information sought, because the Society's publica-

tion prominently alluded to by himself *is the last* publication. As respects myself, I have printed no further particulars in addition to those which the Professor dismisses, briefly for the present, with the announcement of having discovered, "total contradictions" to certain "conspicuous features."

3. It is necessary to point out, that the designation for my observations adopted by the Professor of "the Royal Society's and Mr. Hennessey's high-sun series" suggests existence of the *divided* responsibility which is plainly disavowed in the "Advertisement" to the *Philosophical Transactions*, 1875, Part I., and elsewhere; for the professor can hardly intend that two separate and independent high-sun series taken on the Himalaya Mountains, one by the Royal Society, and the other by myself, have appeared in the *Transactions.*

4. I shall look forward with interest to the perusal of Prof. Piazzi Smyth's promised *complete* account of his sun-high observations at Lisbon; meanwhile I may be pardoned for my inability to follow his prompt and brief announcement of "total contradictions," written while yet on his return voyage.

J. B. N. HENNESSEY

N.W. Provinces, India, Dehra Doon, October 3

Singing Mice

PERHAPS the following account of a singing mouse may be of interest to your readers:—

Last winter we occupied the rooms we now do at Menton. Early in February we heard as we thought the song of a canary, and fancied it was outside our balcony; however we soon discovered that the singing was in our *salon*, and that the songster was a mouse; at that time the weather was rather cold, and we had a little fire, and the mouse spent most of the day under the fender, where we kept it supplied with bits of biscuit; in a few days it became quite tame, and would come on the hearth in an evening and sing for several hours, sometimes it would climb up the chiffonier and ascend a vase of flowers to drink at the water, and then sit and sing on the edge of the table and allow us to go quite near to it without ceasing its warble; one of its favourite haunts was the wood basket, and it would often sit and sing on the edge of it. On February 12, the last night of the carnival, we had a number of friends in our *salon*, and the little mouse sang most vigorously much to their delight and astonishment and was not in the least disturbed by the talking. In the evening the mouse would often run about the room and under the door into the corridor and adjoining rooms, and then return to its own hearth; after amusing us for nearly a month it disappeared, and we suspect it was caught in a trap set in one of the rooms beyond. The mouse was small and had very large ears, which it moved about much whilst singing; the song was not unlike that of the canary in many of its trills, and it sang quite as beautifully as any canary, but it had more variety, and some of its notes were much lower, more like those of the bullfinch. One great peculiarity was a sort of double song, which we had now and then—an air with an accompaniment; the air was loud and full, the notes being low and the accompaniment quite subdued. Some of our party were sure that there was more than one mouse until we had the performance from the edge of the wood basket, and were within a yard or two of it. My son has suggested that many or all mice may have the same power, but that the notes are usually so much higher in the scale that, like the cry of the dormouse and the bat, they are at the verge of the pitch to which the human ear is sensitive; this may be so, but the notes of our mouse were so low and even the highest so far within the limits of the human ear, that I am inclined to think the gift of singing in mice is but of very rare occurrence.

JOSEPH SIDEBOTHAM

Hotel de Menton, Menton, S. France, October 31

SEVERAL years ago I received some of these animals from a friend, and kept them in confinement for one or two months. The description which your correspondent gives of their performance leaves very little to be added by me, as in all respects this description agrees perfectly with my own observations. I write, however, to remark one curious fact about the singing of these mice, namely, that it seemed to be evoked by two very opposite sets of conditions. When undisturbed, the little animals used for the most part to remain quiet during the day, and begin to sing at night; but if at any time they were alarmed, by handling them or otherwise, whether during the day or night, they were sure to sing vigorously. Thus the action seemed to be occasioned either by contentment or by fear. The character of the song, however, was slightly different in the two cases.

That these mice did not learn this art from singing birds there can be no doubt, for they were captured in a house where no such birds were kept. It may be worth while to add that this house (a London one) seemed to have been suddenly invaded, so to speak, by a number of these animals, for although my friend has lived in this house since the year 1862, it was only during a few months that singing mice were heard in it, and during these few months they were heard in considerable numbers.

Regent's Park, November 1 GEORGE J. ROMANES

Meteor

THE following account of a meteor seen here may perhaps interest some of your readers:—

On October 29, at 8h. 1m. 30s. Greenwich mean time, a brilliant meteor exploded in right ascension 268°, declination + 60° (equator of 1855); it left a bright crooked train scarcely half a degree long, which remained visible for about ten seconds, and pointed towards ξ Draconis. The course of the meteor must have been directed downwards, almost exactly towards this observatory. The flash of the explosion was seen by the assistant-astronomer, Mr. Lohse, although he was sitting in such a position as to be unable to see the meteor directly.

Lord Lindsay's Observatory, RALPH COPELAND

Dunecht, Aberdeen, November 3

INTERNATIONAL POLAR EXPEDITIONS

IN February, 1875, when the Arctic Expedition was being prepared, I asked the First Lord of the Admiralty, in Parliament, whether, in view of the small value for scientific purposes of isolated observations in the Arctic regions, in comparison with simultaneous observations at different places, and in view, also, of the interest now taken in Arctic science by foreign Governments, he would postpone for one season the departure of the proposed Arctic Expedition, and in the interval communicate with foreign Governments with a view to the organisation of other expeditions to make observations simultaneously with our own at fixed times? The First Lord said that he considered the preparations for an expedition too far advanced to admit of this, and added: "I should regard the project of combination with other powers to attain the objects in view as one beset with difficulties"—in which, I think, he was in error. In the following month, when the Supplementary Estimate for the Arctic Vote was under discussion, I again drew the attention of the Government and Parliament to the advantages of simultaneous Arctic expeditions (see *Hansard*, vol. ccxxii. p. 1354), and in *Naval Science* for April of the same year, in an article on "Foreign Polar Expeditions," I drew still further attention to the matter, concluding with an extract from a paper by Capt. Weyprecht (who so greatly distinguished himself in the Austro-Hungarian polar expeditions of 1871 and 1872–74), in which he pointed out in the clearest manner the desirability of extending future Arctic researches far beyond mere geographical exploration, and pressing forward with our studies of magnetism, electricity, the best of meteorology, &c. "The solution of these questions cannot," he said, "be expected until all nations which claim to come up to the present high standard of civilisation unite to go hand in hand, setting aside all national rivalries. To bring about decisive scientific results it will be necessary to make a number of simultaneous observations, so conducted that they will furnish a yearly *résumé* of observations made in different parts of the Arctic regions with exactly similar instruments, and from exactly similar instructions."

Upwards of a year ago NATURE gave details of Weyprecht's project for the scientific exploration of the Polar regions. It was referred to on several occasions, and pointed out that Weyprecht's plan was the only satisfactory method of obtaining results of real and permanent value.

The programme has now been extended and completed, and was prepared for submission to the International Meteorological Congress which was to have met at Rome in September, but which has been adjourned to next year. I have just received from my friend Weyprecht a copy, and may summarise its contents as follows :—

The enterprise proposed by Count Wilczek and Capt. Weyprecht has for its aim strictly scientific exploration, purely geographical discovery being a secondary matter. It will be the first step towards a systematic scientific investigation of the regions around the poles of the earth and the minute observation of phenomena peculiar to these regions—phenomena the earnest investigation of which is of the highest importance in connection with a great number of problems with regard to the physics of the globe. The international expedition will have for its aim to make in the Arctic and Antarctic regions, or in the neighbourhood of these regions, and at as many stations as it is possible to establish, synchronous observations according to a programme mutually agreed upon ; for the purpose, on the one hand, of deducing by comparison from observations collected at different points, independently of the peculiarities which characterise the years of different observations, the general laws of the phenomena investigated ; and, on the other hand, of arriving by probable inductions at a knowledge of the chances of penetrating further into the interior of the unknown regions. For this purpose each of the states participating in the work will undertake to equip at its own expense, and send out an expedition to one of the points designated. Each state will of course be at liberty to authorise its expedition to carry on work outside of that mutually agreed on.

The investigations to be made in common bear only on meteorological phenomena, those of terrestrial magnetism, aurora borealis, and on ice phenomena. At each station the observations must be continued one year, from September 1 to August 31. The meteorological observations will be made in conformity with the resolutions of the permanent International Committee, and will relate to atmospheric pressure, the temperature and humidity of the air, the direction and force of the wind, the state of the sky and its degree of clearness, and also to phenomena of condensation. The programme then gives detailed instructions as to methods and instruments of observation, all being arranged to secure accuracy, fulness, and uniformity.

It is probable that each station will be near a coast, and one of the chief objects of the expedition will be to observe the connection between the movements of the ice and the winds and currents, and if these are observed regularly, important results will no doubt be obtained as to the movements of the ice in the Arctic regions, and therefore as to the routes most favourable for reaching the pole. The best ice-observations will of course be at those stations where local conditions have the least influence.

The magnetic observations are divided into absolute determinations and determinations of the three elements. Minute directions are given in the programme as to the method to be followed in taking these observations, the fixing of the positions of the various instruments, the kinds of instruments to be used, the methods of verification and testing, the construction of observatories, &c. These directions, if faithfully carried out, would give the observer plenty of work to do, but the result would be of unprecedented value. In consequence of the persistent perturbations which prevail in these regions, isolated readings made only from hour to hour, even when carried on for long periods, are not sufficient to give with precision the hourly, daily, and monthly magnetic character of the place of observation. It is necessary, consequently, to multiply these observations. Ten observations per hour for each of the three elements will be sufficient, and to insure a rigorous synchronism it is stipulated that the three instruments of variation be read during ten minutes, from minute to minute, viz., at the full minute (– h. 56m. 0s.) the declination, ten seconds after (– h. 56m. 10s) the horizontal intensity, and ten seconds after that (– h. 56m. 20s.) the inclination. Before and after each observation, viz., – h. 52m. 0s., and at – h. 69m. 0s. the form and position of the auroræ should be noted. Immediately after the meteorological observations should be proceeded with in the following order :—Temperature, humidity, winds, clouds, atmospheric pressure. (For magnetic observations it is proposed to use Göttingen mean time.) Besides observations of the regular magnetic variations, it will be of great importance to have made, by three observers, rigidly synchronous readings of the three elements in order to obtain precise data of the total intensity. For this purpose there will be made, during one hour each day, by these observers, from minute to minute, from – h – m. 0s., readings of the three instruments. The hours of these observations should be advanced an hour each day, so as to return to the point of departure at the end of every twenty-four days.

The auroræ should be observed as to their form, their intensity, and their position. The programme then names and describes the various forms assumed by auroræ—arches, streamers, beams, corona borealis, haze, waves, flashes—for the adequate and scientific observation of which the programme gives directions.

The most favourable time for this joint expedition will be October and November, when the temperature is not so low as to necessitate special preparations.

As the absolute simultaneity of the observations is of the utmost importance, each station must be furnished with the means of obtaining the exact longitude ; good chronometers will also be necessary. To carry out the above observations to their fullest extent, four observers will suffice for each station, if among the subordinates there are men who can perform the purely mechanical duty of reading the instruments.

The programme concludes with three propositions, the purpose of which is to insure the possibility of the exact comparison of the magnetic observations.

The following are the points proposed as most favourable for the various observations referred to above :—In the northern hemisphere—The north coast of Spitzbergen ; north coast of Novaya Zemlya ; Finmark, near the North Cape ; the mouth of the Lena, on the north coast of Siberia ; New Siberia ; Point Barrow, on the north-east of Behring Strait ; the west coast of Greenland ; the east coast of Greenland, about 75° N. lat. In the southern hemisphere—The neighbourhood of Cape Horn ; the Kerguelen or Macdonald Islands ; one of the groups south of the Auckland Islands.

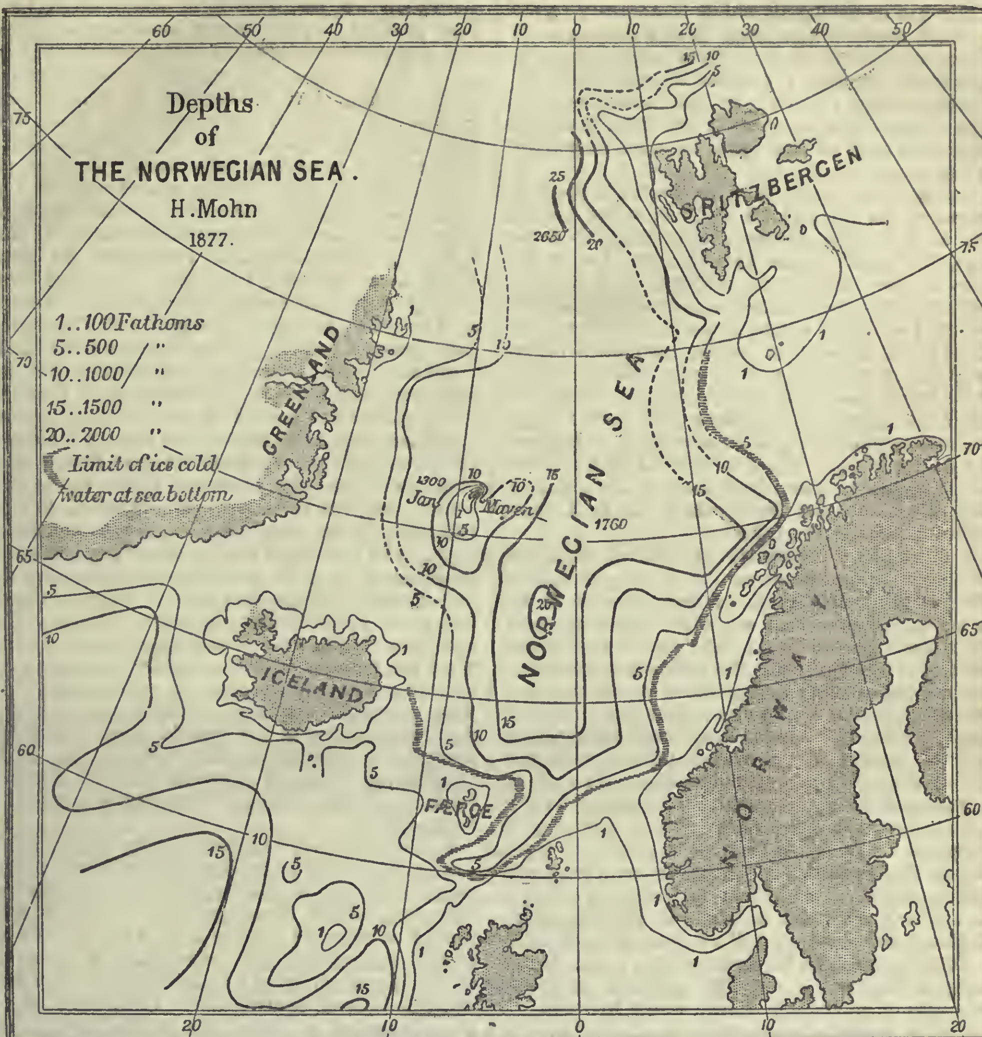
I wish that in the influential pages of NATURE this great international scientific subject could be again urged. I cannot help thinking that in the present Hydrographer of the Navy we have an officer who would be at once most able and willing to take part in giving, in the way suggested, true scientific direction and scope to future Arctic research. My confidence in the great value of simultaneous observations in comparison with the meagre results of isolated expeditions must be my apology for thus writing.

E. J. REED

THE NORWEGIAN DEEP-SEA EXPEDITION

FROM soundings taken by the second German Polar Expedition, and kindly communicated by Capt. Koldewey, of Hamburgh, I have been induced to alter

my views about the configuration of the sea-bottom around Jan Mayen. The figure of the bottom which I at



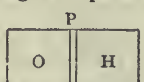
present find the most probable I have given in the chart which I send herewith. It will be observed that it is the part of the sea between Jan Mayen and Iceland which is to be corrected on the small chart which was published in NATURE, vol. xvi. p. 527.

Christiania, October 23 H. MOHN

ON THE DIFFUSION OF MATTER IN RELATION TO THE SECOND LAW OF THERMODYNAMICS

1. THE purpose of this paper is to call attention to a natural process that appears to constitute an exception to the second law of thermodynamics, and which, if noticed by others, would at least appear from its importance to merit a more general recognition. The subject may be best dealt with by means of a simple illustration, the principles involved in the action of which are already perfectly well known.

2. Let the annexed figure represent a cylinder, contain-



ing a piston, P; a suitable (plumbago) porous diaphragm (as used for diffusion experiments) being fitted into the piston. The piston can be connected conveniently with any outer arrangement for doing work. Suppose the one half of the cylinder to be filled with oxygen, the other half with hydrogen. Then, as is known, according to the kinetic theory, the molecules of O and H are impinging continually against the porous partition or diaphragm, P, and the molecules in their impacts thus occasionally encounter vacant spaces or pores, and so continue their motion on across the diaphragm into the opposite compartment. Owing, however, to the fact that the molecules of hydrogen are moving four times as fast as the molecules of oxygen, they strike the diaphragm correspondingly more frequently, and thus four times as many hydrogen molecules pass through into division O, as oxygen molecules pass through into division H. [The piston is supposed fixed at present, so that no work being done, there is consequently no heating or cooling of the gas.] But on account of the excess of molecules passing into division O, the pressure there will rise. If, then, after the pressure has risen to a certain degree, the piston be

suddenly released, it will be driven by the excess of pressure in the direction O H, and in that act the gas in O will be chilled and the gas in H heated, which is contrary to the second law of thermodynamics, since in this process work is derived from matter *all at* a uniform temperature, or work is derived by cooling a portion of gas *below the coldest of surrounding objects.* In the same way the piston might have been connected to some external mechanism, and so part of the work be done externally (in a self-acting manner).

3. There can be little doubt that such work is done in natural processes (in the animal and vegetable world) since plants and organic tissues are distinguished for their *porosity*, and such tissues are permeated with the various gases of the atmosphere, carbonic acid, &c. It may be observed that even without any porous diaphragm at all, or when two gases whose molecules possess different velocities are allowed to diffuse into each other, there is invariably a transference of heat, which is contrary to the second law of thermodynamics, which law assumes that heat cannot pass between two bodies originally at the *same* temperature, or heat cannot pass from a colder to a hotter body. Yet it is evident that as soon as the heat has begun to pass from one of the diffusing gases to the other, the one from which the heat commences to pass is already the colder.

4. Such a principle is evidently capable of an enormously wide application in nature. It is only necessary for example for the constituents of the universe to be *diverse*, to get any amount of work by diffusing them together, even if all originally at the *same temperature.* The principle of *the tendency to the uniform diffusion of Matter*, is capable of completely overthrowing *the tendency to the uniform diffusion of Energy*; for even if energy were uniformly diffused, the uniformity could be upset by the diffusion of matter (*i.e.* provided matter were not already all uniformly diffused or homogeneous): and, as we have seen, the quantity of work to be derived by the diffusion of matter is limited only by the quantity of matter at disposal.[1] In order that all capacity for work might cease in the universe, it would be necessary not only that there should be a uniform diffusion of energy, but also a uniform diffusion of matter. Heterogeneity confers a capacity for work, as well as inequality of temperature. Heterogeneity, as far as is known, is one of the distinguishing characteristics of the material universe. Any dissimilarity of molecular mass, which (by equality of temperature) is necessarily attended by dissimilarity of molecular *velocity*, confers a capacity for work. The dissimilarity of *velocity* is evidently the efficient cause in determining the work, and therefore in the exceptional case where dissimilarity of molecular structure is not attended by inequality of mass (and consequently not by inequality of velocity), work could not be derived. We may note, therefore, that inequality of molecular *velocity*, as well as inequality of molecular *energy*, confers a capacity for work, and in order that all capacity for work should cease, not only must molecular *energy*, but also molecular *velocity* be uniformly distributed, or the molecules of matter which (by equality of temperature) possess unequal velocities, must be uniformly diffused.

5. We may observe that gravity which does not interfere with the uniform diffusion of *energy*, does interfere with the uniform diffusion of *matter.* Thus, for example, the energy (heat) of the atmosphere tends to be uniformly diffused throughout a vertical column of the atmosphere, in spite of the action of gravity. But the uniform diffusion of matter (*i.e.*, the uniform mixture of the gases of the atmosphere through each other) is prevented by gravity. For by the well-known law of Dalton (which accords with the result of the kinetic theory of gases), each gas arranges itself as a layer upon the earth's surface, precisely as it would do if no other gas were present. Thus (as is known), owing to the fact that a greater quantity of nitrogen exists in the atmosphere than oxygen, the nitrogen consequently rises to a greater height than the oxygen, so that at considerable heights the nitrogen predominates. Thus the uniform diffusion of the constituents of the atmosphere through each other is prevented by gravity. It may, perhaps, be just as well to note in connection with this point that those gases which are observed at the surface of nebulæ are not necessarily at the surface because of their greater *lightness*, but this is also determined by *quantity;* for as we have observed, each gas (according to the known conditions of equilibrium) arranges itself about a centre as if no other gas were present; and therefore each gas must penetrate to the centre of the nebula, and therefore could not reach as far as the surface unless its *quantity* were sufficient (though, no doubt, by a greater lightness a less quantity of gas will suffice for that purpose). There might possibly be a tendency to assume (unless the consequences of the above principle were rigidly kept in view) that the light gas observed (such as hydrogen) was floating on the surface of the nebula. We know that according to the conditions of gaseous equilibrium this is wrong, and that each gas (if freed from other disturbing causes) will have its basis at the centre of the nebula, where, therefore, the composition or mixture of gaseous matter is uniform, but nowhere else (excepting in the very improbable case where the quantities and densities of all the gaseous constituents are the same). If gravity were to cease (and the gaseous constituents of the nebula were supposed confined or prevented from expanding), the constituents of the nebula would uniformly diffuse themselves throughout the entire mass, and this act of diffusion would be attended by a transference of heat, even if all the gaseous constituents were at the same temperature.

6. Thus we may observe that by merely modifying the action of gravity or by altering the position of a portion of gas relatively to gravity, work may be derived through diffusion. Thus if we suppose a portion of gas to be moved to different positions in a nebula, the constitution of the portion of gas or the mixture of its constituents is changed according to its position, and in these changes work is derived, or available. Only when the portion of gas is situated at the centre of the nebula are its constituents uniformly diffused through each other; less and less so towards the outside.

7. It would thus appear to follow that, as far as present knowledge goes, a uniform diffusion of *matter* as well as a uniform diffusion of *energy* would be at least required, in order that all capacity for work and physical change should cease in the universe. At the same time does it not rather behove us to look to a time when, through *increase of knowledge*, a means for recurrence may possibly be discovered, whereby physical change is continued, rather than to look to the purposeless end of a chaos of uniform temperature and uniform distribution of matter? Humboldt says relatively to this point (Preface to "Cosmos"): "I would therefore venture to hope that an attempt to delineate nature in all its vivid animation and exalted grandeur, and to trace the *stable* amid the vacillating ever-recurring alternation of physical metamorphoses, will not be wholly disregarded at a future age."

S. TOLVER PRESTON

[1] Since the first draft of this paper was written, I have been informed that the question of the quantity of work to be derived by diffusing gases has been treated of by Lord Rayleigh (*Phil. Mag.*, April, 1875), but he does not apparently mention the bearing of the case on the second law of thermodynamics.

MUSIC A SCIENCE OF NUMBERS[1]

THE subject which I submit for your consideration this afternoon is the influence of numbers in music, as in the various combinations of consonances and dissonances

[1] Read before the Musical Association of London, November 5, 1877, by W. Chappell, F.S.A.

which we hear every day, and to show how these are explained by the fundamental laws of the science.

Although music has appeared to many persons a difficult subject, it is really one of the most easily intelligible and one of the most firmly grounded of sciences. It is purely a science of numbers.

The consonances which charm the ear, such as the octave, twelfth, fifth, fourth, and the major and minor thirds, have two concurrent sets of vibrations; the one set produced by the lower string or pipe, and the other by the upper. Although they vibrate at different rates, yet there are periodical coincidences of vibration between them, and these coincidences sound with much more power upon the ear than the vibrations which are non-coincident, or sound apart. It has been calculated that two hammers striking simultaneously upon an anvil have, through the greater displacement of air, fourfold loudness, instead of merely double. The same law applies to musical sounds. Coincidence of vibration is more briefly expressed by its synonym, "consonance;" and all non-coincident vibrations are included in "dissonances," meaning only that they sound apart. In a musical sense, dissonance is the medium between concord and discord, running from one into the other; for, in the most pleasing intervals, there are some non-coincident vibrations, and when these become very numerous, they overpower all concord. This will be shown in the sequel.

Suppose we take one long pianoforte string or an organ-pipe. The lowest sound it can produce will be that of its whole length, and this may be made the foundation of an entire scale of consonant notes, for every aliquot part of the length, being such as will measure without any remainder, will be also a multiple of the vibrations of No. 1. Thus No. 2, the octave, is half the length and vibrates twice as fast as the whole string. No. 3, the so-called twelfth, or octave and fifth, is a third of the length of No. 1, and it vibrates thrice as fast. Then, if we sound No. 3 with No. 2 instead of No. 1, we throw off the lower octave and have the fifth only, or 3 to 2. It is essential for consonance that the intervals should be aliquot parts of No. 1, for if otherwise, we should only create discord. The musical law is expressed very simply, that the number of vibrations is in inverse ratio to the length of a string.

The scale of all consonances is called the harmonic scale, copies of which are before you. It is exemplified by string or pipe. Let us consider, first, the Æolian harp, on which the winds alone produce the consecutive sounds. The strings are tuned in unison, except the two outmost, one on each side, and those are covered with wire, and tuned an octave lower. When the wind blows quickly enough to sound the bass strings, which we will suppose to have tuned to C on the bass clef, with 128 vibrations in a second of time, it is the whole string which sounds first, and the rapidity of the wind must be doubled before the harp will sound any change of note, and that note will be the octave above the first. It has already been said that the octave is produced by half the length of a string, and that it vibrates twice as fast as the whole—but mark the coincidence between the music and consecutive numbers; 1 and 2 have no note between them, although the sound jumps from the whole length to that of the half! When the bass strings sound the half length they have divided themselves into equal halves by a node, and that node creates tension in opposite directions, the one ventral segment pulling, as it were, against the other. These self-forming nodes may be easily seen by daylight, and at night by throwing a light upon the string. They were shown at our first *conversazione* in these rooms by Mr. Spiller, and at the Edinburgh meeting of the British Association by Mr. Ladd. The gust of wind which sounds the octave, or half length of the bass strings of the Æolian harp, sounds at the same time the whole length of the gut strings, because they are tuned to that pitch. Then, as the wind rises, subdivision goes on in both with every multiple of 128 vibrations for the bass, and of 256 vibrations for the tenor strings.

The reason for tuning the Æolian harp to a low pitch is, that the strings may be more easily acted upon by the wind. We read, poetically, of hanging one in a tree, but it requires a much stronger draught than it will get there, except during a hurricane, when no one will care to go to listen. Our late lamented Vice-President, Sir Charles Wheatstone, F.R.S., fixed a single violin string under a very draughty door, as an Æolian harp, and he calculated the increase of draught caused by lighting a fire in the room, and by the opening of an outer door, by the rising pitch of the note. The varieties produced by this string have been described as "simultaneous sounds," but they were purely consecutive. Anyone may satisfy himself that it could only be so, by repeating the experiment with a good violin string. The change of note is simultaneous with the change of nodes in the string. Mere undulations, or irregularities of vibration, will not change the note, but injure the quality of the tone. All the curves that a string may describe in vibration have been calculated by mathematicians, but only when nodes are formed are they of any importance in music.

Often have I experimented upon harmonics or natural sounds, in former years, and have watched the changes of node, and have heard the simultaneous change of note. The experiments may be tried by any one who has access to a harpsichord, or a very old grand pianoforte. The tension is too great in modern instruments to allow free play to the string. Raise the damper and strike one of the longest uncovered strings with a hard pianoforte hammer near the bridge. The changes follow in numerical order, 1, 2, 3, 4, 5, as in the paper before you, and the sounds ascend by octave, fifth, fourth, major and minor third, harmonic seventh, to the third octave, and then to the major and minor tones. It is difficult to attain the highest of these numbers, but the harmonic seventh, No. 7, is readily distinguished by its unusual sound.

In the Æolian harp the rising pitch of the sounds is caused by the increasing rapidity of the wind; but it is not so on a pianoforte. It is there due to gradual contractions of the string till it ceases to vibrate, and sinks to rest. The vibrations of a long string are widely discursive, but they become gradually more and more contracted as the nodes of the string diminish in length. The point to be remarked is that the sounds jump over intermediate discords—all are consonances—all aliquot parts: all the sounds are multiples of No. 1. It matters not whether it be wind, string, or pipe; in each of them nature teaches us the scale which is to resolve all musical doubts, all disputed chords. She indicates all the basses for musical intervals, the more remote ones adapted only for melody, and the nearest for consonant harmony.

To prove the case further we may take an illustration from a pipe. It must not be from those which have lateral openings, or keys, because they shorten the column of air artificially, but from such instruments as the coach horn, or hunting horn, the so-called French horn, or the trumpet without valves.

The fundamental tone, No. 1, or lowest sound it can produce, is derived from the whole column of air within the tube. To produce No. 2 the rapidity of the breathing must be doubled, and then the column of air within the horn divides itself into two equal halves, and the sound is an octave above; so that, if the first note be tenor C with 256 vibrations in a second of time, this treble C requires to be blown at the rate of 256 vibrations to produce it. Here, again, we arrive at the identification of sounds with numbers; for, just as there is no intermediate number between 1 and 2, so is there no intermediate sound between 1 and 2, its double in vibrations, produced by half its length, upon the horn. The

numbers run both ways. They are fractions as to length of tube, and multiples as to vibrations. Again, just as there *is* an intermediate number between 2 and 4 (the second octave), so is there one intermediate sound, and one only; it is No. 3, which is produced by a third of the length of the tube, and is the fifth above No. 2. The fifth and fourth divide the vibrations of the octave equally between them, so that the fifth is three times No. 1, and the fourth immediately above it is four times;—this, notwithstanding the diminution of the musical interval. The names which we have adopted for musical intervals are usually calculated from the keynote, as from C to E a third, from C to F a fourth, and from C to G a fifth, but these names are not real quantities, and are rather confusing than an assistance. The octave is not an eighth, but half, and the double octave is not a fifteenth, but a quarter of the length of No. 1, and vibrates four times as fast. Octaves are powers of 2, thus 2, 4, 8, 16, and 32 are successive octaves. But the octave 4 to 8 has only four sounds, and these are our major and minor third, and two others, divided by the harmonic seventh, which we do not use. From 8 to 16 are eight sounds, of which we use three, the major and minor tones, and the so-called diatonic semitone, as from B to C. It is really the smallest of the eight tones, and not a semitone. The next octave is from 16 to 32, and that is all of semitones, while 32 to 64 is all of quarter-tones. After that, the octave is divided into eighths, sixteenths, and thirty-second parts of tones, among which it is only useful to note (and that only among musicians and mathematicians, that the so-called "comma," having the ratio of 80 to 81, is the eighth of a tone above the third of any key—as it is above E in the key of C. We have lately had mathematicians among us who are not *μουσικοί*, and who have, therefore, proposed to divide an octave into "twelve *equal* semitones." This is pure geometry, and not music. In music there cannot be even two equal semitones within an octave. If our friends will only change their theme from twelve equal semitones into twelve *equally tempered* semitones, and give us their experience of the proposed sounds when heard *with the bass* (which seems not to have yet been taken into account), we shall gladly avail ourselves of their research, on the grounds of modern expediency. In the meantime we must be content to leave the tempering of a scale in the hands of experienced practical men, who, judging only by their ears, as they always will, have hitherto satisfied our immediate requirements.

The interval of a fifth is 2 to 3 in ascending and 3 to 2 in descending, but, as the figures are usually placed over the upper note in scales, the 3 is written above the 2 as in the scale in your hands (the third of them), where it appears over G, referring to C as 2.

And now for the practical use of these figures, for although the harmonic scale may be referred to, they are most easily remembered. All young pupils are taught the difference between an octave, a fifth, a fourth, and a third, upon the pianoforte, and it is only to associate the numbers with those intervals, to find out the best bass, and *every admissible bass*. All octaves are in the ratio of 2 to 1, whether it be 4 to 2, 8 to 4, or 16 to 8. All fifths are in the ratio of 3 to 2, all fourths in that of 4 to 3, all major thirds 5 to 4, and minor thirds 6 to 5.

For instance, in the key of C, C to the F above it is a fourth, and F is No. 4, therefore, the F, two octaves below, is the consonant bass; whereas, if we strike G with the C above, C becomes the natural bass to that interval. The most consonant basses are always found in the lowest numbers, because the proportion of consonant vibrations is there greatest. Thus, from D to G is also a fourth, in the key of C, but the numbers are 9 to 12, with a remote bass in C, and there will be 21 vibrations, of which only two will coincide in every cycle—1 of the 8, with 1 of the 9. Then, the proportion of non-coincidence will be so great as to make the sound unpleasing to the ear. But as 9 to 12 is in the *ratio* of 3 to 4, we have the best bass in these lowest numbers, and take G. By the various basses to intervals we modulate into other keys.

At the International Exhibition, held at South Kensington in 1862, Mr. Saxe, the eminent inventor of Saxe horns, exhibited an immense horn with an exceedingly long coil of tube, and perhaps standing six feet in height. When asked by the jury the object of this excesssive size and length, he answered, "C'est pour jouer dans le cinquième étage"—"It is for playing in the fifth octave," and he produced with facility any of the sixteen tones and semitones of that octave from it. Half the length of any open conical tube is expended upon its second note, the octave. No human power could have blown the low notes of that horn. Supposing it to have been tuned to the lowest C upon the pianoforte, with thirty-three vibrations in a second, as the usual French pitch, it would have had 66, 132, 264, and 528 for its first, second, third, and fourth octaves, while its fifth octave would commence on treble C, with 528, and extend to C above the lines with 1056 vibrations in a second of time. It would thus be within the power of the lungs. He utilized only from the 16th to the 32nd part of his enormous tube, but it gave him the command of the semitones.

This great incumbrance of length is not necessary in a cylindrical stopped tube. It will take up its own octave according to the ratio of its length to its diameter. We have here an example in a resonating tube invented by Charles Wheatstone just fifty years ago. The lecture for which he invented it was afterwards reported in the twenty-fifth volume of the *Quarterly Journal of Science, Literature, and Art*, January to March, 1828. Both he and I knew Eulenstein, an accomplished musician, whose admirable skill in playing upon the Jew's harp was the inducing cause of that particular lecture. Eulenstein had a peculiar facility for contracting and expanding the cavity of his mouth, through the pliability of his very thin cheeks and by the management of his tongue, so that he could fit them for any harmonic note within a certain compass. Wheatstone then gave the law, that a perfect harmonic scale might be drawn from a single tuning-fork, or from the vibrating tongue of a Jew's harp, by resonators adapted, or adapting themselves, to *multiples* of the original number of vibrations. "I took," said Sir Charles, "a tube, closed at one end by a movable piston, and placed before its end the branch [or prong] of a vibrating tuning-fork of the ordinary pitch—C. The length of the column of air [within the tube] was six inches. On diminishing the length of the column of air to three inches [by moving up the piston], the sound of the tuning-fork was no longer reciprocated [in unison], but its octave was produced." "It is therefore evident from experiments," says he "that a column of air may vibrate by reciprocation, not only with another body whose vibrations are isochronous [or in unison] with its own, but also when the number of its own vibrations is any multiple of the sounding body." Again, he says: "No other sounds can be produced by reciprocation from a column of air, but those which are perfectly *identical with the multiplications* of the original vibrations of the tuning-fork or the tongue of the Jew's harp." I produced the original tube in this room about two years ago, to check a recent theory—that resonators strengthened the ear, and answered only in unison, and Sir Charles ordered this one for me, made by Mr. Groves, under his own superintendence. The improvement in this is, that the piston now works in a groove and is not liable to stick. Two octaves are produced from the tongue of one Jew's harp as rapidly as the piston can be moved up and down. There is

no slurring between one sound and another, but clear jumps from one multiple to another, and every one of them may be arrested and heard by itself by checking the piston. But, although I am glad to produce this tube before those who were not present on the last occasion, and to do honour to the memory of our eminent vice-president, who declined to refer in any way to himself, I have another motive also. This is a principle which has never been utilised. We have had pipes stopped at the top, like the usual pitch-pipe, but they have been found too slow in action to be suitable for any other purpose. This is rapidity itself, and might surely be utilised for some such purpose as pedal-pipes for an organ. The piston can be balanced outside to the greatest nicety, and one such pipe will take the scale of C, and another that of F. All that is required is to blow across the top in the manner of the Pandean pipes, or, as it appears, better still, to set free a fan or cogged wheel at the mouth tuned to each of the two fundamental notes. The wheel might be set free by the action of the foot upon the pedal. It is now well known that the length of a 32 or a 16 foot pipe may be greatly reduced by breadth of scale. We Europeans have made little, if any, use of resonators, and yet they have been long in use in Java. The drawing on the wall is of an instrument brought from Java by Sir Stamford Raffles more than half a century ago. There is one of the same kind in the British Museum. But this is perhaps of greater interest, as it may have suggested to Wheatstone the *principle* of the resonating tube. The natives of Java cast metal plates which they suspend in a row upon strings, and strike them with drum-sticks, which are fitted into circular heads. As all cast metal is more or less false in tone, owing to inequalities and lack of homogeneity, they place some of the largest bamboos, cut to short lengths, and placed upright, under the metal to make the true sounds of these resonators to overcome the false harmonics of the metal plates.

Resonators were used in the theatres of ancient Greece —we here find them used in Java; but these powerful auxiliaries to tone still await their development in modern Europe.

And now, in conclusion, permit me to draw your attento a harmonium with two keyboards, the upper one having four octaves of our scale tuned without tempering, and the lower with the five octaves of the harmonic scale, and the sixteen notes in the fifth octave. Much has been said of the harmonic scale, and this is perhaps the only instrument on which the harmonics can be fully heard and sustained for experimental use.

ROBERT SWINHOE, F.R.S.

WITHIN the last thirty years or so their respective vocations happen to have called two able lovers of natural history in the direction of the Celestial Empire —Mr. Robert Swinhoe, from England, and the Père Armand David, a Frenchman. The simultaneous investigations of these two biologists have added immensely to our knowledge of a country whose fauna not long ago was thought to be in no way interesting, because the huge population had succeeded in extirpating all the indigenous species. How far from the truth such an assumption is, has been demonstrated by the researches of the two naturalists above mentioned, the lamented death of the former of whom, at the early age of forty-one years, we recorded last week.

Mr. Swinhoe was born at Calcutta on September 1, 1836, and was educated at King's College, London, whence he matriculated at the University of London, in 1853. The next year he went, as supernumerary interpreter, to Hong Kong, being transferred to Amoy in 1855, and to Shanghai in 1858. In the same year he was attached to the Earl of Elgin's special mission to China, and afterwards to H.M.S. *Inflexible* as interpreter in a circumnavigating expedition round Formosa, in search of certain Europeans said to have been held in captivity at the sulphur mines on the island.

In 1860 Mr. Swinhoe attended Gen. Napier, and afterwards Sir Hope Grant, the Commander-in-Chief, as interpreter, and received a medal for war service. At the end of the same year he was appointed Vice-Consul at Taiwan, Formosa, and in 1865 to the full Consulship. In 1866 he was Consul, temporarily, at Amoy, and in 1868 went to explore the Island of Hainan. From May, 1871, to February, 1873, he was acting Consul at Ningpo, and at Chefoo until October of the latter year, when he had to retire from the service, on account of increasing paraplegia, from which he died on October 28 last.

Mr. Swinhoe was a Fellow of the Asiatic Societies of China and of Bengal, as well as of many other societies, having been elected into the Royal Society in 1876.

By far the majority of Mr. Swinhoe's scientific communications—fifty-two in number—mostly on the mammalia and birds of China, are to be found in the *Proceedings* of the Zoological Society of London between 1861 and 1874. Other papers appeared in the *Ibis* and the *Annals and Magazine of Natural History* within the same period. Among the most important of these are the "Catalogues" of the mammals and birds of China and its islands, in which are to be found descriptions of many new species of both classes, among which are St. John's Macaque (*Macacus sancti-johannis*), the Water Deer of Shanghai (*Hydropotes incruris*), the Mantchurian Deer (*Cervus mantchuricus*), the Orange-bellied Helictis (*Helictis subaurantiaca*), the Superb Flying Squirrel (*Pteromys grandis*), Boyce's Stork (*Ciconia boyciana*), together with a great number of other birds, for a complete account of which we cannot do better than refer our readers to a work upon the birds of China, by M. l'Abbé David and M. E. Oustalet, published at Paris a week ago.

Michie's Deer (*Lophotragus michianus*) is the name given by Mr. Swinhoe to a small deer from Ningpo, with antlers more diminutive than many other species. This, or a very closely-allied species, was previously sent to Paris by Père David, and described by M. A. Milne-Edwards under the name *Elaphodus cephalophus*.

Mr. Swinhoe, besides the collections which he made, was indefatigable and particularly successful in his endeavours to send living animals from China to this country, and there are many species, including *Cervus swinhoii*, *Hydropotes incruris*, and *Ciconia boyciana*, which were first procured by him.

It will be some time, we fear, before so enterprising a naturalist as Mr. Swinhoe takes up his residence in China, and employs every available opportunity for the prosecution of his favourite line of research.

DOUGLAS A. SPALDING

OUR readers must be familiar with this name as that of an occasional contributor to NATURE of thoughtful and acute articles in the department of mental science; they will be sorry to hear—but those who knew him will not be surprised—that Mr. Spalding died on October 31, at Dunkirk, just as he was preparing to go to the Mediterranean coast to spend the winter. Not much is known of Mr. Spalding's early life, but we are told by one who ought to know that his parents, belonging to Aberdeenshire, were in very humble circumstances, and that he was born in London about the year 1840. He himself spent his early years in Aberdeen as a working slater, doing his best to educate himself. By the kindness of Prof. Bain Mr. Spalding was allowed to attend the classes of Literature and Philosophy in Aberdeen University free of charge, in the year 1862. After that he got some teaching about London, and worked very hard to support himself, and even managed to keep his

terms as barrister, though he never practised. It was during this period of privation that he contracted disease of the lungs, from which he suffered greatly up to the time of his premature death. The first thing that brought him to the notice of the scientific world was his experiments on the instinctive movements of birds, which were first described at the Brighton meeting of the British Association in 1872, and published in *Macmillan's Magazine* for February, 1873. From a series of interesting experiments on chickens he showed that the only theory in explanation of the phenomena of instinct that has an air of science about it is the doctrine of inherited association. Instinct, he maintained, in the present generation of animals, is the product of the accumulated experiences of past generations. In another paper at the Bristol meeting of 1875 he communicated the results of further experiments, some described in NATURE, vol. viii. p. 289, bearing out still more strongly the conclusions he had already reached, and which he summed up in the statement that "animals and men are conscious automata." The Brighton paper brought Mr. Spalding into deserved repute. While travelling in France he became acquainted with John Stuart Mill, and through him afterwards with many other distinguished men, who all treated Spalding with great respect. Through Mill also, we believe, he became acquainted with Lord and Lady Amberley, with whom he lived as companion and tutor to their children from 1873 until the death of Lord Amberley. Mr. Spalding was appointed guardian to the children, but was ultimately compelled to withdraw from this office owing to his religious opinions, Earl Russell, however, allowing him to retain for life the salary settled upon him by Lord Amberley. For the last two years Mr. Spalding has lived mostly in the south of France, bearing his fatal and protracting illness with the greatest equanimity, regretting only his powerlessness to work and his enforced absence from London.

As to the value of his scientific work our readers having the material before them are able to judge. By his experiments on animals he did much not only to clear up the nature of what is called instinct, but also to shed a new light on certain mental phenomena in man himself. His papers in NATURE, mostly reviews of works connected with psychology, on the metaphysics of instinct and evolution—of the latter doctrine he was a warm advocate—were good specimens of clear and close reasoning. That he had a tender side to his character is evident from even his Association papers, and still more so from the interesting letters written by him to NATURE, last April, on the swallows and cuckoos at Menton. All who knew him felt that had his health permitted he would have added largely to scientific knowledge in the special department to which he had devoted himself—physiological psychology.

OUR ASTRONOMICAL COLUMN

THE SOLAR ECLIPSE OF 1878, FEBRUARY 2.—The eclipse of the sun in February next will be annular, but the central line passes at such high southern latitudes that the annular phase is not likely to be observed unless it be in the western parts of Tasmania near sun-set. Thus the central eclipse will commence in longitude 103° 0′ west of Greenwich, latitude 73° 8′ south, and will end in longitude 149° 25′ east, latitude 40° 58′, and the eclipse is central at noon in longitude 112° 27′ west, and latitude 84° 3′ south. Another point upon the central line is in longitude 145° 25′ east, and latitude 42° 25′, where the sun's altitude, however, will be less than 4°; this point lies on the west coast of Tasmania. Launceston is near the central line, but at the middle of the eclipse the sun at that place is almost in the horizon.

A large partial eclipse will be visible over the southern parts of Australia. At Melbourne it will commence at 6h. 1m. P.M. local mean time, at 120° from the sun's north point towards the west, and will attain its greatest magnitude 0·91, just before sunset, or at 7h. 4. At Adelaide the eclipse will begin at 5h. 44m. local time and will be greatest about 6h. 45m., when the magnitude will be 0·85, with the sun at an altitude of between 5° and 6°. At Perth, in Western Australia, the whole eclipse will be visible; greatest about 5h. 25m. local time, magnitude 0·66, with the sun at an elevation of 23°.

The next *total* eclipse of the sun visible in those parts of the earth will take place on the morning of September 9, 1885. At Wellington, New Zealand, the eclipse begins about a quarter of an hour after sunrise; totality commences at 7h. 42m. A.M., but continues only about forty seconds; in 175° 3′ east, and 40° 34′ south, on the central line, the duration of totality is 1m. 54s. It should be stated that these figures are founded upon the tables of Damoiseau and Carlini.

THE MINOR PLANET EUPHROSYNE.—It does not frequently happen that we have to look for a planet at 60° of north declination; such, however, will be the case at the end of the present year, and in the first days of 1878 as regards Euphrosyne, No. 31 of the group, which was discovered by Ferguson at Washington, on September 1, 1854. The planet will be in opposition on December 18, with the brightness of a star of the tenth magnitude. The following are its calculated positions when passing its greatest northern declination.

12h. Berlin M.T.	Right Ascension. h. m. s.	Declination. ° ′ ″	Distance from the Earth.
1877, December 31	5 20 17·1	60 2 56	1·613
1878, January 1	5 18 49·5	60 2 59	1·614
,, ,, 2	5 17 24·1	60 2 38	1·618

The star L. 10067 in Camelopardus, which Lalande calls an eighth, and Argelander a seventh, will be a good guide for identifying the planet in this position. At midnight at Greenwich on January 1, by calculation, Euphrosyne will precede the star seven seconds in R.A., seven minutes to the south of it.

The latest elements of this body which, it will be seen, approaches much nearer to the pole of the equator than the generality of the small planets, are as follows, according to the computations of Mr. S. W. Hill:—

Epoch 1877, December 18 0 M.T. at Berlin.

	° ′ ″
Mean Longitude	90 10 23
Longitude of Perihelion	93 17 30
,, Ascending Node	31 33 23
Inclination	26 28 34
Eccentricity	0·222786
Semi-axis major	3·14902

COMETS OF SHORT PERIOD IN 1878.—Of the comets known to be performing their revolutions in periods of less than ten years, two are due in perihelion again in the ensuing year, probably within a few days of each other. According to Dr. von Asten's elements of Encke's comet at its appearance in 1875, the next perihelion passage, neglecting perturbation, would fall about July 27·0, which involves an apparent track in the heavens unfavourable for observation. In 1845, when the conditions were more nearly the same than at any of the comet's returns since its periodicity was first ascertained, only four observations were secured between July 4 and 14—at Rome, Philadelphia, and Washington. If the comet is not observed before the perihelion in 1878, while at a considerable distance from the earth, it may be found at the observatories of the southern hemisphere, after perihelion, or in the latter part of August, when it makes its nearest approach to us, although its distance at that time will not be less than the mean distance of the earth from the sun. The second comet, which is due in perihelion in 1878, is that discovered by Dr. Tempel on July 1, 1873. The period of revolution assigned by Mr. W. E. Plummer from observations extending to October 20, is 1,850 days; and the comet, neglecting perturbations

which are not likely to be material, would be in perihelion again about July 20; this date, however, will be uncertain, as thus far no definite discussion of the observations in 1873 has been published. Some time since it was stated that Herr Schulhof, of the Vienna Observatory, was at work upon this comet. With the above date for perihelion passage, the apparent path would be favourable for observations, and the comet would approach the earth almost as closely as is possible with the actual form of orbit.

NOTES

THE session of the Royal Society opens next Thursday with the Bakerian Lecture On the Organisation of the Fossil Plants of the Coal Measures, Part ix., which will be delivered by Prof. W. C. Williamson, of Manchester, F.R.S.

WE learn from the *Times* that the following is the list of the new Council which will be submitted to the Royal Society for election at their anniversary meeting on St. Andrew's Day next, the 30th instant:—President, Sir Joseph Dalton Hooker, C.B., K.C.S.I., M.D., D.C.L., LL.D.; Treasurer, William Spottiswoode, M.A., LL.D.; Secretaries, Prof. George Gabriel Stokes, M.A. D.C.L., LL.D., Prof. Thomas Henry Huxley, LL D.; Foreign Secretary, Prof. Alexander William Williamson, Ph.D.; other members of the Council—Frederick A. Abel, C.B., V.P.C.S., William Bowman, F.R.C.S., Frederick J. Bramwell, M.I.C.E., William B. Carpenter, C.B., M.D., LL.D., William Carruthers, F.L.S., William Crookes, V.P.C.S., Prof. P. Martin Duncan, M.B., P.G.S., William Farr, M.D., D.C.L., Prof. William H. Flower, F.R.C.S., Prof. G. Carey Foster, B.A., F.C.S., John Russell Hind, F.R.A.S., Lord Rayleigh, M.A., Vice-Admiral Sir G. H. Richards, C.B., Prof. Henry J. Stephen Smith, M.A., Prof. Balfour Stewart, M.A., and Prof. Allen Thomson, M.D., F.R.S.E.

MR. F. M. BALFOUR, Fellow and Lecturer of Trinity College, Cambridge, has joined the editorial staff of the *Quarterly Journal of Microscopic Science*. The journal will in future be conducted by Prof. Ray Lankester as responsible editor, with the co-operation of Mr. Archer in Dublin, Mr. Balfour in Cambridge, and Dr. Klein in London. The volume for the year just concluded shows an increase in the number and efficiency of the lithographic plates. Instead of sixteen octavo plates as usual four years ago, there are twenty-five, many of which are double sized, and some coloured.

MADAME LEVERRIER, the widow of the astronomer, died on November 1, at the age of fifty-eight years. This lady was suffering from a protracted illness, when the loss of her husband produced a shock from which she was not able to recover. She was a daughter of M. Choquet, an eminent professor of mathematics in Paris. Her father, about eighty years old, was present at the funeral. On the very day that Madame Leverrier died, the *Journal Officiel* published a decree, signed by M. Brunet, the Minister of Public Instruction, ordering the bust of Leverrier to be placed in the Palace at Versailles, where are to be collected the memorials of the great Frenchmen of the nineteenth century. This honour has been decreed to a number of other men who have ranked foremost amongst *littérateurs*, artists, or politicians. M. Leverrier, it is strange to say, has been chosen as the only representative of science.

THE French Academy of Medicine has been authorised by the ministry to accept a legacy of 4,000*l.* bequeathed by Dr. Demorquay, to help them to build a hall of meeting.

M. FAYE, Inspector-General in Science of Secondary Education in France, has been appointed to a similar office for superior education in succession to the late M. Leverrier. M. Fernet has succeeded to M. Faye's post.

M. WATTEVILLE, director of Arts and Sciences in the French Ministry of Public Instruction, has issued a circular notifying that a special exhibition will be held at the Champ de Mars, for collecting the results of the scientific missions granted by the French Government in 1867. Almost every country, civilised and barbarian, near or remote, has been visited.

M. BERTRAND, the perpetual secretary of the French Academy of Sciences, has been appointed by M. Bonnet member of the International Metric Commission.

COMMANDER GUISEPPE TELFENER has announced his intention of placing at the disposal of the Italian Geographical Society a sum of 40,000 francs to found a section of commercial geography and organise at Rome a museum to contain specimens of all the products which Italy exports and imports.

AT a meeting held at the London Library on October 26 (Mr. Robert Harrison in the chair), it was determined to form an Index Society, with the immediate object of compiling subject indexes and indexes of standard books of facts, to be printed and circulated among the members; and with the ultimate object of building up a general index of universal literature, which can be referred to at the office of the society during compilation. The great aim of the society will be the gradual accumulation of aids towards the preparation of a key to all knowledge, and with this object a library of indexes will be commenced. The subscription will be one guinea. Subscribers' names and suggestions on the subject of the proposed society will be received by Henry B. Wheatley, hon. sec. *pro tem.*, 5, Minford Gardens, West Kensington Park, W. The utility of such a society and such an index to scientific men of all classes and grades will be obvious, and the effort now being made deserves their hearty support.

THE system under which the official addresses are made at the annual meeting of the American Association for the Advancement of Science seems curiously complicated, and sometimes is a puzzle even to the old members of that body. The retiring president, who has been the presiding officer in the preceding year, makes the opening address, which is the presidential address for that year. The presidents of the sections, on the other hand, who have just entered on their duties, open their sections respectively with an address. There are only two sections, A and B; other divisions are parts of these, and are characterised as sub-sections. Section A has charge of mathematics, astronomy, physics, chemistry, and microscopy; Section B of zoology, botany, geology, palæontology, ethnology, and archæology. There is a further complication in the circumstance that the presidents of the sections are also the two vice-presidents of the Association. To illustrate this arrangement, we may cite proceedings at the meeting of last August at Nashville. Prof. W. B. Rogers, who was the president of the Association last year, and president at the Buffalo meeting, was expected to open the Nashville meeting with the presidential address, but was prevented by illness. Professors E. C. Pickering and O. C. Marsh are respectively presidents for the present year of Sections A and B, and also vice-presidents of the Association. The address on "The Introduction and Succession of Vertebrate Life in America," by Prof. Marsh, which we recently published in full, was his official address as the president of Section B, delivered at the opening of the Section. To carry the illustration further, it may be added that Prof. Marsh, who was elected at this year's meeting, president of the Association, will not preside till next year at St. Louis, and will not be expected to deliver his presidential address until the meeting of the following year, 1879.

THE death is announced of Dr. Henry Lawson, until recently editor of the *Popular Science Review*.

MR. JAMES FLOWER, for many years the articulator of the skeletons at the Royal College of Surgeons, has just died from

carcinoma of the rectum, from which he had been suffering for some time past. Mr. James Flower was seventy-seven years of age, and had served in the army in his younger days.

FROM statements made at a meeting of the California Academy of Sciences, the eucalyptus tree may be enumerated among the means for checking fire. Eucalyptus shingles are said to be fire-proof. A tree of this species was exposed to the San Francisco fire of 1876, and is still flourishing. The notion is urged that the spread of fires in cities could be checked by setting out such trees for shade and ornament. All varieties of the eucalyptus are said to possess this valuable property.

THE first examination of Surveyors and Inspectors of Nuisances by the Sanitary Institute of Great Britain, took place on Monday, October 29. Eight candidates presented themselves, five of whom were successful in obtaining certificates of competence, namely, Mr. H. M. Robinson, Surveyor, Ulverston; Mr. J. Parker, ditto, Bridgwater; Mr. F. Booker, Inspector of Nuisances, Bradford; Mr. W. S. Prebbles, ditto, Blackburn; Mr. Thomas Blanchard, ditto, Evesham. Fifteen candidates have already entered their names for the next examination.

NEWS has been received, the *Geographical Magazine* states, from M. Kelsief, who has been making researches during the past summer along the Murmanian coast and in Lapland, for the Moscow Anthropological Exhibition of 1879. M. Kelsief had been travelling with M. Singer, secretary of the Natural History Society; and the two had, up to the time of their parting company on the borders of the White Sea, made a good collection of stone implements and other prehistoric remains, M. Kelsief then took a cruise in a small vessel, and traversede with considerable difficulty, about 800 versts in all in the White and Polar Seas, and passed the whole of the summer within the Arctic circle. Along the Murmanian coast he visited the Lapps, who inhabit there subterranean dwellings, grouped at intervals of between 70 and 100 versts. He was accompanied by only one servant, and after enduring considerable hardships through exposure and insufficiency of food, he started on August 29 for the north of Finland, where he proposes to visit the Lapps of Lake Enara, and to return to St. Petersburg by way of Tornea.

THE portion of the Indus River where it emerges from Kashmir territory and flows through the mountainous region of Gilgit and Chilas to rejoin our frontier near Darband—a strip in all of about 120 miles in length—has just received, we learn from the *Geographical Magazine*, detailed exploration at the hands of a Punjab surveyor. This piece of work will complete our geographical knowledge of this river, and will contribute useful topographical information to our future maps, though it must be remembered that the general course of the river had been pretty accurately determined in 1870 by Capt. Carter's careful triangulation of the peaks flanking its eastern and western banks.

THE London papers contain frequent announcements of expected high tides, which are no doubt useful to many as forewarnings of danger. But we cannot understand why the burden of such predictions should fall solely upon Capt. Saxby. Is he the only one qualified and concerned to make such predictions?

WE have received a reprint of four important papers which originally appeared in the *New York Tribune*, and which are now sold separately by that paper at the insignificant price of 10 cents. The papers are on the Evolution of Life, by Dr. Draper; Ancient Life in America, by Prof. Marsh; Catastrophism and Environment, by Mr. Clarence King; and the Peabody Museum (illustrated), by Mr. Wyckoff. This is No. 37 of these science numbers of the *Tribune*; evidently, then, it is the interest of the management to find space for so much science.

A COMMITTEE has been formed in Holland under the patronage of Prince Henry of the Netherlands, and 24,000 florins have been collected, to send out in May of next year a small but strong sailing vessel to the west coast of Spitzbergen, with the view of reaching the mouth of the Yenissei. The objects of the expedition are to explore the new commercial route to the Siberian rivers, to train sailors who might ultimately be intrusted with the formation of a scientific station, and to erect a few monuments to the memory of the early Dutch arctic navigators.

THE celebrated mammalian and reptilian remains obtained by Mr. Beckles from the base of the middle Purbecks at Durdlestone Bay, and described by Prof. Owen in the Palæontographical Society's *Memoirs* were acquired last year by the British Museum. Under the care of Mr. Davis they have been carefully cleaned, mounted, and labelled, and are now being placed in cases. The total number of specimens acquired was about 2,000, but only some of the best are exhibited.

THE tank prepared at the Westminster Aquarium for the whale is now used as a seal pond. Its large size gives ample scope for the gambols of the young seals, which can now be seen under circumstances more favourable than have before been offered in London.

MR. O. H. A. MOGGS writing to the *Times* from Bullpits, Bourton, Dorset, states that that place was visited on Friday last by what seemed to be two shocks of an earthquake. The first occurred at about 8.10 A.M., and was accompanied by a rumbling sound, which lasted about ten or twelve seconds. The vibration of the ground was very slight, although it could be distinctly felt. The second shock was felt at 11.20 A.M. The vibration of the ground was very violent, causing houses to shake and the windows to rattle. This lasted about six seconds, and was accompanied by a rumble like the former, only heavier, which lasted about eight or ten seconds.

A SLIGHT shock of earthquake was felt on Sunday afternoon at New York. It was also felt in New Brunswick and Quebec.

MESSRS. J. AND A. CHURCHILL have published in a separate form, for the use of students, the valuable "Notes on Embryology and Classification" by Prof. Lankester, from the current number of the *Quarterly Journal of Microscopic Science*.

UNDER the title of "The Lazy Lays and Prose Imaginings, written, printed, published, and reviewed by William H. Harrison," of Great Russell Street, the author has published a collection of verse interspersed with short prose pieces partly sentimental but mostly intended apparently to be funny. Scientific men and matters are in one or two cases alluded to, and the imprint bears that the work is published "A.D. 1877 (popular chronology); A.M. 5877 (Torquemada); A.M. 50,800,077 (Huxley)." We believe our readers may derive a little amusement from a perusal of the volume.

THE additions to the Zoological Society's Gardens during the past week include an Anubis Baboon (*Cynocephalus anubis*) from West Africa, presented by Mr. Ward; a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Dr. W. B. Stirling; a Grey Ichneumon (*Herpestes griseus*) from India, presented by Mrs. Henry Jephson Mello; a Central American Agouti (*Dasyprocta isthmica*) from Central America, presented by Mr. A. Stradling; three Sclater's Curassows (*Crax sclateri*) from Paraguay, presented by Mr. Alex. F. Baillie; a Puff Adder (*Vipera arietans*) from South Africa, presented by Mr. A. Biden; a Pike (*Esox lucius*) from British Fresh Waters, presented by Mr. A. D. Bartlett; an Axis Deer (*Cervus axis*) from India, a Three-banded Armadillo (*Tolypeutes conurus*) from La Plata, deposited; a Cape Buffalo (*Bubalus caffer*), two Coatis (*Nasua nasica*), born in the Gardens.

AMERICAN SCIENCE

THE chief signal officer of the U.S. army has been urging that physical observations of the sun be made, as of sun-spots, faculæ, protuberances, &c., in reference to their supposed influence upon terrestrial meteorology, and has offered to publish the results monthly, or such of them as may be considered desirable by the observer, in the *Monthly Weather Review.* The United States Naval Observatory at Washington has already accepted this proposition, and it is considered very desirable that some other observatories in the east, and at least one on the western coast, co-operate in this undertaking.

Dr. C. A. White, palæontologist to the United States Geological and Geographical Survey of the Territories, has spent the past season making a critical study of the mesozoic and cainozoic strata of the great Rocky Mountain Region, and the results have tended to confirm in a remarkably clear manner the statement so often expressed by Dr. Hayden in his annual reports, that the entire series of deposits are consecutive from the Dakota group of cretaceous age below, to the Bridger group of tertiary age above. The sedimentation was evidently continuous through all the changes, from marine to brackish, and from brackish to fresh waters, that successively took place in that great region, although those changes in aqueous conditions produced corresponding changes in the then prevailing forms of invertebrate life.

The annual report of the Board of Regents of the Smithsonian Institution for 1876 has been published, and, as usual, contains a great deal of matter interesting to men of science. The portions of the volume detailing the operations of the institution for 1876 is more especially occupied with an account of what was done in connection with the International Exhibition of 1876, at Philadelphia, and especially of the very extensive and valuable presents made to the United States by the various foreign commissions, and taken charge of by the institution, in accordance with the law of Congress. Reference is made to an application for an appropriation to erect an additional building to accommodate these objects, for which it is estimated that a floor space of 80,000 square feet will be required. Until this is done the collections in question must remain in their original packages, more than 4,000 in number, which are stored on four floors of a separate building, 50 by 100 feet, and filling them completely from floor to ceiling. As usual, the funds of the institution are reported as being in a favourable condition, the income not being exceeded by the expenditure, and an available balance even remaining in hand at the end of the fiscal year. The second part of the volume embraces biographical notices of Dom Pedro II., and also of Gay-Lussac, articles on the kinetic theories of gravitation, the revolutions of the crust of the earth, the asteroids between Mars and Jupiter, and a number of papers on ethnology and archæology. Of these the most important is by Prof. Mason on the Latimer collection of antiquities from Porto Rico, in which the more interesting objects of this unique series are figured. Other papers on ancient mines and mounds, implements of various kinds, &c., are also contained in the volume.

We have to record the death of Mr. Timothy Abbott Conrad, one of the oldest and most accomplished palæontologists of the United States. Mr. Conrad was born in 1803, and commenced his investigations early in the century, beginning with the tertiary and cretaceous formations of the United States. In 1832 he commenced an illustrated work on the "Fossil Shells of the Tertiary Formations of the United States," which was, however, preceded in 1831 by his "American Marine Conchology." Most of his papers appeared in the *American Journal of Science and Arts*, and in the *Proceedings* and *Memoirs* of the Academy of Natural Sciences, Philadelphia. He also contributed largely to the reports of the various government exploring expeditions.

The Nation announces the death of Mr. John G. Anthony, for many years a devoted coadjutor of Agassiz in the Museum of Comparative Zoology at Cambridge, where he had charge of the conchological department. Long residence and extensive travel in the Ohio Valley had made him the first authority in the United States on fresh-water shells. He accompanied the Thayer expedition to Brazil, but sickness prevented him from taking part in it after its arrival. In addition to his special work Mr. Anthony always maintained an interest in Botany and horticulture. He was a native of Rhode Island, and was in the seventy-fourth year of his age.

Prof. Marsh makes the announcement of the interesting discovery of the remains of two species of fossil bison in the lower pliocene of Nebraska and Kansas. They were much larger than the existing bison, with more powerful horns.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—The Vice-Chancellor, Dr. Atkinson, on resigning his office on November 3 (he has been re-elected) spoke of the progress of scientific teaching in the University. The efficiency of the University as a school of natural science has been greatly promoted, Dr. Atkinson stated, during the past year by the erection of the new buildings for the department of comparative anatomy and physiology. Although the whole building is not yet completed, many of the rooms are already in use, and the accommodation which is thus provided for both teachers and students will be of the greatest advantage. In connection with this subject Dr. Atkinson referred to Prof. Clerk Maxwell's announcement that His Grace the Chancellor has now completely equipped the Cavendish Laboratory with all the apparatus and instruments which the professor considers that a first-class institution of this kind ought to possess. This singular munificence, continued so steadily and ungrudgingly for such a number of years, is but one of the many proofs which His Grace is constantly giving of his unwearied care and concern for the welfare of the University.

The following gentlemen have been elected to fellowships at St. John's College:—Arthur Milnes Marshall, B.A., Senior in Natural Science Tripos, 1874, and Donald M'Alister, B.A., Senior Wrangler and First Smith's Prizeman, 1877.

OXFORD.—At a special meeting of the Town Council held at Oxford on Monday it was resolved to establish a first-class grammar school, the Corporation granting a site in the centre of the city of nearly an acre in extent, 4,000*l.* towards the building, and 100*l.* per annum towards its maintenance. There are to be fifty free scholarships tenable for three years, thirty of which are to be filled up from the public elementary schools.

LONDON.—The Council of University College, London, have appointed the Rev. T. G. Bonney, B.D., of St. John's College, Cambridge, Professor of Geology and Mineralogy for five years.

ST. ANDREWS.—Mr. George Chrystal, B.A., Fellow and Lecturer of Corpus Christi College, Cambridge, has been appointed to succeed Prof. Fischer in the chair of mathematics.

Among the names likely to be brought forward by the students for the honorary and honourable post of rector of the University, that of Prof. Tyndall is mentioned.

SCIENTIFIC SERIALS

Morphologisches Jahrbuch, vol. iii. Part 3.—R. Bonnet, on the structure of, and circulation in, the gills of Acephala, pp. 45, three plates.—C. Hasse, fossil vertebræ (the Squatinæ), two plates.—R. Wiedersheim, the skull of Urodeles, pp. 97, five plates; a most valuable memoir on Menobranchus, Siren, Proteus, Amphiuma, Cryptobranchus, Menopoma, Salamandrina, Triton, Axolotl, Plethodon, Spelerpes, Ellipsoglossa, Amblystoma.—M. Fürbringer, on the cephalic skeleton of Cephalopods.

Annalen der Physik und Chemie, No. 9.—On discontinuous liquid motions, by M. Oberbeck.—Explanation of Dufour's and Merget's experiments on the diffusion of vapours, by M. Kundt.—On the diffusion of liquids, by M. Johannisgänz.—On the internal friction of solid bodies, by M. Schmidt.—On the photo-electricity of fluorspar, by M. Hankel.—On the resistance of flames to the galvanic current, by M. Hoppe.—On the electrochemical process at an aluminium anode, by M. Beetz.—Further experiments on galvanic expansion, by M. Exner.—Reply to Zöllner's objections against my electro-dynamic views, by M. Clausius.—On a mode of inference employed by Prof. Tait in the mechanical theory of heat, by M. Clausius.—On the sounding of air in pipes, by M. Ciamician.—The spectrum of nitrous and hyponitric acid, by M. Moser.—On optical illusion, by M. Trappe.

Beiblätter zu den Annalen, &c., No. 8.—On the equilibrium of a drop between two horizontal plates, by M. Bosscha.—On cylindrical sound-waves, by M. Grinwis.—Application of the galvanic current to investigation of the spheroidal state of some liquids, by M. Hesehus.—On the tenacity of copper and steel, by MM. Pisati and Saporita Ricca.—On the polymorphism of crystals, by M. Moutier.—The heat of solution of chlorine, bromine, and iodine compounds, by M. Thomsen.—New

saccharimeter, by M. Laurent.—Lecture experiment on the colour-change of certain double iodides, by M. Boettger.

No. 9.—On physical isomerism, by M. Lehmann.—On the elasticity of gypsum and mica, by M. Coromilas.—On the influence of pressure on the temperature at which water shows a maximum density, by M. Van der Waals.—Apparatus for measurement of the expansion of rigid bodies by heat, by M. Reusch.

SOCIETIES AND ACADEMIES

LONDON

Chemical Society, November 1.—Dr. Gladstone in the chair.—The following papers were read:—On some hydrocarbons obtained from the homologues of cinnamic acid, by W. H. Perkin. These hydrocarbons were prepared either by heating the acids or by treating the hydrobromo acids with bases. The following acids were prepared and examined:—Hydrobromocinnenylacrylic, hydrobromocinnenylcrotonic, hydrobromocinnenylangelic. The following hydrocarbons were obtained:—Isopropylvinylbenzene, isopropylallylbenzene, isopropylbutenylbenzene, allylbenzene, and butenylbenzene; the dibromides of these bodies were also prepared and examined.—On anethol and its homologues, by W. H. Perkin. By heating methylparoxyphenylacrylic acid, vinylic anethol was obtained, similarly allylic or ordinary anethol and butenylic anethol were prepared. In conclusion the author discusses the formation of the hydrocarbons from the hydrobromo acids, and concludes that they are formed simply by the separation of hydrobromic acid and carbonic anhydride.—On two new methods for estimating bismuth volumetrically, by M. M. P. Muir. To a solution of bismuth in nitric acid an excess of sodium acetate is added, and then a measured volume of standard sodium phosphate solution also in excess; the bismuth is precipitated, the precipitate filtered off, and the excess of phosphoric acid determined in the filtrate by uranium acetate. The other method given does not yield such accurate results.—On the oxidation of ditolyl, by T. Carnelly. By the oxidation of solid ditolyl the author obtained diparatolylphenylcarbonic acid and diparadiphenyldicarbonic acid; liquid ditolyl yielded orthoparatolylphenylcarbonic acid, orthoparadiphenylcarbonic acid, and finally terephthallic acid.—On a new manganese reaction, by J. B. Hannay. If a solution of a manganous salt in strong nitric acid is warmed in the presence of an iron salt with some crystals of potassic chlorate, the iron and manganese are precipitated as a double manganate of iron and manganese. The author proposes this reaction for separating iron from aluminium, &c.

PARIS

Academy of Sciences, October 29.—M. Peligot in the chair.—The following papers were read:—*Résumé* of a history of matter (second article), by M. Chevreul.—On the solar photospheric system, by M. Janssen.—The telephone of Mr. Graham Bell, by M. Breguet.—On the determination of the quantity of mud contained in current water, by M. Bouquet de la Grye. He uses an instrument named a pelometer (πῆλος, mud), consisting of a V-shaped vessel whose rectangular faces, inclined one-tenth, are of thin glass, while its sides are of copper or white iron. One glass face has a centimetre scale commencing from the angle. The pelometer is filled and held vertical; it then presents a succession of vertical layers of increasing thickness upwards, and, by comparison with glass-ended tubes containing muddy water of various known densities, the proportion of mud may be ascertained. Other methods are given. Experiments made at Rochelle show that the quantity of mud per litre varies from one to ten times according to the depth. He considers regular measurements of the kind on watercourses desirable for agriculture, &c.—On an American vine-stock not attacked by phylloxera, by M. Fabre. This vine belongs to the species *Riparia*. Among other merits (besides its immunity) it gives cuttings readily, receives grafts from French species better than any other American variety, thrives in most arid soils, compact clays, and soils long exhausted by vine cultivation, and grows very rapidly.—On treatment of phylloxerised vines with sulphocarbonate of potassium applied with the distributing pail, in 1876-77, by M. Gueyraud.—Observations of the planet (175) Palisa, made at the Paris Observatory, with the west equatorial of the Garden, by MM. Paul and Prosper Henry.—Stellar systems of 36 Ophiuchus and 40 Eridan, by M. Flammarion.—General form of coefficients of certain developments, by M. André.—New mode of plane representation of classes of graduated surfaces, by M. Mannheim.—Experiments on the disruptive discharge made with the chloride of silver battery by MM. Warren de la Rue and H. W. Müller.—Rheostatic machine, by M. Planté. He combines a number of condensers (made of mica and tin), so as to be easily charged, from a secondary battery, in *quantity* and discharged in tension. The commutator is a long cylinder of hardened caoutchouc, having longitudinal metallic bands, and traversed by bent copper wire (for the two objects named). Metallic springs are connected with the two armatures of each condenser, and fixed on an ebonite plate on each side of the cylinder, which is rotated. A series of sparks can be got between the branches of the exciter in this arrangement, quite like those from electric machines with condensers. The discharges are always in the same direction, and the loss of force is less than in induction apparatus. A great many discharges can be had without the secondary battery being perceptibly weakened, as each discharge removes only a very small quantity of electricity.—On semi-diurnal barometric variations, by M. de Parville. The tropical hours may present, at a few days' interval, divergences amounting, during the great period, to forty-five minutes. The barometric variations in the tropical hours are not uniform; the maximum of descent of the mercury column occurs about three o'clock. Equality between the periods of day and night has never occurred. The amplitude of the variation is greater by day than by night, and during the dry season than during the wet.—On the action of anhydrous acids on anhydrous bases, by M. Bechamp. They are capable of uniting wholly.—On the determination of reducing sugar contained in commercial products, by M. Girard.—On the reducing sugar of commercial products in its relations to saccharimetry, by M. Morin. He shows the optical inactivity of this sugar.—On the production of racemic acid in the manufacture of tartaric acid, by M. Jungfleisch.—On some physical properties of *quercite*, by M. Prunier.—Action of solar light with variable degrees of intensity on the vine, by M. Macagno. Diminution of intensity hinders the production of glucose; the other elements (produced or assimilated) are in direct ratio of the luminous intensity. A portion only of potash is in inverse ratio of the luminous intensity: the contrary occurs in the case of potash combined with tartaric acid.—On the Orthonectida, a new class of animal parasites of Echinoderms and Turbellaria, by M. Giard.—On the calcareous algæ belonging to the group of verticillate Siphoneæ (Dasycladeæ, *Harv.*), and confounded with the Foraminiferæ, by M. Munier Chalmas.—Effects of faradisation in a case of hydrophobia in man, by M. Menesson. Considerable sedative effects were obtained; the patient, however, died after two days through a sudden stoppage of the heart's contractions.

CONTENTS

THURSDAY, NOVEMBER 15, 1877

BREHM'S THIERLEBEN

Die Säugethiere, von Dr. A. E. Brehm, 2 vols.; and *Die Insekten*, von Dr. E. Taschenberg, 1 vol. (Leipzig: Verlag des bibliographischen Instituts, 1877.)

THE three fine volumes of Natural History with the above titles form part of Brehm's well-known "Thierleben," a book which has had a well-merited success in Germany and on the Continent generally. The volumes on the mammalia, of which those under consideration are new and enlarged editions, and which contain the Quadrumana, Chiroptera, Carnivora, Insectivora, Rodentia, Edentata, Marsupialia, and Monotremes, had been well appreciated in this country on account of its excellent illustrations, trustworthy anecdotes, and general literary merits. The new edition issued in 1876 surpasses the old, for not only are the additional and new drawings of first class art and most truthful, but much of the context is more decidedly scientific than before. Brehm, with his practical knowledge of animals, especially of some important groups, and his literary powers and judicious choice of illustrative anecdote, was just the man to undertake a popular natural history; and the success of the very bulky attempt not only is greatly to his credit, but is a testimony of the love of good reading amongst the mass of his countrymen. A familiarly written introduction on the structure and physiology of the Mammalia occcupies the first chapter, and then the Primates are considered. There are two plates of sitting, standing, jumping, climbing, and swinging Anthropomorpha which are exquisite, and evidently carefully taken from the life. The rare Troglodytes Tschiego, the *Nsehiego mbouvé* anatomised by Duvernay, is delineated in four attitudes, and the reach of the fingers below the knee is well shown; below it are three capital chimpanzees, but none of them equalling Wolf's admirable swinging chimpanzee in the possession of the Zoological Society of London. The orangs with their globular heads, projecting lips, and hirsute arms, are drawn with great force, and three gibbons, spidery and dangling, complete the show. A plate of hands and feet illustrates this part of the book, and the transition from the highest hand, probably that of *T. tschiego*, for it is more symmetrical than that of gorilla, to the lowest paw amongst the American marmosets is admirably shown. The dwindling of the thumb, the gradual equalisation in length of the three middle fingers, and the march first on the knuckles, and then, in the lower groups, on the palms are carefully demonstrated. Osteological anatomy is not much cared for, and therefore the skeleton of gorilla is not worthy of the book; but in the chapter on this great ape there are some very remarkable plates which enable us to approach the truth.

The chimpanzee comes next—and oh! there is such a sly-faced fellow in a woodcut, utterly beyond the capacity of any British cutter on wood—illustrating the genial species, after which, instead of before, as one would think ought to have been done, comes Du Chaillu's bald Troglodyte, the *T. tschiego*. With regard to this little-known beast, Brehm gives some more information about its size and general zoology, but he does not enlarge on the *Troglodytes aubryi* of Gratiolet and Alix. A group of Entellus monkeys, with their forehead tufts rather exaggerated, illustrates in part the few pages on the Semnopitheci, and the Macaci are finely delineated, a life-like savagery being given to the Rhesus and pig-tailed kinds. Brehm's anecdotes about the baboons are first-rate, and although the drawings of Hamadryas and the mandrill are slightly in exaggeration of what is common, they give a capital idea of the impudence of the one and the brutality of the other.

Brehm has collected all the good anecdotes and descriptions from Humboldt, Schomburgk, Rennger, and Henfel about the Howlers, and in spite of the silence of those in captivity in Europe we can imagine the terrific noise of a tree full of the adults of *Mycetes caraya*. Bartlett is fully and deservedly quoted in illustration of the natural history of the spider monkeys, and the delineations of the group are fairly done, ease of position being often wanting in the illustrations, however. The short-tailed Brachyures are for once described and drawn not in caricature; the context is mainly from Bates, and the sketch looks like a monkey and not like the distressed old man with a tail like an American vegetable marrow which is usually given in books. The Saimaris are introduced under the generic title Pithesciurus to which, and indeed to much of Brehm's zoological nomenclature, we demur. The marmosets are grouped, as by Huxley, as Arctopitheci, a name given to some sloths by Gray, and there is much confusion in introducing new generic terms to the genera Hapale and Midas. The use of the term Leopithecus for Hapale, of Hapale for Midas, for instance, shows the necessity for a final zoological arrangement of these small monkeys. No less than 239 pages are devoted to the apes and monkeys, and then the Lemuroida are commenced under the old-fashioned terms Hemipitheci or Prosimii. Sonnerat, Pollen, and Peters are Brehm's authorities for the natural history of this group, and he does not appear to have had the advantage of studying Mivart, Murie, Grandidier, and Owen; nevertheless the article is of good scientific value and of course the illustrations are superexcellent. There is, however, the old jumble of synonyms for the genus Indris, and Lichanotus and Propithecus are put in most unadvisedly. The queer Stenops, oddly named *gracilis*, Galago with his ears on the move and a bogie of *Tarsius spectrum*—if it were as big as a man how truly hobgoblin-like it would be—are excellent pictures. There is nothing new, however, about the group, and really more good information on anatomical and physiological subjects might have been given without detracting from the popular nature of the book. The Chiroptera are arranged in rather an old-fashioned manner, and are rather curtly treated; and then the second part of the volume opens with the Carnivora, to the exclusion of the Insectivora. The lion of course comes first, and although there is nothing to notice particularly in the context, every one must admire *Leo capensis* and the lioness of *Leo senegalensis*, although the specific determinations should sink into those of varieties. The sequence of species then becomes rather strange to English natural history, the puma and *Felis eyra* preceding "*Tigris regalis*" or *Felis tigris*. The clouded

tiger comes next as "Neofelis," and the illustration is hardly that of the short-legged meek-looking creature in the Zoological Gardens. The jaguar, as drawn in the next page is too long in the neck, but the rounded top to the head is well given; the anecdotes and general history of this fine South American beast are beyond ordinary praise.

Brehm has paid unusual attention to the smaller cats, and the pages devoted to them are amongst the most interesting in the book, and when telling of the lynx, he gives a wood engraving by Beckmann of the common form which is wonderful in its details of face-expression and fur (p. 490). The Cheetahs, so interestingly numerous just now in our Zoological Gardens, are fully considered, and in the illustration there is the upward whisk of the tail given to perfection, but the muzzle of the beast is too long, and the fore legs hardly long and stilty enough. Cryptoprocta concludes the Felidæ, and *Canis primævus* of Cashmir commences a most interesting article on the dogs. Amongst other beauties there is "Der Bulldogg oder Boxer," and Mr. Bill Sykes would have been surprised to have learnt that it is called *Canis familiaris molossus gladiator*. It is "ein wüthendes, unzugängliches und stumpfsinniges Thier." Then there is its relation, *Mops*, with its sharply curled-up tail and black short nose, the tiny tongue tip not, however, being shown in the engraving, which tells the ladies of the period that Pug's real name is *Canis familiaris molossus fricator*. Amongst other dogs a sketch of a pointer by Beckmann is capital; he is pointing, and just a little in doubt, the tail dropping slightly and the head being not over-expectant. The first volume concludes with the natural history of *Otocyon* and *Canis procyonides*.

The second volume commences with a notice of the hyænas, and although there is not much to be said in praise of this contribution to their literature, still the delineations of *H. crocuta* and *H. brunnea* place the distinctions between the species plainly. *H. crocuta* is admirably drawn and the artist has managed to give it the peculiar weak look of the hind legs and drooping quarters of the caged animal. The Viverridæ are shortly treated, and one of the few doubtful drawings of the work is in illustration of *Cynogale bennettii*. The genus *Herpestes*, the habits of some of whose species have taxed the imagination of Europeans as well as that of Eastern races, follows; it is judiciously described and the anecdotes are good. The fur-yielding martens and their allies and other small carnivora valuable to the furrier are well illustrated, but Brehm had not the valuable volume on their natural history, lately issued by the American Survey, to learn from. The bears form a very interesting part of the book, but many of the illustrations have the positions of the animals rendered awkward by the attempt to give prominence to specific and peculiar structural points. Thus the polar bear in the water is wretchedly done, thanks to the endeavour to render the claws and narrowish snout very definitely comprehensible. The moles and hedgehogs are fairly noticed, but want of space begins to affect the treatment of these lower groups, but Galeopithecus, very shortly described, is properly placed at the head of the Insectivora. The Rodentia are of course full of anecdote and light literature, but Brehm's illustrations are by no means as good as those of the other groups; perhaps the most striking is that of *Cercolabes prehensilis*. A capital plate of the sloth shows the short snout which almost looks moist, and for once in a way amongst books of this kind, there is a truthful rendering of the long narrow wrist with its two claws. The essay on the sloths and ant-eaters is admirable, but the anxiety to show the peculiar progression of the last group, on the anterior extremities and the position of the claws, has often led the artist to exaggerate. The Marsupials are well illustrated and with great ability, but we miss some of Gould's most life-like sketches so familiar in most popular works. The pages devoted to the Monotremes contain the usual stories, and unfortunately were written before those important additions to their natural history were published, and which have lately been noticed in NATURE.

The other volume (the ninth of the work) before us is by Taschenberg, of Halle, and is a second edition of the part containing the Insecta, Myriopoda, and Arachnida. The species representing groups are of course well chosen as types, and the author has often taken pains to place novelties before the reader, especially in the way of illustration. The short anatomical introduction is sufficient for the general reader, but barely so for the young student. Amongst unusual forms, or rather unusual to the common routine of book making, is *Mormolyce phyllodes*, from the upper hill country of Java, with its wide leaf-looking elytra and long antennæ, and the very common and opposite-looking *Scarites pyracmon*. The burying propensities of Necrophorus are told and illustrated, and there is a very curious and striking plate of a mole hanging by the neck in a trap, with a crowd of Silphidæ (shield beetles) and larvæ, besides blow-flies, on and about it, doing their best to turn its protoplasm into theirs. Some pests to museum-keepers and housekeepers are especially figured in the act of working away at a hare's foot which rests on pen, ink, and paper *Anthrenus musærum* larva and adult, *Attagenus pellio* and *Dermestes lardarius* are there in full enjoyment of their mischievous propensities. The natural history of Lampyris, Meloe, and Sitaris, is cut too short, doubtless for want of space, but their interesting life cycles merit more attention than that of many others which are barely more than mentioned by name and might be left out. *Apoderus longicollis*, a Javanese species looking like a cameleopard amongst beetles, and unfortunately little known, has an interesting engraving; and equally good is that of the langkäfer Brenthus. Amongst the Hymenoptera the habits and nests of *Bombus terrestris*, of *Odynerus parietum*, and of the curious Belonogaster and the Sandwasp are very well explained and drawn; and great praise must be given to the delineations of the life cycle of Othalia and Cimbex.

The only fault to be found in the treatment of the Lepidoptera is that the article is too short, but the illustrations are very good. A plate of a rush of a myriad of the maggots of *Sciara militaris* is a strange subject, but very effective, and the long crowd of closely-packed dark-headed long things looks as if short work was to be made of carrion. Amongst the leaf insects there is *Mantis religiosa* preying rather than praying with a fly in its clasp, and a host of larvæ escaping from a mass of eggs; and there is an equally interesting cut of *Bacillus Rossii*, one of the Phasmodea. A short chapter on the unsavoury subject of Pediculi precedes a sketch of the Cochineal

insects, and then, after noticing the Chermes that attacks that very strong food the larch, we come to a full description of *Phylloxera vastatrix*. The Hemiptera are shortly mentioned, and then the Myriopoda. There is a good picture of Geophilus clinging around its great prey, a large earthworm, and also of a Polydesmus. Amongst the Scorpions the long-armed Phrynus and Gonoleptes, and amongst the Spiders a long Tetragnatha and the extraordinary-bellied Gasteracantha, form admirable illustrations. A short chapter on Pycnogonum and Nymphon concludes this really wonderful volume. P. M. D.

OUR BOOK SHELF

Heat. By B. Loewy (Lardner's Handbook of Natural Philosophy. Crosby Lockwood and Co., 1877.)

THIS, though not a bulky book, is a sort of miniature Encyclopædia of the subject. So far as we have read it it seems to have all the faults of the original (?) work to which Lardner's name was prefixed, with the important exception of the inaccuracies. These have been to a great extent removed, and the work has been brought up to date, but there is still the woeful want of order, or indeed of any guiding principle whatever which distinguished the former editions. It is a very curious mixture of good and bad, and cannot be called, in any sense, attractive to the reader. Numerous tables of experimental data are given, but they are in many cases carried to a number of places of figures quite beyond the present power of experimental science. Two, or perhaps three of the figures in the earlier places of each number are probably correct; the others give a show of minute accuracy which may altogether deceive the beginner. The treatment of the theoretical part is very meagre, but in the experimental part many curious facts not usually known are given. The book may be useful as a work of reference to those who are not in possession of Balfour Stewart's treatise, but we cannot say more in its favour.

Ferns, British and Foreign. The History, Organography, Classification, and Enumeration of the Species of Garden Ferns, with a Treatise on their Cultivation. By John Smith, A.L.S., Ex-Curator of the Royal Gardens, Kew. New and Enlarged Edition. (London: Hardwicke and Bogue, 1877.)

THAT Mr. Smith's "Ferns, British and Foreign" should have reached a new edition in a comparatively short time is no small tribute to its value as a book of reference for amateurs and fern cultivators. The chief portion of this very neatly got up work is occupied by an enumeration of cultivated ferns. The different genera, as understood by the author, who was one of the foremost pteridologists of his day, are described and figured, while a list of the cultivated forms, with synonyms and range of geographical distribution, follow under each genus, no attempt being made to give a diagnosis of the species. The scope of the work is therefore entirely different from that of the "Synopsis Filicum" of Hooker and Baker. The classification adopted is that propounded by Mr. Smith in his early publication on ferns, an arrangement not much used by modern writers. An appendix of recently-introduced ferns is given. These have been collected and arranged under their respective genera and tribes, as their names have from time to time been noticed in the horticultural journals and in nurserymen's catalogues. The list has thus no pretensions to be a critical one. The most interesting part of the book is the history of the introduction of exotic ferns, a subject about which, probably, no man living knows more than Mr. Smith. This is followed by an explanation of terms used in describing ferns, perhaps the least satisfactory part of the whole volume, as many of the terms are more or less obsolete, or only used in the book now before us. In this section nothing is said about the recent researches into the nature of the prothallus, construction of the reproductive organs, and morphological nature of the sporangia. The last part of the work is occupied by an essay on the cultivation of ferns, reprinted without alteration from the first edition, but giving the results of long experience of the successful cultivation of all groups of ferns. As a work of reference and guide to the cultivation, this book will most undoubtedly be of great service to the fern-growing public.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

The Radiometer and its Lessons

I HAVE little doubt that Prof. Osborne Reynolds is much more competent than I am to say what is or is not consistent with the kinetic theory of gases, but I hardly think that he gives evidence of this in his letter to last week's NATURE (p. 27). Unless my ignorance of the matter is more complete than I am aware of, the law that the rate of communication of heat to a gas is independent of the density, applies only when the space occupied by the gas is so great, or the variations of density so small, that these variations do not alter the temperatures of those portions of the gas which are at each instant respectively receiving and giving out heat. This condition cannot, I imagine, be fulfilled in the radiometer, where it seems to me inevitable that an action of the kind to which Mr. Johnstone Stoney called attention must take place. G. CAREY FOSTER

P.S.—Since writing my previous letter to NATURE, a fortnight ago, I have read a paper by Mr. R. Finkener, in *Poggendorff's Annalen* (vol. clviii. pp. 572–595). This paper contains, besides a theoretical investigation of the motion of the radiometer founded on the kinetic theory of gases, an experimental proof that the action becomes much less when an extremely high degree of rarefaction is reached. The paper itself is not dated, but, as the Part of the *Annalen* which contains it was "closed" on July 31, 1876, the experiments described in it cannot have been much, if at all, subsequent to those (communicated to the Royal Society, June 13, 1876) which led Mr. Crookes to a like result. G. C. F.

UNTIL I read Dr. Carpenter's letter in your issue of the 8th inst., it had never occurred to me that his "special purpose" was to bring out strongly my "thoroughly scientific and philosophical method!" This is an act of disinterested kindness which recalls to me the exquisite truth of Dean Swift's remark, "No enemy can match a friend.'

Dr. Carpenter's only reply to my letter which appeared in your issue of the 1st inst. is contained in the following passage:—"If I had not found," he says, "after the publication of my Lectures, that he had himself been 'digging up the hatchet,' which I was quite disposed to keep buried, by giving his public attestation to the 'spiritualistic' genuineness of what had been proved to be a most barefaced imposture, I should not have again brought his name into the controversy."

Further on Dr. Carpenter paraphrases passages from his article in *Fraser's Magazine* for this month, in which he goes more into detail touching this "public attestation," of which in his eyes I stand accused.

"Eva Fay," he says, "returned to the United States, carrying with her a letter from Mr. Crookes, which set forth that since doubts had been thrown on the Spiritualistic nature of her 'manifestations,' and since he in common with other Fellows of the Royal Society had satisfied himself of their genuineness by 'scientific tests,' he willingly gave her the benefit of his attestation. This letter was published in *facsimile* in American newspapers."

My answer to this calumny shall be brief.

It is untrue that I dug up the hatchet—Dr. Carpenter's

expression—in the interval between November 30, 1875, when he proposed it should be buried, and the time of his first subsequent attack upon me.

It is untrue that during this interval, or at any other time, I gave my "public attestation to the spiritualistic genuineness of what had been proved to be a most barefaced imposture."

It is untrue that I gave Eva Fay a letter, speaking of the "Spiritualistic nature of her manifestations," and referring to "Fellows of the Royal Society."

It is untrue that Eva Fay "returned to the United States carrying with her" such a letter.

It is untrue that "this letter was published in *facsimile* in American newspapers."

When Dr. Carpenter limits himself to definite statements, my task is not difficult. It is, however, less easy to answer a *rumour* of *something* which *somebody* told Dr. Carpenter I *privately* admitted.

"It has been rumoured," says Dr. Carpenter, in *Fraser's Magazine*, "that Mr. Crookes has *privately* admitted that some of his 'mediums,' when they could not evoke the 'manifestations' by *fair* means, have done so by *foul*."

I admit that such a rumour respecting Eva Fay was circulated in the United States, and a Boston gentleman wrote and asked me if there was any truth in this statement. I replied as follows under date November 8, 1875:—

"In reply to your favour of October 25, which I have received this morning, I beg to state that *no one* has any authority from me to state that I have any doubts of Mrs. Fay's mediumship. The published accounts of the test *séances* which took place at my house are the best evidence which I can give of my belief in Mrs. Fay's powers. I should be sorry to find that any such rumours as you mention should injure Mrs. Fay, whom I always found most ready to submit to any conditions I thought fit to propose."

Considering that this was a private letter from one gentleman to another, written *currente calamo* without any thought of subsequent publication, few of your readers, I believe, will see much harm in it. Not being aware that private communications were less sacred in America than in England, I was certainly surprised one morning to receive a copy of an American newspaper containing a *facsimile* of this private letter.

It will be observed that this letter is dated November 8, 1875, whereas the "bury-the-hatchet" episode took place on November 30, 1875; this therefore cannot be the letter which convicts me of attesting to a "barefaced imposture" subsequent to November 30.

Moreover, this letter does not contain the words "Spiritualistic nature of her manifestations." Neither does it allude to "Fellows of the Royal Society." Nor did Eva Fay return to "the United States, carrying with her this letter." Nor was it even addressed to Eva Fay. It is then impossible that this can be the letter to which Dr. Carpenter refers, and I demand that he prove the truth of his allegation by producing a copy of the "American newspapers" containing a *facsimile* of a letter written by me answering his description, containing the words which he professes to quote from it, and justifying his defamatory remarks.

In your issue of last week (p. 26) Dr. Carpenter says nothing about this *facsimile* letter, but lays stress on an article written by me ten months previously. Does he seriously mean that the publication in March, 1875, of an account of some test experiments is a breach on my part of his "bury-the-hatchet" offer made the following November?

I have evidently been labouring under a misapprehension as to what Dr. Carpenter meant when he proposed to "bury the hatchet." I supposed he intended to cease misrepresenting my views and falsifying my experiments at his public lectures, and never afterwards to repeat such calumnies on my scientific position as he had anonymously contributed to the *Quarterly Review* for October, 1871. It seems, however, that Dr. Carpenter really meant that I was no longer to go poaching on his own special preserve, and was to abstain for the rest of my life from writing even a private letter on a subject which he has investigated for more than thirty years, and about which he is now writing and lecturing with redoubled vigour.

Dr. Carpenter refers to an offer made in May, 1875, "by Eva Fay's manager, that for an adequate sum of money the 'medium' should expose the whole affair," and he vouches for its truth by saying he has seen "copies of the letters." I can supply, not *copies*, but *original* letters. I have before me letters from Eva Fay, dated Birmingham, May, 1875, speaking bitterly of the temptations and persecution to which she was being subjected to induce her to join in the scheme, to which she was no party.

But how, may I ask, does an abortive conspiracy to complicate "*six big guns*" prove that my "scientific tests"—which with all deference to Dr. Carpenter's "good authority" can *not* be evaded by a "dodge"—were useless, and that in spite of them Eva Fay cheated me?

I am weary of protesting against the imputation which Dr. Carpenter conveys in the words "scientific advocates of the system." I emphatically deny that I have ever advocated any "system" in connection with the phenomena ascribed to spiritualism. I have never for one moment doubted that this name has covered an enormous mass of fraud and trickery; but being convinced that amidst all this falsehood—which it is Dr. Carpenter's mission to denounce in the most fervid eloquence at command—there was a solid nucleus of fact, and believing that every unrecognised fact is a reproach to science, and every uninvestigated phenomenon is a probable mine of discovery, I considered myself not merely entitled, but almost bound in scientific honour, to attempt the solution of the question. My attempt to bring the so-called supernatural within the realm of fact, to turn the light of science on a problem that required investigation, has exposed me to many misrepresentations, but especially to the ire of Dr. Carpenter, who never tires of repeating every idle tale from hearsay evidence. I look back to the days of the alchemists, and find the very same kind of fraud, mysticism, and trickery, differing but little from the impostures of the present day. But then, as now, there were a few earnest students who detected the germs of truth amidst the ravings and juggleries of the gold makers; they cherished these germs, and although the Dr. Carpenter of that period would doubtless have denounced those students as "scientific advocates of the system" of alchemy, and felt it his duty to "undermine" their reputations, they persevered through calumny and ridicule, and thereby laid the foundations of our modern science of chemistry.

The readers of NATURE have now before them ample illustration of the falsity of the accusations with which I have been persecuted for so many years. A calumny once born, said the Great Napoleon, can never be killed. I have, however, done my utmost to prove the groundlessness of the very serious charges Dr. Carpenter has brought against me, down to the grave indictments in your issue of last week (p. 26). There is not a single charge which Dr. Carpenter has ever brought against me that I am unable to answer with like completeness; and, judging from long experience, I venture to say that any future charges he may bring will be equally unfounded. But I cannot, out of regard for your readers, to say nothing of the sacrifice of time, continue to defend myself from every petty accusation; and unless really forced by some imputation too serious to pass over, this must be my last letter on a subject which to me involves painfully too much self-reference. I have been constrained, in self-defence, to speak in somewhat downright fashion, but Dr. Carpenter's industrious misconstruction has drawn this protest from me. Misstatements expressed in a few lines may require pages to refute them. A calumny which takes a minute to write may demand days to answer. Memories of half-forgotten occurrences have to be revived, conversations recalled, old letters hunted out, journals exhumed, and, in fact, as much time and trouble expended as if getting up evidence for an important legal trial. So great a tax for so trivial a purpose is monstrous in its disproportion, and I can waste on this fruitless discussion no more precious time—time stolen from my physical work in the laboratory, already too much curtailed by the pressure of outward business.

November 10 WILLIAM CROOKES

THE latter half of Dr. Carpenter's letter in last week's NATURE (p. 26) consists of almost verbatim extracts from his article in this month's *Fraser*. I beg to refer your readers to a reply to Dr. Carpenter's attack, and a full exposure of his false accusations against Mr. Crookes and myself, which will appear in the next issue of that magazine. They will then see *who* has been led by "prepossession" to adopt "methods which are thoroughly *unscientific*," and *whose* are "the statements which ought to be rejected as completely untrustworthy."

ALFRED R. WALLACE

Experiment on Fluid Films

I AM experimenting on the optical phenomena exhibited by thin fluid films in a state of vibration, and have just obtained

some beautiful results, including the formation of fixed straight and curved coloured bands, arranged in symmetrical figures, and of pairs of colour-vortices rotating in opposite directions.

Unless these results prove to have been already described, I shall shortly publish an account of my experiments.

SEDLEY TAYLOR

Trinity College, Cambridge, November 12

Expected High Tides

IN your "Notes" last week you say that you cannot understand why the burden of such predictions should fall solely upon Capt. Saxby. This is what many of the public also do not understand. Why does not, say, the Meteorological Office take the matter in hand, and not leave it to some private individual? There can be no doubt the forewarnings are often of the greatest service and have saved the public tens of thousands of pounds and prevented a great deal of misery. What I think Capt. Saxby is to be blamed for is the desire—it may be only apparent—to make a mystery of his predictions with the general public; and what gives weight to this is the fact that the Astronomer-Royal and the heads of the Meteorological Office and Society do not offer the public any aid in what is a purely scientific and eminently practical subject, in which Londoners are more interested than in the transit of Venus, and quite as much as in the storm-warnings for the Channel.

When in March, 1874, Capt. Saxby came forward and in an oracular way predicted a great tide on the 20th, he gave no reasons. This many felt was unsatisfactory. Knowing that it must result from the action of natural laws curiosity led me to investigate the matter, and I found that the subject of extraordinary tides was a matter of much simplicity; that the chief factors reside in the moon with its varying distances and declinations; the next in the sun and the seasons; the next in the winds and atmosphere; and the next, perhaps, in the action of the planets, as Venus and Jupiter, the former of which we know affects the orbit of the earth. and both have probably some power in producing the atmospheric disturbances in the sun.

With these factors I predicted a year in advance the extraordinary tide of November, 1875, which had escaped Capt. Saxby's notice. I was also able to say that there are two unusually high tides revolving through the year, exactly six-and-a-half synodic months apart, each forty-eight days after the same tide of the previous year; that these with the preceding and succeeding tides are chiefly those which may with bad weather develop into extraordinary ones; and that the next great one—a very giant among tides—will be on March 20, 1878.

If Capt. Saxby has some knowledge on the subject which others have not, how is it he did not predict the unusually high tide of October 26 last, which happened when the moon was neither full nor new, nor in perigee? Why it happened is somewhat of a mystery; the only explanation I can suggest is, that the moon had her highest northern declination on that day, and that a barometric depression passed over the North Sea the previous day, both which would tend to heighten the tide.

November 12 B. G. JENKINS

The Towering of Wounded Birds

LAST season I fired at a song thrush at a distance of fifty yards, but the bird continued its course, as if uninjured, for upwards of 200 yards, when it suddenly "towered" in the air, and as suddenly fell to the ground. Upon examination the bird was found to have been shot through the lungs alone, and had bled internally, the throat being full of clotted blood. The head was totally free from any injury. I have known similar instances occur in the pigeon, swallow, and starling. In all these cases the head remained uninjured, and death occurred through internal hæmorrhage. In the case of the starling one pellet entered the spine; the bird continued its course for a few yards, towered, and suddenly fell to the ground dead.

Should you consider these instances bearing on the matter of sufficient importance for an insertion in NATURE they may prove acceptable to those who are interested in the subject.

Heeley, near Sheffield CHARLES DIXON

Cruelty to Animals' Act and Physiological Teaching

I AM desirous of knowing through your many readers if, amongst physiologists, the belief is anything like general, that showing under the microscope the circulation of the blood in a web of a frog's foot is a contravention of "The Cruelty to Animals' Act, 1876."

Dr. M. Foster, in his "Primer of Physiology" (Macmillan and Co., 1877), advises the reader to "go and look at it at once; you will never know any physiology till you do;" and some naturalists here say if no incision is made, the animal being merely tied down, the exhibition of the phenomenon is quite legitimate, while, on the other hand, Prof. Huxley, in his paper before the Domestic Economy Congress (reported in NATURE, vol. xvi. p. 234) states it as his opinion that a teacher is "open to the penalty of fine and imprisonment if he *uses*" a frog "for the purpose of exhibiting one of the most beautiful and instructive of physiological spectacles."

It was this, the expressed opinion of so distinguished an authority as Prof. Huxley, which caused me first to doubt the teacher's right to exhibit the experiment, and it is because of the differences of opinion I have mentioned that I seek to know through your columns, if a teacher is or is not at liberty to illustrate the blood circulation by this harmless experiment.

FRANK W. YOUNG

High School, Dundee, November 12

Smell and Hearing in Moths

NUMBERS of moths, of many different species, are attracted into my room on summer evenings by the light; and they are fond of resting on the lamp shade. One night I was using some very strong ammonia solution—and by way of driving them off I held a 3-ounce bottle of it with the open mouth almost close to them. To my surprise they seemed quite unconscious of it *as a smell*; they would bear it unmoved for a minute, or sometimes for two or three minutes, and they then merely walked an inch or two further away from it. I have since tried the experiment repeatedly, and with several different species; but none of them seem to detect the presence of ammonia except as a man might detect the presence of carbonic acid or of nitrogen in excess, that is, by their effects on his system generally.

The common black and white "magpie moth," it is well known, often feigns death when captured. I caught two, one after the other; both pretended to be dead, and I laid them gently on the table a few inches apart. They had remained motionless for ten minutes, when I took up a wine glass by the stem, and gave it one sharp stroke with a pencil, about six inches away from them. Both moths flew off at the instant the sound was heard. I repeated this many times with the same result—both with these and with other individuals of the same species; and I also found that merely holding the glass near them and waving the pencil about noiselessly, did not arouse them.

Loughton J. C.

Bees Killed by Tritoma

IN a friend's garden here where there are quantities of Tritomas or "red-hot-pokers," hundreds of bees have been this year destroyed by them. The honey produced by the flower is very abundant, and the bees enter the tube of the corolla to get at it; but the tube, which is only just large enough at the mouth, tapers gradually, and so the bee gets wedged in and cannot extricate itself. I saw numbers so caught, some in the fresh flower, while others remained in the completely withered and decaying blossoms. Perhaps it may be due to the fine warm days we have had this autumn, inducing the bees to work too late after our native honey-producing flowers have been destroyed by the wet and frosts; or is it a regular thing which happens every year? If so bee-keepers should discourage the Tritoma, or set to work to select varieties with flowers large enough not to kill their bees.

ALFRED R. WALLACE

Dorking, November 3

Lecture Experiment

THE experiment described below illustrates in a very striking manner the particular instance of the "conservation of energy" exhibited by the equilibrium of liquids of unequal densities, in communicating vessels.

The apparatus consists of a two-necked bottle, having in one neck a very strong glass tube half a metre, or more, in length, and terminating above in a funnel of 200 c.c. capacity, while its lower end nearly reaches the bottom of the bottle; in the other neck is a piece of glass tube, drawn to a jet, and passing only a short distance into the bottle. As the pressure inside the appa-

ratus is considerable, the corks by which these tubes are fixed must fit very tightly.

In using the arrangement the bottle is filled with water, the jet is then closed with the finger, and the funnel, which should be supported on the ring of a retort stand, is filled with mercury; on removing the finger from the jet the mercury falls into the bottle, expelling the water which rises in a fountain to a height depending upon that of the column of mercury, but rather less than is theoretically possible, the height of the fountain being ten or eleven times that of the fall of mercury. By employing mercury as the falling liquid in Hero's fountain a similar increase of effect may be obtained with that apparatus.

W. A. SHENSTONE

Fownes' "Manual of Chemistry"

IN my review of Fownes' "Manual of Chemistry" are two mistakes which I beg to correct. On page 25, line 1, read *improbable* instead of improvable; and line 6, *dimorphides* instead of isomorphides.

THE REVIEWER

OUR ASTRONOMICAL COLUMN

THE TRANSIT OF MERCURY, MAY 6, 1878.—The transit of Mercury, which will occur on May 6 in the ensuing year, is the last during the present century in which the planet can be observed upon the sun's disc for any length of time in this country, and on that occasion the nearest approach of centres will take place only half an hour before sunset; owing, however, to the long duration of the transit, 7h. 35m. geocentric, Mercury will have been upon the disc more than four hours and a quarter when the sun sets. Reducing to Greenwich by the *Nautical Almanac* data it appears the first external contact will occur at 3h. 10m. 58s. mean time, and the first internal contact at 3h. 14m. 4s., or the planet will be 3m. 6s. in wholly entering upon the disc. The least distance of centres will occur at 7h. 0m., and sun-set at 7h. 29m. The duration of the transit is longer than in any other of this century, or indeed than in any one that has occurred since the year 1756.

Up to the present year twenty-four transits of Mercury have been more or less observed; in this number are included that of 1631, November 7, predicted by Kepler, when the planet was seen upon the sun's disc for the first time by Gassendi, at Paris, who observed on the dark-chamber method—by allowing the sun's light to pass into the room through a small aperture in the window, and throwing his image upon a white screen; that of 1651, November 3, imperfectly seen by Shakerley at Surat, and that of 1707, May 6, which was observed through clouds by Roemer at Copenhagen near the egress. Of these *twenty-four* transits it is singular that only *eight* have taken place at the descending node or in May, as will be the case next year. Two-thirds of the number have therefore occurred in November, when we might have expected the hindrances to observation to have operated unfavourably in these latitudes.

Of the three transits of the present century subsequent to 1878, that of 1881, November 7, will be wholly invisible in this country, the ingress taking place at 10h. 16m. and the egress at 15h. 37m.; in the transit of 1891, May 10, the egress occurs soon after sun-rise; and in that of 1894, November 10, it occurs near sun-set. The reader who is curious respecting the transits of Mercury in the next century may consult a communication from the Rev. S. J. Johnson to the Royal Astronomical Society in the *Monthly Notices*, vol. xxxvii. p. 425; and for an account of Gassendi's long watch for the transit of 1631, and his successful observation of it, he may be referred to Prof. Grant's classical work, the "History of Physical Astronomy."

NOVA CYGNI, 1876.—Prof. Julius Schmidt mentions that the star which he first remarked on November 24, 1876 (and which is not found in the *Durchmusterung*) diminished very regularly from January to August of the present year; it exhibited none of the slight oscillations in brightness which are still seen in T Coronæ, and we may add in other "Novæ." With the Athens refractor he has observed three small stars near the variable, with the following differences of right ascension and declination:—

13m	... y = Nova − 1·0 s.	...	Nova − 45″
13	... z = ,, − 1·6	...	,, − 81
12·5	... x = ,, + 4·6	...	,, + 20

It will be remembered that this star suddenly shone out of 3·4 magnitude, and had diminished to the limit of naked-eye vision soon after the middle of December. Its mean place for 1880·0 is in R.A. 21h. 36m. 59·9s., N.P.D. 47° 42′ 16″.

COMET 1873, IV.—M. Raoul Gautier has worked out definitive elements of the comet discovered by M. Borrelly on August 20, 1873, and finds the observations best represented by an ellipse with a period of 3,277½ years, the probable errors of perihelion distance and eccentricity limiting the period between 3,012 and 3,585 years. This comet, however, was observed for one month only, or through an orbital arc of only 58°, and such results of calculation in the present case are not perhaps to be allowed any great weight. There are many other comets which we imagine would better have repaid the labour expended by M. Gautier upon Comet 1873, IV. Expressing his best *parabolic* elements in the manner adopted in catalogues of comet-orbits, we have the following figures:—

Perihelion Passage, 1873, September 10·83679 M.T. at Berlin.

Longitude of perihelion	36° 48′ 40″	M.Eq. 1873·0
,, ascending node ...	230 38 4	
,, inclination	84 0 50	
Log. perihelion distance	9·899956	

Motion—retrograde.

MINOR PLANETS.—A remark in this column some time since upon the probability of several discoveries of so-called new planets proving to be observations of bodies previously detected, appears to be justified by recent experience. Thus the object announced as a new planet by Prof. Watson and M. Borrelly in August last was shown by Herr Knorre, of Berlin, to be identical with No. 141, detected by M. Paul Henry at Paris, on January 13, 1875, and it is now stated that the small planet remarked by Herr Palisa at Pola on October 2 is really No. 161, which was discovered by Prof. Watson on April 18, 1876, and received the name *Athor*. As was to be expected from the rapidity with which discoveries of small planets have succeeded one another of late years, calculation is now considerably behind observation, and we are still without published elements of a number of the bodies lately brought to light.—Prof. Peters states that he has proposed the name *Idunna* for the planet discovered by him on October 14, which is No. 175, a name which he says will be understood by those members of the "Astronomische Gesellschaft" who, at their late meeting at Stockholm, participated in the hospitality of "Ydun."—There is now a strange confusion of mythologies and systems of nomenclature in the minor-planet group, a state of things that at one time might have been readily avoided.

THE ROYAL DUBLIN SOCIETY

A SCHEME for the reorganisation of this society as a branch of the National Museum of Science and Art established by the Government has been under consideration for some time, and a report of the council on the subject was submitted to the society at its meeting on November 8. The scheme includes a recommenda-

tion in favour of the amalgamation of the agricultural department of the society and the Royal Agricultural Society, under the title of the Royal Irish Agricultural Society; after some discussion the report was carried. The following are the principal points involved in the reorganisation:—

In accordance with the agreement entered into with the Government, the principal conditions of which are embodied in the "Act for the Establishment in Dublin of a Science and Art Museum and the Development of the Library of the Royal Dublin Society into a National Library," the property of the society in land, buildings, and collections has passed into the possession of the Government. The society will, in accordance with such agreement, receive the sum of 10,000*l.*, which will be in-invested in such security as, subject to the approval of the Treasury, may be selected; it will continue to be provided with the requisite accommodation in Leinster House; the members will have free access to the several departments as heretofore, whilst the existing members, as well as all those who shall be admitted before January 1 next, will have the right to borrow books from the National Library. In order to assist in the more complete development of that part of the society's work which is devoted to the promotion of science and the useful arts, it has been arranged that all the scientific serials and transactions of learned societies, as well as all duplicates in the library, shall remain the property of the society; the Lecture Hall and Laboratory will be reserved for its use; and the collections in the Botanic Garden and Museum of Natural History will be available as formerly for the illustration of papers read before the society. The most important condition, however, for the successful prosecution of the society's scientific work, pure as well as applied, is that for five years the cost of printing the scientific papers read before the society will be defrayed by the Government. Concessions equally favourable have been obtained for the agricultural department. Thus in lieu of the premises around Leinster House, which will be required for museum buildings, &c., the Government has undertaken to provide accommodation for agricultural shows elsewhere, and to reimburse the society for any pecuniary loss it may sustain in consequence of the change of site from the city to the suburbs. In order to develop the scientific work of the society, and thus secure to the fullest extent the great advantage of having the scientific papers read before it, printed, the Committee of Science have submitted a scheme for the complete reorganisation of the department under their superintendence. Thus, the meetings for the discussion of subjects connected with science pure and applied will be held in these sections:—1. For the physical and experimental sciences. 2. For the natural science, including geology and physical geography. 3. Science applied to the useful arts and industries. The papers to be read at these sectional meetings will be published in 8vo, as the Scientific Proceedings, the more important to be published in 4to, under the title of "Transactions." In order to consolidate and economise both work and time other scientific bodies have been invited to associate themselves with the work of the sections, the meetings of which will be held simultaneously on the third Monday of each month, an invitation to which the Royal Geological Society and the Scientific Club have responded. A special committee is now engaged in considering the measures most advisable to adopt with regard to the future of the society, so as to maintain it as an object of attraction to the educated classes, and a preliminary report has been presented to the council, in which it is advised that in addition to the more complete organisation of the scientific department steps should be taken to render the reading-rooms more efficient, to establish a lending library for the use of future as well as present members, to arrange for the delivery of lectures for the elucidation of the latest discoveries in science, and to hold occasional *conversaziones*. According to one of the conditions contained in Lord Sandon's letter of February 9, 1876, the National Library will be placed under the superintendence of a council of twelve trustees, eight of whom are to be nominated by the Royal Dublin Society and four by the Government.

Then followed the Report of the Committees of the Royal Dublin Society and of the Royal Agricultural Society on the subject of amalgamation, which, as we have said, was adopted. The two societies will to some extent remain connected; the Agricultural Society, Lord Powerscourt stated, would be a branch of the Royal Society, though under different management.

ON THE EOCENE FLORA OF BOURNEMOUTH

DURING this last summer and autumn I have seized several opportunities of continuing my examination of the Bagshot Beds of Hampshire and the Isle of Wight, some of the results of which I think may interest your readers. This series is, as is now well known, of great importance from the fact of its being almost the only series from the tertiaries whose absolute relative geological age is positively known, it being under and overlapped on the mainland by the London clay and Bracklesham beds respectively, whilst in the Isle of Wight, occurring in a complete series of eocene strata, upheaved vertically, its true position is even still more plainly seen. It is further important as exhibiting in gradual sequence the change from an upland to a swamp flora, and represents very fairly the local flora of a long period and of an entire continent that has passed away. Of the richness and completeness of the flora an idea may be formed from the fact that I can reckon in my own collection not less than 10,000 selected specimens, many of large size, exclusive of twice that number which I have discarded, whilst there are also local collections at Bournemouth, a splendid series in the Cambridge Museum, and a scarcely less important one from Alum Bay, at the British Museum. But perhaps the most valuable discovery—to the botanist, at all events—is that of various beds containing well-preserved fruits above the horizon of the leaf-patches, identifiable with fruits from Sheppey which are found in the London clay, and therefore below the leaves. We thus appear to have at Bournemouth the leaves of trees which may be descended from those whose fruits are imbedded at Sheppey. The assistance, it will be readily seen, of the Sheppey fruits will be of the greatest value in determining the genera of the Bournemouth leaves and flowers. At Bournemouth about sixteen kinds of fruit may be collected in the seed-beds just mentioned, including Nipadites, Hightea, Cucumites, and Petrophiloides, quite sufficient to establish the fact that no break took place in the succession of the London clay flora.

The number of forms also common to Bovey Tracey is worthy of note. The most abundant fern at either locality is *Pecopteris lignitum* (now believed to be an Osmunda). *Palmacites dæmonorops* of Heer, from Bovey, is no other than the Cactus of which I have frequently made mention. The dicotyledons of Bovey ascribed to Laurus, Ficus, Daphnogene, Dryandroides, &c., appear also to be identical with those of the Bagshots, and it is therefore not at all improbable that the miocene age of the Bovey Tracey beds, determined, as it seems to me, on most slender grounds, will have to be reconsidered.

The extremely local distribution of the leaves in patches, each with its distinguishing group of plants, has again in fresh instances come prominently under my notice. At Studland, in one bed, fan palms with a three-foot radius lie massed together, but in a decomposed state; and I only succeeded by using the utmost care in extracting one specimen showing the full length of the leaf. At

Bournemouth a small bed of dark clay was found full of leaves of feather palm, crossing each other in every direction; the tip of a frond in my collection measures four feet in length, by three feet broad. Amongst other interesting specimens is a Smilax leaf of larger dimensions than any now living, and a twig of Dryandra, with many leaves attached, from Alum Bay, which unites in itself several of M. Watelet's species from the Grès du Soissonnais. The discovery of a finely preserved neuropterous wing, and of two apparently hemipterous abdomens, are of interest in connection with the large series of insect remains from Gurnet Bay, Isle of Wight, lately deposited in the British Museum.

The history remains to be written of the subsidence of the great continent, whose further limits Edward Forbes surmised are yet traceable in the banks of Gulf Weed, ranging between the 15th and 45th parallels. Many, however, have written on Atlantis, but lacking the direct proof of its former existence in comparatively recent times, which has since come to light. The disappearance of almost an entire continent, is not a more startling proposition than the elevation of the Alps, Pyrenees, Apennines, and Carpathians, over whose highest summits the sea rolled at this period. Of the history of this disappearance Bournemouth presents us with but a page, still a page full of meaning. The incoming and disappearance in succession of oaks and beeches, figs and laurels, palms and delicate ferns, the swamp-loving aroids and Eucalyptus, Chrysodeum and Osmunda, on the same spot; the appearance in masses of the fruit of Nipa, which is stated by travellers to be found in brackish estuaries; the incoming of shore crabs and mud-boring crustacea, sea-shells and Flustra, shingle beeches and deeper sea deposits, are each well-marked stages in the history of the disappearance of this continent, whose existence at this and a later period may be gathered from the writings, made from different standpoints, of Prestwich, Godwin-Austen, Sorby, and many others. The Bournemouth and Sheppey vegetable remains were brought down by one of the rivers draining this continent, which at a later period silted over the reptiles of Hordwell and the estuarine shells of the fluvio-marine series. That the oscillations which gradually led to the disappearance of the land, vestiges of which remain in Cornwall, the Channel Isles, Brittany, Madeira, &c., have not ceased, even in historic times, there is ample local evidence to show. This branch of the subject, however, is scarcely yet ripe for discussion, nor would space here allow it to be fully entered into.

Baron Ettingshausen and myself are preparing a monograph upon the ferns of this flora which I hope very shortly to place in the hands of the Palæontographical Society.

J. S. GARDNER

THE TELEPHONE

At the Society of Telegraph Engineers on the evening of October 31 a lecture of great interest was given by Prof. Graham Bell on the Telephone, with the invention and improvement of which his name is so intimately connected. The lecture was largely illustrated by diagrams, to which Prof. Bell made constant reference, and with these illustrations will be published at length in the forthcoming part of the *Journal* of the Society. We have already given a full account of the telephone and its principles, and will only now refer to some of the interesting episodes which occurred in the course of Prof. Bell's experiments.

Prof. Bell's account of his experiments for devising methods of exhibiting the vibrations of sound, specially for use in teaching the deaf and dumb, is very interesting. For some time he carried on experiments with the manometric capsule of Koenig, and with the phonautograph of Léon Scott. He was led to the idea of constructing a phonautograph modelled closely on the mechanism of the human ear, and at the suggestion of Dr. C. J. Blake, he made use of the human ear itself, a specimen of which was prepared by Dr. Blake, for conducting these experiments.

It occurred to him that if a membrane as thin as tissue paper could control the vibration of bones that were, compared to it, of immense size and weight, why should not a larger and thicker membrane be able to vibrate a piece of iron in front of an electro-magnet, in which case the complication of steel rods in his first form of telephone could be done away with, and a simple piece of iron attached to a membrane be placed at either end of the telegraphic circuit.

The form of apparatus he was then employing for producing undulatory currents of electricity for the purposes of multiple telegraphy he describes thus: a steel reed was clamped firmly by one extremity to the uncovered leg of an electro-magnet, and the free end of the reed projected above the covered leg. When the reed was vibrated in any mechanical way, the battery current was thrown into waves, and electrical undulations traversed the circuit, throwing into vibration the corresponding reed at the other end of circuit. He immediately proceeded to put his new idea to the test of practical experiment, and for this purpose he attached the reed loosely by one extremity to the uncovered pole of the magnet, and fastened the other extremity to the centre of a stretched membrane of goldbeater's skin. He presumed that upon speaking in the neighbourhood of the membrane it would be thrown into vibration and cause the steel reed to move in a similar manner, occasioning undulations in the electrical current that would correspond to the changes in the density of the air during production of the sound; and he further thought that the change of the intensity of the current at the receiving end would cause the magnet there to attract the reed at that end in such a manner that it should copy the motion of the reed at the transmitting end, in which case its movements would occasion a sound from the membrane there similar in *timbre* to that which had occasioned the original vibration.

The results, however, were unsatisfactory and discouraging. With a reduction, however, in the size and weight of the spring employed, distinctly audible effects were obtained. "I remember," Prof. Bell said, "an experiment made with this telephone, which at the time gave me great satisfaction and delight. One of the telephones was placed in my lecture-room in the Boston University, and the other in the basement of the adjoining building. One of my students repaired to the distant telephone to observe the effects of articulate speech, while I uttered the sentence, 'Do you understand what I say?' into the telephone placed in the lecture-hall. To my delight an answer was returned through the instrument itself, articulate sounds proceeded from the steel spring attached to the steel membrane, and I heard the sentence, 'Yes, I understand you perfectly.' It is a mistake, however, to suppose that the articulation was by any means perfect, and expectancy no doubt had a great deal to do with my recognition of the sentence; still, the articulation was there, and I recognised the fact that the indistinctness was entirely due to the imperfection of the instrument." After a time he produced a form of instrument which served very well as a receiving telephone; and it was in this condition his invention was exhibited at the Centennial Exhibition in Philadelphia. It was in this condition also that Sir William Thomson exhibited the instrument to the British Association in Glasgow.

In pursuing his investigations Prof. Bell has come across many interesting facts which we regret we cannot refer to in detail. It has long been known that when an intermittent current of electricity is passed through the coils of an electro-magnet a musical tone proceeds from the magnet. "I have discovered," he said, "that these sounds

are not due wholly to sudden changes in the magnetic condition of the iron core, as heretofore supposed, but that a portion of the effect results from vibrations in the insulated copper wires composing the coils. An electro-magnet was arranged upon circuit unto an instrument for interrupting the current, the rheotome being placed in a distant room so as to avoid interference with the experiment. Upon applying the ear to the magnet a musical note was clearly perceived, and the sound continued after the iron core had been removed from the coils. The effect may probably be explained by the attraction of the coils for one another during the passage of the galvanic current and the sudden cessation of such attraction when the current ceased. It is probable, too, that a molecular vibration is occasioned in the conducting wire by the passage of an intermittent current. I have found that very distinct sounds proceed from straight pieces of iron, steel retort-carbon, and plumbago, when an intermittent current is passed through them."

When a powerful current is passed through the body a musical note can be perceived when the ear is closely applied to the arm of the person experimented upon. The sound seems to proceed from the muscles of the fore-arm and from the biceps muscle. Mr. Elisha Gray has also produced audible effects by the passage of electricity through the human body. An extremely loud musical note is occasioned by the spark of a Ruhmkorff's coil when the primary circuit is made and broken with sufficient rapidity; when two rheotomes of different pitch are caused simultaneously to open and close the primary circuit a double tone proceeds from the spark.

A curious discovery has been made by Prof. Blake. He constructed a telephone in which a rod of soft iron, about six feet in length, was used instead of a permanent magnet. A friend sang a continuous musical tone into the mouth-piece of a telephone, which was connected with the soft iron instrument alluded to above. It was found that the loudness of the sound produced in this telephone varied with the direction in which the iron rod was held, and that the maximum effect was produced when the rod was in the position of the dipping needle.

This curious discovery of Prof. Blake has been verified by Prof. Bell.

"Prof. Peirce has observed the most curious sounds produced from a telephone in connection with a telegraph-wire during the aurora borealis; and I have just heard of a curious phenomenon lately observed by Dr. Channing. In the City of Providence, Rhode Island, there is an over-house wire about one mile in extent with a telephone at either end. On one occasion the sound of music and singing was faintly audible upon one of the telephones. It seemed as if some one were practising vocal music with a pianoforte accompaniment. The natural supposition was that experiments were being made with the telephone at the other end of the circuit, but upon inquiry this proved not to have been the case. Attention having thus been directed to the phenomenon, a watch was kept upon the instruments, and upon several subsequent occasions the same fact was observed at both ends of the line by Dr. Channing and his friends. It was proved that the sounds continued for about two hours, and usually commenced about the same time. A searching examination of the line disclosed nothing abnormal in its condition, and I am unable to give you any explanation of this curious phenomenon. Dr. Channing has, however, addressed a letter upon the subject to the editor of one of the Providence papers, giving the names of such songs as were recognised, with full details of the observations, in the hope that publicity may lead to the discovery of the performer, and thus afford a solution of the mystery."

Prof. Bell referred to some experiments made by Mr. F. A. Gower and himself to show the slight earth connection required to establish a circuit for the telephone.

"One experiment which we made is so very interesting that I must speak of it in detail. Mr. Gower made earth connection at his end of the line by standing upon a grass plot, whilst at the other end of the line I stood upon a wooden board. I requested Mr. Gower to sing a continuous musical note, and to my surprise the sound was very distinctly audible from the telephone in my hand. Upon examining my feet I discovered that a single blade of grass was bent over the edge of the board, and that my foot touched it. The removal of this blade of grass was followed by the cessation of the sound from the telephone, and I found that the moment I touched with the toe of my boot a blade of grass or the petal of a daisy, the sound was again audible."

Prof. Bell concluded as follows:—"The question will naturally arise, through what length of wire can the telephone be used? In reply to this I may say that the maximum amount of resistance through which the undulatory current will pass, and yet retain sufficient force to produce an audible sound at the disturbed end, has yet to be determined; no difficulty has, however, been experienced in laboratory experiments in conversing through a resistance of 60,000 ohms, which has been the maximum at my disposal. On one occasion, not having a rheostat at hand, I may mention having passed the current through the bodies of sixteen persons, who stood hand in hand. The longest length of real telegraph line through which I have attempted to converse has been about 250 miles. On this occasion no difficulty was experienced so long as parallel lines were not in operation. Sunday was chosen as the day on which it was probable other circuits would be at rest. Conversation was carried on between myself, in New York, and Mr. Thomas A. Watson, in Boston, until the opening of business upon the other wires. When this happened the vocal sounds were very much diminished, but still audible. It seemed, indeed, like talking through a storm. Conversation, though possible, could be carried on with difficulty, owing to the distracting nature of the interposing currents.

"I have had the opportunity of testing the telephone upon the artificial cable owned by Sir William Thomson. No difficulty was experienced in conversing through the equivalent of 120 miles of submarine cable. Vocal sounds were audible when the equivalent of the whole Atlantic cable was interposed between the two telephones, but the sounds were so faint that conversation could not be carried on. Songs that were sung into one telephone were readily recognised at the other end of the circuit, and the articulation of pre-arranged sentences was readily recognised. That the sounds were electrically produced was evident from the fact that they ceased when the circuit was broken and when the coils of the telephone were short circuited. No difference was observed between the pitch of the note which was transmitted through the artificial cable and the same note when transmitted directly through the air. The artificial cable experimented upon had four times the resistance of the Atlantic cable, and one-fourth its electrostatic capacity. I am informed by my friend, Mr. Preece, that conversation has been successfully carried on through a submarine cable, sixty miles in length, extending from Dartmouth to the Island of Guernsey, by means of hand telephones."

In a lecture on the 8th inst. at Glasgow, Prof. Bell, referring to the use of the telephone in mines, pointed out how the instrument might be of the greatest service in determining whether the ventilation of a mine was perfect or not; for by listening to the telephone, if the mine was in good order, a little sound could be heard every moment.

AFRICAN EXPLORATION

MR. STANLEY'S letter and the map in the *Telegraph* of Monday enable us to realise somewhat more fully the nature and extent of the discoveries made by the

intrepid traveller. Mr. Stanley is bent on calling the great river, so much of which he has explored, by the name of Livingstone. As a rule we think it a mistake to change native geographical names where these can be satisfactorily ascertained. In the case of the Lualaba-Congo, however, the river seems to have quite as many names as there are tribes or villages on its banks, and it would be a happy solution of the difficulty to confer upon it the most memorable name among African explorers. Mr. Stanley himself has taken great pains to obtain accurately the native names of tribes and places, and he animadverts with severity on geographers for crowding the map of Africa with names that probably correspond to nothing. For this they cannot be greatly blamed, neither need he be too hard on previous travellers for misunderstanding the significance of native words.

A glance at the map, notwithstanding that it is based to some extent on conjecture, shows at once the vast importance of Mr. Stanley's discovery. Great tributaries join the main river from both sides, and we are assured there are many more besides these shown on the map. For more than 800 miles of its course, above the Yellala Falls, the river looks more like a long winding lake than anything else, forming a magnificent channel for navigation. Above the upper cataract, again, about the equator, many other long reaches are capable of navigation, while the affluents will afford over 1,200 miles, and perhaps much more. Some idea of the increasing magnitude of the river below Nyangwe may be obtained from Stanley's statement that at Nyangwe the volume is 124,000 cubic feet per second, while Behm's calculation on the basis of Tuckey's trustworthy observations makes its volume at the mouth to be 1,800,000 cubic feet per second. Poor Tuckey comes in for a share of Stanley's castigation, because, according to Stanley, the former mistook the number of stages of the Yellala Rapids; even if Tuckey was a little out in his counting, which we doubt, he will still be found to have been, all circumstances considered, an accurate observer. Many points, also, in connection with the map, show how true was Livingstone's geographical instinct, and how near the truth his inferences came from the information obtained from the Arabs and natives. Stanley is probably right in conjecturing that the Aruwimi, coming from the north-east, and joining the Livingstone a little north of the equator, is the Welle, and that the Ikelemba is the lower course of the Kasai. The water of the latter is of the colour of tea, and does not thoroughly mingle with the main stream until after 130 miles below the confluence. The banks of the great river are thickly populated by what appear to be industrious people living in extensive and well laid out towns, and naturally jealous of intruders. The three most powerful tribes on the middle and lower rivers are the Wa-Mangala, the Warunga, and the Wyanzi.

The Livingstone, Mr. Stanley found, is subject to periodical rises mainly owing to the rains, and varying from eight to fifty feet. The entire length of the Livingstone Mr. Stanley calculates at 2,900 miles, and its basin at 860,000 square miles. The extreme sources of the Bemba Lake, from which the Luapula flows, are in 33° E. long. Lake Bemba, or Bangweolo, Stanley states—and there appears to be good ground for the belief—is the residuum of an enormous lake that in very ancient times must have occupied an area of 500,000 square miles, "until by some great convulsion the western maritime mountain chain was riven asunder, and the Livingstone began to roar through the fracture." As to the "great convulsion" and the "fracture," geologists may be able to decide when they are in possession of full information as to Mr. Stanley's observations. Nyangwe, Mr. Stanley informs us, is in 4° 16′ S., and 26° 5′ E.; but by an unaccountable mistake in another place he gives the latitude as 26° 15′ 45″, and that, too, while pointing out, in his peculiar way, a slight mistake in the position on Stanford's map of 1874. The position then was perfectly correct according to the data, and in the latest editions the position is exactly as Stanley gives it.

Mr. Stanley insists on the importance of the river as a commercial highway, the country traversed by it being abundantly rich in products that would find a ready market in Europe. Naturally, on Monday night, Africa was the burden of the president's address at the opening of the Geographical Society. Sir Rutherford Alcock insisted that it now remained with the merchant, aided if need be by Government, to open up Africa still further. Indeed the country is now being attacked by national and private expeditions on all sides, and if a basis for minute exploration were formed by trading stations under government sanction and regulation, along the Livingstone, our knowledge of the country would grow rapidly, and the benefits to commerce would be incalculable. Only, however, could the natives have fair play by governmental regulation of private enterprise. There is no danger of extinction for the native African, and it would be both prudent and just to protect him from the horrible cruelties at which Mr. Stanley hints in the conclusion of his letter.

It is worth noticing that in the map the Lukuga runs boldly from Lake Tanganyika and joins the Lualaba, and the source of the Alexandra Nile is brought to near 4° south on the east side of the lake.

According to latest intelligence Mr. Stanley is at the Cape wanting to get his followers sent back to Zanzibar. In his letter in yesterday's *Telegraph* he gives an interesting account of his companion, Frank Pocock, of whom he speaks in the highest terms, and whose death is a real loss to African exploration.

The *Daily News* Alexandria Correspondent writes (on the 5th) that Signori Gessi and Matteucci have just started from Cairo for Khartum, *viâ* Assouan, by the Nile, instead of taking the shorter route by the Red Sea to Massowa. They are provided with the newest and most improved scientific instruments, and having promised to keep up constant communication with the Geographical Society at Rome, interesting accounts of their movement and progress will be looked for.

MODERN TORPEDO WARFARE

TWO elements have contributed to make torpedo warfare what it is: electricity and the new explosive compounds. It is true that in the Whitehead or fish torpedo recourse is had only to the latter of these, but it is the sole material exception, and all the mischief effected by this branch of marine warfare has been, so far, the result of electric torpedoes. Both on the Danube and in the last American war, when no less than twenty-five ships were sunk by the Confederates, the electric torpedo has worked extensive injury, and it is no wonder therefore that a keen interest should be taken in all that pertains to so novel and destructive a method of killing and wounding.

We have called the torpedo a novel weapon, and the instruments that go by the name to-day undoubtedly are so. At the time of the Crimean war, we had to do with torpedoes of a kind; nay, even so far back as the beginning of the seventeenth century, floating charges, called petards, were employed, but these were of too insignificant a nature to merit attention. The "infernal machines" strewn in the Baltic by the Russians twenty years ago were small canisters of powder containing by way of igniting arrangement a mixture of chlorate of potash and sugar, together with a glass bulb with sulphuric acid; and the latter, escaping from its envelope when this was broken by a shock or collision, brought about an immediate explosion. These mechanical torpedoes had two disadvantages; the igniting arrangement was of such a character that it could be set in action just as well by friend as by foe, and the explosion of the gun-

powder was insufficient to effect any material injury. All this has been remedied. Electricity is nowadays employed as the igniting agent, and those terribly violent explosives, gun-cotton and dynamite, replace the comparatively innocuous gunpowder.

Electric torpedoes may be broadly divided into two classes: offensive and defensive torpedoes. The latter are employed for the protection of harbours, channels, and roadsteads; the former, in the shape of drifting or spar-torpedoes, are carried to the attack in small swift-sailing steam-launches. In this country we are favourably disposed to the employment of compressed gun-cotton in our machines, while on the Continent they seem to entertain a predilection for nitroglycerine, or rather dynamite. Both compounds are what chemists term nitro-compounds, in contradistinction to gunpowder, which comes under the class of nitrate-compounds, and appear to exercise an explosive force of almost similar violence, measuring the substances weight for weight. Compressed gun-cotton, we need hardly say, is cotton yarn acted upon by nitric and sulphuric acids and then pulped and washed, so that the result is a finely-divided mass which may be made to assume any shape or form. As a rule the material is pressed into cakes of disc-like form, which weigh from a few ounces to a pound, and while still wet the slabs are stored away in the magazines. In this moist condition

Fig. 1.—Fish Torpedo exploding against a ship.

the compressed pulp is not only non-explosive, but actually non-inflammable, except one possesses the key to its detonation. This is nothing more than a dry cake of the same material, or as the latter is termed in military parlance, a "primer," which on being detonated by a few grains of fulminate, brings about the explosion of any wet gun-cotton in its immediate neighbourhood. Thus if simply a net is filled with gun-cotton slabs and thrown into the sea, the whole charge may be ignited by a primer contained in a waterproof bag having an electric fuze and wire attached. The possibility of communicating explosion in this way by vibration instead of by spark or flame is, too, as we shall presently see, the germ of a system of counter-mining, or torpedo annihilation, which bids fair to develop into a particularly effective means of defence against these terrible machines. Dynamite is similarly exploded to gun-cotton. The active principle in this case is nitro-glycerine, or, if you will, liquid gun-cotton, prepared by simply allowing glycerine to fall drop by drop into nitric acid. As a solid is usually more convenient to handle than a liquid, the use of pure nitro-glycerine has given way to dynamite, which may be described as siliceous earth impregnated with the explosive fluid.

Dynamite and gun-cotton explode with something like four or five times the force of gunpowder, and for this reason a very destructive charge may be confined within a comparatively small space. Moreover they are peculiarly adapted to submarine mines, since nitro-glycerine is no more affected by water than gun-cotton; and the old adage "to keep your powder dry" does not apply to either of them. In the case of moored torpedoes which are connected with batteries to the shore or carry their own means of generating electricity, as in the Herz topedo of our German cousins, there is no limit to size, and machines containing as much as 500 lbs. of gun-cotton have, in fact, been constructed; but for a spar-torpedo, or in other words one which is thrust under an enemy's keel by means of a thirty-foot pole projecting from the prow of a launch, the charge must be considerably smaller, and for two reasons. A great weight at the end of such a lever could not be properly manipulated, while the explosion, if the charge were a very large one, would destroy both the attacking and attacked. A big moored torpedo of 500 lbs. has been found, when sunk in thirty or forty feet of water, to be fatal to a strong ironclad if the latter happens to be within this distance of the source of explosion; or, in other words, a cushion of water forty feet in thickness is not sufficient to secure the immunity of such a vessel. What would happen if this terrible volcano were to erupt—if we may use the word—in contact with the sides of an armoured ship, must be left to the imagination; but despite Mr. Ward Hunt's opinion to the contrary, we do not think it would require

Fig. 2.—A moored Torpedo exploding. Height of column 60 feet, base 220 feet.

three such torpedoes successfully exploded, to bring our boasted *Inflexible* to grief. And in this opinion our readers, we suspect, will fully agree, when we inform them that a heavy torpedo like this throws up a cone of water sixty feet in height, with a diameter at its base of no less than 220 feet. Such an heaving of waters, if it did not break the back of an ironclad, as there is every reason to suppose it would, must inevitably capsize her without more ado. But it is, of course, only on very rare occasions that such a monster torpedo could be brought to bear, and in all cases of attack the charge must needs be considerably less. The smaller Whitehead torpedoes, which, as our readers know very well, are narrow cigar-shaped weapons, that move through the water by the agency of compressed air, do not in all probability carry more than a 40 lb. or 50 lb. charge in the head, while a spar or drift torpedo of 100 lbs. is already as large as would be convenient to handle. At the same time either of these would quite suffice to fracture an iron plate several inches in thickness, and therefore be fatal, probably, to any ironclad afloat, supposing there was no water-cushion between the craft and the torpedo. We have no definite information respecting the size or weight of the torpedoes which sank the Turkish monitor in the Matchin Canal, but as the expedition was hastily arranged and organised, the charges were, no doubt, not very large.

The fish torpedo is a rare example of a complicated apparatus coming into practical use, and its elaborate

construction and fine workmanship may be imagined when the reader is informed that the machines cost 500*l.* a piece to manufacture. The long tube is divided into three compartments: the head, which contains the explosive charge, the reservoir, in which the compressed air is stored, and the machinery by means of which the stored-up energy is converted into a propelling force. The air is compressed to the extent of 600 lbs. on the square inch, and to bring about this result an exceedingly powerful air-pump is necessary, which forms an additional item of expense in the case of this torpedo. The latter when properly charged will do a journey of a mile, or mile and a half, under water, the first 1,000 yards being got over at a rate of no less than twenty miles an hour, and if unaffected by tide or current, the machine will proceed in a perfectly straight direction. It floats at any distance under water that may be desirable, but is usually made sufficiently buoyant to swim at eight feet from the surface; it explodes on striking any object, but the machine is so contrived that if it fails to strike, then it floats to the surface, and a trigger guard renders the fish at the same time innocuous, and permits of its recapture without risk. Ingenious as the little creature is, there has been, we repeat, no authenticated employment of it during the present war.

On the Danube the spar-torpedo alone seems to have been used against Turkish monitors. As in the case of the *Thornycroft* launch, of which we are to have a flotilla of thirty in the British navy, the torpedo is projected at the end of a spar, and is ignited either by concussion or by electricity. The Turkish ironclad at Matchin was the victim of two torpedoes of this class, the first of which, we are told, was ignited by the crew of the launch by electricity, and the other on concussion with the vessel attacked. These Russian torpedoes are said to be innocuous at a distance of ten feet from the seat of explosion, and hence those in the launch do not suffer much except from the water that is thrown into the air. From the fact that small batteries in the boat are used to fire the charges, we may safely conclude that their explosion is brought about by a platinum wire fuze, which, together with a few grains of fulminate, would determine the detonation of dynamite or gun-cotton. Each launch is provided with a pair of these spar torpedoes, carried, when not in action, on each side, running the length of the boat, and only on making an attack is one or other projected at the bow, the torpedoist being stationed behind a shield, or under an iron screen, where he can make his observations tolerably free from danger.

In the case of moored torpedoes depending for their ignition upon electricity, many points of scientific interest have recently been brought to light. Some experiments undertaken in Denmark two or three years ago showed most conclusively that dynamite torpedoes cannot be placed close together without incurring the danger of one charge bringing about the explosion of others. A dynamite torpedo of 150 lbs. ignited in ten feet of water, was found capable of exploding other charges at a distance of 300 feet by the mere vibration imparted to the water; so that in constructing coast defences with dynamite torpedoes it is absolutely necessary to keep them far apart from one another. Another point was also noted. A current of electricity, if it emanates from a powerful frictional electric machine, traversing one of a bundle of wires, will induce a current in the other wires, and thus bring about the explosion of torpedoes other than that which the operator on shore desires to ignite. It is these facts particularly which have led to the development of a system of counter-attack and have enabled our sailors to devise a means of defending themselves from the terrible sea-monsters. Both dynamite and gun-cotton are peculiarly sensitive to vibration—indeed their detonation, as we have seen, is brought about by no other cause—and hence a captain of a man-of-war by exploding counter-mines in his vicinity may soon get rid of any lurking torpedoes lying in wait for him, at any rate if they contain a nitro-glycerine compound, and so speedily clear a way for his ship.

This is certainly subject for congratulation, for it seemed at one time as if the poor sailor was absolutely defenceless against these submarine abominations. A crinoline of spars and wire rope may be employed to catch the fish torpedo and explode the vermin harmlessly in its toils, provided the ingenious brute is not a very large one, and the net is at some distance from the ship; but heavy moored torpedoes have been hitherto considered too dangerous to approach, so that marine countermining must prove invaluable. The spar or drifting torpedo cannot be dealt with by nets or booms alone, and in this case the only plan would seem to be to meet attack with attack and beat off launches with other small boats. That all ironclads in time of war will have to be surrounded by lesser craft as a protection is a matter that we may now take for granted, as also that such vessels must be provided with some powerful means of illumination—the electric light, for instance—to prevent swift, low-lying torpedo launches from approaching unperceived at night time.

Special schools of instruction for acquainting officers with the science of electricity and explosives have for some time past been established, and there is indeed scarcely a naval power which has not paid attention to submarine warfare; consequently we may expect to see future battles upon the sea carried on just as much under water as above it. In this country we have a torpedo school on board H.M.S. *Vernon* at Portsmouth, while at the Royal Naval College at Greenwich instruction in the experimental sciences now forms one of the most important items in the curriculum. France has its naval torpedo school at Boyardville, where both officers and seamen are made acquainted with the principles of submarine warfare. Germany, as all the world knows, practised torpedo warfare to such good purpose seven years ago that the magnificent fleet of the French never once ventured to visit the coast of the Fatherland. Both at Kiel and at Wilhelmshaven are to be found torpedo depôts and a well-organised staff of instructors. Lastly the news comes to us from Russia that the Czar has sanctioned the organisation of a distinct torpedo service, and two depôts and instructional schools are to be formed at Kertch and Cronstadt, whence torpedo appliances are to be issued for the defence of the Baltic and the Black Sea.

NOTES

Mr. Darwin will receive the honorary degree of LL.D. at Cambridge on Saturday next, at 2 p.m., at a congregation specially convened for the purpose. In the evening the annual dinner of the Philosophical Society will take place in the Hall of Clare College, when a brilliant gathering is expected to meet the illustrious visitor, among the non-resident guests being Profs. Huxley, Tyndall, and Parker, and Sir John Lubbock.

The Postmaster-General of the German Empire is about to have an extensive series of experiments made with a view to the introduction of the telephone into the telegraphic service. Several hundred specimens of the telephonic apparatus manufactured by Siemens and Halske have been ordered.

The French Ministry had granted a pension to the widow of Leverrier. Unfortunately the lady died, as we mentioned in our last number, before the first monthly instalment became due. It is hoped that a part of the pension will go, against ordinary rules, to the son and daughter of the astronomer.

The Minister of Public Instruction has been authorised by a

decree of the President of the French Republic to accept a sum of 8,000*l.*, bequeathed by Madame Thuret, in order to establish at Antibes, in the Department of Alpes Maritimes, an agricultural station connected with the lectureship on Agriculture and Botany of the Paris Museum of Natural History.

MR. PARK HARRISON has completed the exploration of the galleries belonging to the "Cave Pit" at Cissbury—in which rune-like characters were found in 1875—and found that they communicate with galleries connected with other shafts, at distances of from 20 to 30 feet, on the north, west, and east sides. Mr. Harrison thinks there appears to be sufficient evidence that they were used for purposes of shelter or concealment long after they were originally excavated. One of the shafts last cleared out, was found to have been left in an incomplete state, as if the work had been for some reason interrupted. On the south of the cave pit, and immediately adjoining it, Mr. Harrison has discovered several small pits, the largest being 5 feet in diameter, and 4 feet 6 inches deep. All contained flint flakes, sling-stones, and a few bones. In some there were small ornaments, pots of good quality, bone combs, terracotta beads, and hard polishing-stones. In one pit there was an iron hook.

THE following testimony from so competent and disinterested an observer as Prof. Monier Williams to the necessity for systematic meteorological observation in India is valuable, and we hope will have weight with the proper authorities. In the last of his series of articles on his second tour in India, in the *Times* of November 7, Prof. Williams writes thus :—"One thing requires instant attention. The connection between agriculture, meteorology, and astronomy is now admitted on all hands, and no country in the world would be benefited more than India by systematic meteorological and astronomical observations carried on under Government direction. Much is already being done in this way. Yet I could only find one effective astronomical observatory, and that not adequately supported by Government, though I travelled from Cashmere to Cape Comorin. It is not generally known that from his observations of the present condition of the disc of the sun, in connection with various atmospherical phenomena, the Madras astronomer, Mr. Pogson, prophesied in 1876 a recurrence of the drought and famine in 1877."

ON October 24, we learn from *L'Exploration*, Signor D'Albertis and Prof. Od. Beccari left Genoa in the steamer *Australia* for a year's voyage round the world. They will first visit Egypt, and thence to India, China, and Japan, returning to Europe by New York. They intend to collect during their voyage birds, mammals, and insects for the museums of Italy, principally for that of Genoa.

FOR several years past Major J. W. Powell, in charge of the United States Geographical and Geological Survey of the Rocky Mountain Region, has been paying particular attention in his researches, to the ethnology and philology of the American Indians; and having received from the Smithsonian Institution an immense mass of material on this subject, collected during a period of many years, he has called to his assistance numerous experts for the purpose of preparing a series of memoirs on these topics. We have now a partial result of his labour in the first of a series of quarto volumes, entitled "Contributions to North American Ethnology," and published in most excellent style, with numerous illustrations, at the Government Printing-office. The present volume is occupied with the Indians of North-western America, embracing several papers by Mr. Dall and others on the tribes of Alaska and adjacent territories, and a number of vocabularies, principally by the late Mr. George Gibbs.

OUR readers may remember that last spring Capt. Burton made an expedition into the Land of Midian, which lies to the south-east of the Gulf of Akaba, in the Red Sea. He was accompanied by a mining engineer, M. Marie, and the two explorers came upon traces of extensive mining operations, the ruins of ancient towns, and many other evidences of a flourishing mining district. They brought back specimens containing gold, silver, copper, and other metals, and were most sanguine as to their discovery. Capt. Burton is now again in Egypt, the *Times'* Alexandria correspondent writes, preparing another expedition to Midian. He is now determined to investigate thoroughly that biblical country of which he only got a superficial idea in his twenty-day visit last spring. His intention now is to penetrate to the mountains in the interior, and thoroughly satisfy himself as to their nature and capabilities. He estimates the distance under twenty days' march. It is a curious fact that these mines were known to the ancients so long ago as the time of Ramses III., whose cartouche is inscribed on the Needle which is on its way to England. In the Harris Papyrus in the British Museum is a passage referring to the copper mines of Akaba.

AT the last meeting of the Russian Geographical Society, the Secretary gave some account as to this year's expeditions sent out by the Society. The results of Prshevalsky's expedition are a survey from Kuldja for 800 miles into the interior of the country, seven determinations of latitudes and longitudes, many barometrical measurements of heights along the route, a botanical collection of about 300 species, a zoological collection, numbering 85 mammalia, 180 species (500 specimens) of birds, 50 specimens of fishes, 150 reptiles, and 2,000 insects. The most important objects in the collection are four skins of wild camels. All the collections are now in Kuldja, and will be forwarded to St. Petersburg during the winter. About the end of August M. Prshevalsky had started for Tibet. M. Potanin has returned without having penetrated far into the interior of Mongolia. He proposes now to go to the sources of the Yenissei. M. Mainoff has returned from his travels among the Mordva population of Eastern Russia with very valuable materials. He has obtained anthropological measurements according to the 126 queries of the programme, of 510 individuals, and he brings detailed answers on the queries of the programme as to the ethnographical and juridical customs of the Mordva, as well as numerous skulls, photographs, tools, and dresses.

A RAILWAY official in Berlin was lately fined by the district courts for appending to his name the title of *doctor juris utriusque*, on the strength of a diploma from the University of Philadelphia. An appeal to a higher court resulted in a confirmation of the sentence.

WE notice a very useful Russian work, just published by the St. Petersburg Committee of Primary Education, being a review of all works that have appeared in Russia in the department of primary instruction. The book, 640 pages, gives a complete catalogue of such works, with critical notices on each of any importance, and it is sold at a very low price, for the use of primary teachers.

A YOUNG schoolmistress of Tlemcen (Algeria) has successfully passed her examinations before the Faculty of Aix for Baccalaureate in Letters, and has been warmly congratulated by the Board.

THE statue of Lagrange, the celebrated mathematician, born in Italy, but a naturalised Frenchman, was erected last week in the hall of the Bureau des Longitudes.

AT a recent meeting of the Paris Academy of Sciences a letter from M. Fabre to M. Dumas was read, referring to an American vine which he had cultivated for a long time in the very heart of phylloxeric centres, but which has

escaped the least sign of infection. It flourishes under the most unfavourable circumstances, grows rapidly, and readily receives grafts from French vines.

THE French Association Polytechnique, created in 1830, has just published its programme for 1877-1878. Lectures are given by this institution to working men in each of the twenty municipal districts of Paris, and in almost every manufacturing district of France. For the first time the programme of lectures is uniform, and special text-books are published at a cheap rate under its authority. No salaries are given to teachers, and no fees taken from pupils. It is called "La Sorbonne de l'Ouvrier." All expenses are covered by voluntary contributions. M. Dumas, the perpetual secretary of the Institute has been elected president of the Association. He has filled this important position for a number of years.

THE *Jardin d'Acclimatation* of Paris, as we recently stated, has received a family of Esquimaux, who are quartered alongside the Nubians, who were recently in London. They consist of three men, a woman, and two children, and have charge of a collection of phocas, white bears, and trained dogs. The customary Esquimaux huts have been erected for their accommodation, and their time is spent in the ordinary occupations to which they are accustomed in the Polar regions. The *Société d'Anthropologie de Paris* has appointed a commission to study these unusual visitors, consisting of Dr. Broca, president, and MM. Bordier, Dolly, Girard de Rialle, Mazard, and Topinard.

THE Ministry of Public Instruction has just established, in Paris, a "Bibliothèque Universitaire," containing all works appearing from the pens of the professors of the French University.

AMONG the medals awarded by the Photographic Society in connection with their Exhibition, are one for the best microphotograph, "Proboscis of a Blowfly," to Mr. Edward Viles, and a special medal to Mr. W. J. A. Grant for his Arctic Views.

THE Institution of Civil Engineers resumed its meetings on Tuesday. Among the papers announced to be read early in the session are, a "Review of the Progress of Steam Shipping during the last Quarter of a Century," by Mr. Alfred Holt, M. Inst. C.E. of Liverpool, whilst the latest development of electrical invention and its application to lighting purposes, will be discussed in a paper by Dr. Paget Higgs and Mr. Brittle, Assocs. Inst. C.E., entitled "Some Recent Improvements in Dynamo-Electric Apparatus."

THE fourth annual meeting of the Dundee Naturalists' Society was held recently. Mr. Grothe, the president, occupied the chair. The secretary read the council's report for the past year, which showed that it had been one of great activity and prosperity. The year began with a membership, including all classes, of 232, but at the date of the report this number had increased to 401, being an increase of 169. The property of the society had also been considerably increased during the year, chiefly by gifts of books and specimens for the society's museum. During the last winter nine original papers had been read by members at the ordinary meetings of the society, treating of geological, biological, physical, and archæological subjects. During the summer the interest in, and work of, the society was kept alive by a series of very attractive excursions. One excursion was a sea-dredging expedition, and opened up for the society a new field for its energies. In order to secure a more exhaustive and systematic treatment of the various branches of natural science, the society was formed into sections, three in number, viz.:—1. Physical and Chemical; 2. Geological; 3. Biological. From this arrangement it is hoped that much good will result. The society is in a very healthy and vigorous condition.

THE following modification of an experiment of Prof. Tyndall's is described by M. Terquem in the *Journal de Physique* for October. A trumpet-bell connected by a thick caoutchouc tube with one of König's manometric flames, is fixed vertically over a square plate, which is vibrated so as to give two nodal lines as in Tyndall's experiment. If the axis of the bell be placed exactly over the centre of the plate, the flame remains quite motionless, and the same if the bell be placed above a nodal line. On the other hand, the flame vibrates when the bell is displaced, however little, and the vibrations become very strong when the bell is placed above a ventral segment. With two similar trumpet-bells placed over two ventral segments having similar, or contrary movements, the vibrations may be united on a single flame, by means of a Y-tube, a drawing-tube being placed in the passage of one of the vibratory movements. The advantage of this arrangement consists in producing very strong separate vibrations; moreover, it is possible to give them exactly the same intensity by displacing laterally one of the bells. To obtain absolute motionlessness in the flame the two combined movements must have exactly the same amplitude. To render the flame more brilliant M. Terquem passes the gas through pumice-stone soaked with benzine or the like, and incloses the jet in a tube through which a current of oxygen is sent. A cylinder of mica blackened interiorly, except on the side next the revolving mirror, surrounds the flame.

A RECENTLY-PUBLISHED report by the Criminal Administration of France gives some curious statistics with regard to suicides in 1874. There were in that year 5,617 suicides, the highest number ever recorded in the country. Of these 4,435 (79 per cent.) were committed by men, and 1,182 (21 per cent.) by women. The ages of 105 of the suicides are unknown. The 5,512 others are thus divided:—Minors of 16 years, 29; 16 to 21 years of age, 193; 21 to 40, 1,477; 40 to 60, 2,214; and beyond 60, 1,599. Among the suicides there are enumerated 1,946 celibates (36 per cent.), 2,645 (48 per cent.) were married, and 881 (16 per cent.) were widowed. Of the number of those forming the last two categories there were 2,259, or nearly two-thirds, who had children. The civil state of 145 individuals could not be ascertained. More than seven-tenths of the suicides were by strangulation (2,472), or by submersion (1,514). The suicides were, as always, more frequent in spring (31 per cent.) and in summer (27 per cent.) than in winter (23 per cent.) and in autumn (19 per cent.). As to the motives, there is no information about 481 of the suicides; the others are classed as follows:—Misery and reverses of fortune, 652; family troubles, 701; love, jealousy, debauchery, misconduct, 815 (of which 572 were brought about by drunken habits); physical sufferings, 798; various troubles, 489; mental maladies, 1,622; suicides of persons guilty of capital crimes, 59.

AT the meeting of the Eastbourne Natural History Society, of October 19, Mr. Roper read an important paper on "The Addition to the Flora of Eastbourne since 1875."

THE additions to the Zoological Society's Gardens during the past week include a Tiger (*Felis tigris*) from China, presented by Mr. A. Forbes Angus; a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Mr. H. W. Henderson; a Saker Falcon (*Falco sacer*) from Egypt, presented by Mrs. Arthur Coote; two Grey Plovers (*Squatarola helvetica*), a Ringed Plover (*Œgialitis hiaticula*), a Dunlin (*Tringa cinclus*), European, presented by Mr. F. Cresswell; a Californian Quail (*Callipepla californica*) from California, presented by Mrs. A. H. Jamrach; a Ring Hals Snake (*Sepedon hœmachates*) from South Africa, presented by Mr. Eustace Pillans; a Brown Pelican (*Pelecanus fuscus*) from West Indies, a Cape Zorilla (*Ictonyx zorilla*) from South Africa, purchased; five Reindeer (*Ranifer tarandus*) from Lapland, deposited; a Cape Buffalo (*Bubalus caffer*) from South Africa, received in exchange.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

EDINBURGH.—The Marquis of Hartington has, by a large majority over Mr. Cross, been elected Lord Rector of Edinburgh University.

PRUSSIA.—We notice from the last report of the Prussian Minister of Instruction that the present number of instructors in the ten universities amounts to 896, viz., 466 ordinary professors, 7 honorary, 199 extraordinary, and 224 *privat-docenten*. The philosophical faculties include 400, the medical, 250, the legal, 86, and the theological, 110. The number of instructors varies from 29 at Münster, to 201 at Berlin. The number of students is about nine times that of the professors, viz., 8,209, and includes 1,080 from other countries than Prussia. According to their faculties they are divided as follows: evangelical-theological, 684, catholic-theological, 289, legal, 2,261, medical, 1,349, and philosophical, 3,626. The attendance at the universities during the past summer was Berlin 2,237, Breslau, 1,245, Göttingen, 917, Bonn, 897, Halle, 827, Königsberg, 620, Greifswald, 503, Marburg, 401, Münster, 315, and Kiel, 241.

In the budget submitted to the present Prussian House of Deputies are the following items:—Erection of the German Industrial Museum, 998,000 mk.; erection of a Polytechnic in Berlin, 8,393,370 mk.; erection of an Ethnological Museum in Berlin, 1,800,000 mk.; and for the Berlin University, erection of a Herbarium, 422,000 mk.; of a Clinic, 1,955,000 mk.; of a new building for a second Chemical Laboratory, as well as of a Technical and Pharmaceutical Institute, 967,000 mk.

BONN.—On entering upon the duties of rector of the University, Prof. Kekulé, the distinguished chemist, delivered, on October 18, a brilliant address on the scientific position of chemistry, and the fundamental principles of this science. He made the following definition of chemistry as distinct from physics and mechanics:—"Chemistry is the science of the statics and dynamics of atoms: physics that of the statics and dynamics of molecules; while mechanics considers the masses of water consisting of a large number of molecules." After rapidly sketching the growth of the present atomic theory, he claimed that the mass of results now obtained showed that chemistry was slowly but surely approaching its goal, the knowledge of the constitution of matter. In opposition to the opinion that theory should be banished from the exact sciences, he regarded it as an actual felt necessity of the human mind to classify the endless series of individual facts from general standpoints—at present of a hypothetical nature—and that it was precisely the discussion of these hypotheses which often led to the most valuable discoveries.

VIENNA.—In Vienna the question is being agitated of separating the natural sciences at the University into a separate faculty, apart from the philosophical faculty, as is the case in Strassburg and a few other universities, which have risen superior to the old mediæval classification.

STRASSBURG.—The imperial authorities have finally decided upon extensive appropriations for the new buildings of the University. They will embrace edifices for lecture-rooms, chemical and physical laboratories, and chirurgical and psychiatric clinics. The new observatory will be completed next year, and the botanical gardens are rapidly being laid out. In 1882 the University expects to occupy its new buildings.

KÖNIGSBERG.—Prof. W. Lossen, of Heidelberg, well known by his researches on hydroxylamine, has accepted a call to the Chair of Chemistry at the University of Königsberg.

UPSALA.—The University is attended at present by 1,395 students, of whom the half are included in the philosophical faculty. The corps of teachers embraces sixty-three ordinary and extraordinary professors, and fifty-four *privat-docenten*. Of these eighty-two are in the philosophical faculty.

ST. PETERSBURG.—The lectures at the St. Petersburg Ladies' High Medical School re-opened this year on October 13. One hundred and eighteen students were admitted, though a far larger number of applicants passed the examination. The number of the students admitted, however, was limited as above because of want of room. A fifth class has now been added, and the students receive, after having finished the studies, the degree of surgeons.

SOCIETIES AND ACADEMIES

LONDON

Linnean Society, November 1.—Prof. Allman, F.R.S., president, in the chair.—Messrs. S. M. Samuel and P. Wyatt Squire were duly elected fellows of the Society.—A communication was read by Dr. G. King on the source of the winged cardamom of Nepal. By Dr. Pereira it had been regarded as the produce of *Amomum maximum*, Roxb.; but this is indigenous to Java. Roxburgh named two Indian species, *A. aromaticum* and *A. subulatum*, and Dr. King shows that the latter is the so-called winged cardamom of Nepal, its true habitat being the Morung mountains and not the Khasia hills as asserted by Voigt.—There followed a paper by Capt. W. Armit on Australian finches of the genus *Poëphila*. Mr. Gould had recognised two birds, *P. gouldiæ* and *P. mirabilis*, as good and distinct specific forms, a statement questioned by Mr. Diggles at the Queensl. Phil. Soc., 1876. Capt. Armit having studied the live birds in their native haunts gives his evidence in favour of Mr. Gould as to the just separation of the said Australian finches. — The self-fertilisation of plants formed the subject of an interesting paper by the Rev. G. Henslow, a notice of which we shall give elsewhere.—Mr. Ed. J. Miers gave a revision of the Hippidea." This group of the Anomourous Crustacea, although, by their elongated carapace and antennæ bearing considerable resemblance to certain of the Corystoidea, to wit the Chilian, *Blepharipoda spinnimana* and *Pseudocorystes sicarius*, yet the author considers their true affinities to be with the Oxystomatous Brachyura, through the Raninidæ. The Hippidea inhabit all the warmer temperate and tropical seas of the globe. Their life history and habits lately have received considerable elucidation at the hands of Mr. S. J. Smith, of Connecticut, in a study of the development of the common species of the eastern shores of the United States. Their limits are restricted northwards by the cold winters. The *H. talpoidea* lives gregariously, burrowing in the loose, changing sands near low-water mark. Other species, however, inhabit deep water, such as the *Albunea guerinii* in the Gulf of Algiers, &c.—Mr. E. M. Holmes laid before the meeting the late Dr. Hanbury's collection of cardamoms (from the Pharmaceutical Society) in illustration of Dr. King's paper above mentioned; he also drew attention to an undetermined fungus in a sugar cane, which mould had caused the destruction of a plantation in South India.—The Rev. T. H. Sotheby exhibited branches of two remarkable shrubs, *Colletia cruciata*, Hook., and *C. Bictonensis*, Lindl., grown in Lady Rolles' garden at Bicton. These South American plants it seems, are not unknown in this country (one Fellow present stating he possessed them now in flower), but the history of their introduction, nevertheless, is a curious one.—Dr. Masters showed an unusual specimen of a grape within a grape, viz., adventitious fruit developed in place of the normal seeds; he also explained the *rationale* of adventitious tubers producing buds on the root of some examples of *Brassica Rapa* exhibited by him.—Some twigs and flowers of British grown gum trees were shown by Mr. A. O. Walker, among others *Penstemon Clevelandii* said to have flowered here for the first time.

Physical Society, November 3.—Prof. G. C. Foster, president, in the chair.—The following candidate was elected a member of the Society: Alexander Jesseman.—Prof. McLeod described some experiments he has recently made to determine the exact number of vibrations of tuning forks by means of the apparatus he exhibited to the Society on April 28 last, and which was designed for determining slight variations in the speed of machinery or other analogous purposes. He has studied two sets of forks belonging to the Physical Laboratory at South Kensington, and a new set just received from König, and his results exhibit a remarkable concordance, the extreme results in the worst set of observations on a fork of 256 complete vibrations only differing by 0·005 per cent., and in a good set they agreed within 0·00078 per cent. Examining the new series from 256 to 512, he found them to give from 0·3 to 0·5 of a vibration more than was anticipated, but as this variation may be due to a difference between the temperature and that at which they were adjusted, he is waiting to ascertain what this was. He considers also that the manner in which the fork is held has an effect on its vibrations, and he hopes to be able to get some information as to the effect of temperature on elasticity.—Dr. Huggins exhibited some artificial gems recently prepared by M. Feil, the well-known glass manufacturer of Paris, who has succeeded in crystallising stones of the corundum class.

Rubies, as well as a topaz and emerald, were exhibited. Dr. Huggins believes that the colour is imparted by small quantities of metallic oxides, and that the mass is mixed with boracic acid and maintained in a fused condition for a considerable period. M. Feil hopes to obtain larger stones by maintaining the heat constant for several weeks consecutively.—Dr. Lodge then read a communication from Professors Ayrton and Perry, of the Imperial College, Japan, in continuation of one read to the Society on May 26 last, on ice as an electrolyte, and since published in the *Philosophical Magazine*. The experiments therein described led them to expect a very sudden rise in the specific inductive capacity as the temperature of the ice increased through zero and it became water. Recent results have shown that, though rapid, this increase is not as great as they anticipated, and, whereas at $-12°$ C. the capacity is 0·002 microfarads, at $+5°$ C. it is 0·1185 microfarads, and after this temperature the increase was so rapid as to render exact readings difficult. Referring to Prof. Clerk Maxwell's theory in which he compares electromagnetic disturbances with light vibrations, they point out that he exclusively regards a conducting medium. But they showed in a former paper that no dielectric can be considered non-conducting, hence they conclude that the measured specific inductive capacity can never be even approximately equal to the square of the index of refraction. Prof. Foster mentioned that he recently had occasion to collect as many results as possible on specific inductive capacity and refractive index, and he found that, where these figures were low, the agreement with the law was fairly close, but with greater values the inductive capacity and the square of the refractive index separate very rapidly.—Prof. Guthrie described a simple means for showing the interference between two plane waves by means of two long cords vibrating side by side. If a vibration of considerable amplitude be imparted to them, and the plane in which they travel be carefully examined, two faint black lines will be seen, which cross and recross each other more rapidly as the cords are less and less in unison, and with perfect unison remain stationary.

Royal Microscopical Society, November 7.—Mr. H. C. Sorby, president, in the chair.—A paper was read by Mr. Thos. Palmer on the study of evergreens by means of the microspectroscope, in which he described the results of his examination of solutions of the colouring matters, oils, &c., from the leaves in various stages of growth. The paper was illustrated by drawings and by the exhibition under the micro-spectroscope of some of the solutions referred to.—A paper by Mr. F. A. Bedwell on the building apparatus of *Melicerta ringens*, was read by the secretary. It minutely described the structure and functions of those organs, and was an important addition to the number of contributions to the history of this beautiful rotifer. The paper was illustrated by drawings, some of which were enlarged upon the black board by Mr. Charles Stewart.—A paper was taken as read on the lachrymal gland of the turtle, by Mr. Charles Stewart.

Paris

Academy of Sciences, November 5.—M. Peligot in the chair.—The following papers were read:—On some applications of elliptic functions (continued), by M. Hermite.—*Résumé* of a history of matter (third article), by M. Chevreul. This comprises from the thirteenth to the seventeenth century.—On the hydrogenation of benzine and aromatic compounds, by M. Berthelot. The experiments show that the action (sufficiently intense and prolonged) of hydriodic acid brings all these carburets to the composition of carburets absolutely saturated, such as hydride of hexylene, $C_{12}H_{14}$, volatile about 69°.—Reply to a recent note of M. de Parville, "On the semi-diurnal variation of the barometer," by M. Faye.—The echidna of New Guinea, by M. Gervais. This animal is very different from the echidna of Australia. *Inter alia*, it is larger and has darker hair; the claws (which are strong and adapted for digging) number three on the fore as on the hind feet; and the (black) muzzle is much longer than in *E. aculeata*, and sensibly arched; the tongue is also much longer and very slender, and the horny papillæ are differently arranged; the number of vertebræ and ribs is different, &c. M. Gervais regards the animal as belonging to a separate genus, termed *Acanthoglossus*.—On a project of an interoceanic canal; studies of the international commission of the Isthmus of Darien, by M. de Lesseps. This relates to a report of recent scientific exploration by Lieut. Wyse (of the French Navy). The project comprises a tunnel of about 17 kilometres, the remainder of the length being about 55 kilometres. The total cost is estimated at 600,000,000 francs.—Stellar systems formed of stars associated in a common and rapid proper motion, by M. Flammarion.—On the order (or class) of a plane algebraic curve, of which each point (or each tangent) depends on a corresponding point of another plane curve and on the tangent at that point, by M. Fouret.—Applications of a mode of plane representation of classes of ruled surfaces, by M. Mannheim.—On the liquefaction of acetylene, by M. Cailletet. The gas was compressed by means of a hydraulic pump through mercury, in an apparatus of special form. Acetylene is liquefied, *e.g.*, at $+1°$ under 48 atm., at 18° under 83 atm., at 37° under 103 atm. The liquid is colourless and extremely mobile; it seems very refringent, and is lighter than water, in which it can be largely dissolved. It dissolves paraffin and fatty matters. Hydride of ethylene was liquefied in the apparatus at a slightly higher pressure than that of acetylene. The tensions of these two carburets and ethylene are but little different about zero. Reaction of chlorhydric acid on two isomeric butylenes and on olefines in general, by M. Le Bel. The ethylenic carburets combine with cold chlorhydric acid; on the contrary, the hydrocarbons $CH_2 = CHR$ and probably those with the formula $CHR = CHR'$ are not attacked.—On the alteration of eggs produced by mould from without, by MM. Béchamp and Eustache. Hen's eggs may remain long in a medium filled with infusoria without these organisms penetrating. The shell and its lining membrane can be traversed by mucedineæ, which develop abundantly on the inner face of the latter. The yolk-membrane, however, is impenetrable by mucedineæ or any other microzoa or microphytes. The mediate relations of mucedineæ with the yolk produce a true fermentation apart from any organic ferment except microzymas. The acidification of the white is due exclusively to the mycelium of the mould. The production of bacteria in the yolk is due to development of the normal microzymas of the yolk.—On a new function of the genital glands of sea-urchins, by M. Giard. During part of the year these glands play the part both of excretory organs and of deutoplasmigenic organs. This fact presents a new point of relation between echinoderms and annelids, and even arthropods.—Causes which determine the liberation of agile bodies (zoospores, antherozoids) in the lower plants, by M. Cornu. The exit is not the result simply of a physical phenomenon of endosmose, but is at least partly due to the activity of the corpuscles themselves. This activity requires a sufficient temperature, or a certain quantity of oxygen (furnished directly or by oxidation of the green parts), for its exercise.—Meteorological observations made in a balloon, by M. Terrier. This ascent was made on October 18, at 3.30 P.M., from Paris. It is affirmed that the temperature of the atmospheric layers at sunset decreases uniformly with increase of height (the decrease was 1° per 100 metres). The lower winds are less stable than the upper, and it is necessary to interpret the latter for weather prognostication. The aërial currents of small height and velocity are influenced and notably deflected by the inequalities of the ground.

CONTENTS

THURSDAY, NOVEMBER 22, 1877

DANISH GREENLAND

Danish Greenland; its People and its Products. By Dr. Henry Rink. Edited by Dr. Robert Brown, F.L.S. With Illustrations by the Eskimo and a Map. (London: Henry S. King and Co., 1877.)

THERE is a strange fascination about Greenland, which may be partly owing to the mystery that shrouds its early history,—partly to its being an almost Arctic country, the scanty population of which seems to furnish an example of a nation in the enjoyment of a very primitive culture; and partly because it seems very probable that it was from it started the voyagers who were the first discoverers of what is now called America.

Our knowledge of the early history of Greenland is limited to what we can gather from the Icelandic sagas or popular tales, and from these we find that about the year 986 an Icelander called Erik the Red, who had been outlawed, sailed to the west to look for some land which had some years previously been sighted by Gunbjörn, the son of Ulf Kraku, another Icelander who had once been driven far westward by a very fierce storm. Erik found the land, made a two winters' stay thereon, giving names to many places, and returning to Iceland called this new country Greenland, because, said he, people would sooner be induced to go thither in case it had a good name.

This first colonisation of Greenland seems at the time to have been fairly successful, and several ruins are still to be found which throw a light on the habits of these seafaring people. The present Eskimo station, Igaliko, situated on an isthmus between two fjords, is thought to have been the ancient residence of Erik. One of Erik's friends, named Herjulf, had a son called Bjarni, a promising youth, and very fond of travelling abroad. One year he would spend in Iceland, another with his father in Greenland. Wishing, however, to spend one Yule-tide with his father, he set sail for Greenland, where his father was, with a crew who had never been in the Greenland Ocean before, and the consequence seems to have been that he found himself after many days near a country covered with wood, which was certainly not Greenland, and turning his back upon it to hasten to find his parent, he succeeded in landing at the very spot where his father lived. It is probable that during this voyage he had discovered the tract of country stretching from Connecticut to Newfoundland.

The news of Bjarni's venturesome voyage spread to Iceland and to Norway, and Leif, the son of Erik the Red, bought his ship, and set sail for the new country, on which they landed, and which, from finding on it a species of "fox-grapes," they called Vinland. Returning the next year to Greenland, it was no wonder that Vinland was all the talk, and Thorvald, about 1002, went to settle there and finally had a battle with the natives, in which he was killed. This Vinland was probably the present Massachusetts. Half a century later tidings from the Greenland colonies suddenly became rare, but in 1126 the then pope sent them a bishop, the ruins of whose church are still pointed out, and about 1261 the Greenlanders became subjects of Norway. From this date to 1450 tidings of the colonists, stories of their doings, and records of their misfortunes, came less and less frequently to Europe. The very sailing route passed into oblivion, and the country was only again re-discovered in 1585 by John Davis, whose name will be for ever remembered in connection with the Straits also discovered by him. Another century-and-a-half passed away before the present European stations in Greenland were founded by the well-known Danish missionary, Hans Egede, who in 1721 landed on an island at the mouth of the Godthaab-fjord and founded a regular colony. From then until now, with many a vicissitude; an epidemic of small-pox in 1734, a total interruption with Denmark (1807–1814) on account of the war; the colonies have struggled on. The trade was for some part of the former century made a private monopoly, but in order to keep up the commerce, the government was finally obliged to take it in hand, and since 1774 it has continued to be a royal monopoly. Following the steps of the extending trade, the missionary institutions have gradually incorporated the whole population into Christian communities.

Dr. Rink's book tells us in a very succinct though most interesting manner, of the results of the European transactions thus carried on in Greenland, for now over a century, and he describes the present state, and hints at the future prospects of the population. More than this, he gives us in well-written chapters, an account of the configuration and general physical features of this almost frozen up island, he tells of its "inland ice," and of the origin of the "floating icebergs." We read of the temperature, prevailing winds, the wonderful changeableness of its weather, and we find here a *résumé* of all that is known about its lakes and streams, its mysterious fjords, and of its great fields of drifting ice. Nor is the natural history of the country overlooked, for we have a chapter on its geological and mineral products. Of these latter cyolite appears to be the only one that has become a regular article of trade, about 10,000 tons thereof being exported each year. There are also chapters on its plants and animals, with special ones on the capture of whales and seals, and on the Greenland fisheries.

From an Eskimo point of view the commercial importance of the seal and whale fisheries is very great. The flesh and blubber of these animals not only supply the Greenlander with nutritious food, but also provide him with heat and light. The sealskins too afford material for clothes, boats, and tents, and whaleskin called "matak," yields a favourite article of diet. It may give some idea of the vast numbers of these animals killed yearly to summarise the average annual catch as follows: Of *Phoca fœtida*, 51,000; of *P. vitulina*, 2,000; of *P. groenlandica*, 33,000; of *P. barbata*, 1,000; of *Cystophora cristata*, 3,000; and of narwhals, white whales, and walruses nearly 1,000. The right whale has nearly disappeared and the mean annual catch of the "humpback" whale is scarcely over two.

The most important fisheries in addition appear to be those of the cod fish, the halibut, and the capelin.

Perhaps there was not much to be said about the manners and customs of the people in the olden time; the change in religion seems to have very early modified the social condition of the people, and this portion of Dr.

Rink's book is the one that satisfied our curiosity the least. The sketches of Greenland life by natives, as translated from the "Greenland Journal," are interesting, but they tell us of very little except marvellous escapes from snowstorms and icebergs. The great endurance of suffering, as detailed in some of these stories, demonstrates that heroes can be found even in Greenland; the sublime spirit of martyrdom seems to breathe in the account of the "Kayakers cast ashore in a snowstorm."

Scattered through this volume are some sixteen plates, representing Greenland ways of life. These are exact copies of partially coloured drawings executed by natives entirely after their own ideas. The greater number are the work of a seal-hunter living in Kangek, who, falling sick, could not leave his bed. With the drawing which forms plate 16, he wrote to say that increasing illness prevented him from doing more, and he ended the letter with "from exhaustion I must cut my letter short, this too will be my future fate," and shortly after he died.

E. P. W.

OUR BOOK SHELF

A Sketch of the Geology of Leicestershire and Rutland. By W. J. Harrison. (Sheffield: W. White.)

THIS is a creditable compendium of what is known regarding the geology of the two countries of which it treats. It was originally prepared by its author for White's "History and Gazetteer of the Counties," and has been reprinted in a separate form. It can be had embellished with twelve photographs of various parts of the crystalline nucleus of Leicestershire. These are not particularly successful. Mr. Harrison has done well to put the best of them as a frontispiece. It represents the "coarse ashy slates" of Charnwood Forest. As a local guide this book may no doubt be useful; fuller information can be found in the works which Mr. Harrison cites, and especially in the maps and memoirs of the Geological Survey.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Expected High Tides

IF I may judge from the note published in your issue of November 8 (p. 38), and Mr. Jenkins' letter in the last number of NATURE (p. 45), it would appear that the general public are unaware of publications which contain information respecting high tides.

The Admiralty tide tables contain the time and height of every tide in the year for twenty-four of the principal ports of the United Kingdom. There are also numerous other tide tables published, which give the *heights* as well as the *times* of high water. Amongst these may be mentioned Holden's Liverpool tables, which contain, besides Liverpool, eight other ports (London included), and at Liverpool are held in higher estimation than the Admiralty tables, inasmuch as Holden's predictions take into account the effect of the diurnal inequality at Liverpool, which heretofore has been neglected in the Admiralty tables. There are also published at South Shields, Ainsley's, and at Hartlepool, Pearson's tables, and at Bristol, Arrowsmith's tables (formerly Bunt's), which have deservedly a high reputation for Bristol and the Bristol Channel ports generally.

Any one who will select from these publications the highest perigean spring tides about the time of the equinoxes, and will send them to the papers, can apparently earn for himself the credit of "predicting" high tides.

The increased range of tide in the Thames of about twenty inches during the last twenty years, is undoubtedly due, among other improvements, to the construction of the embankments, the increased water-way at the bridges at Westminster, and notably at Blackfriars, the improved line of wharfage continually being carried out, and the removal from the Pool of the colliers, which at low water acted as a dam, and prevented the improvement of the bed of the river.

An overflow in the Thames at above-average spring-tides is now a matter of meteorological circumstances only. It has been observed, I believe, without exception that the overflows have been caused by a strong northerly wind; the most disastrous overflows, however, have followed a strong south-west wind, changing suddenly to a stiff north-west wind. The reason is obvious. An increased amount of tidal water with a south-west wind and generally low barometer, is carried from the Atlantic to the northern parts of the North Sea, a sudden change in the wind to north-west brings the whole of this water to the southward, with probably little or no disastrous effects until it reaches the mouth of the Thames, where it meets with the tidal water of the English Channel brought through the Straits of Dover. It then rushes up the Thames, and an additional height is given to the water, amounting sometimes to as much as four feet or more if there is much flood water meeting it, and an overflow is the consequence. I find the effect of a south-west wind on the tide in the Thames, as traced on a self-registering tide-gauge I have placed at Greenwich pier, is to depress the water considerably. The high water of Monday morning succeeding the heavy gale of Sunday, November 11, was nearly two feet below the predicted height, the extreme pressure of wind, as registered at the Royal Observatory, being 31 lbs. on the square foot. In the middle of October the effect of a south-west gale was still greater, probably owing to its longer continuance, although the registered pressure did not exceed 23 lbs. No overflow need therefore be feared from a continued south-west gale.

Mr. Jenkins is perhaps unaware that Mr. Saxby has "predicted" high tides for many years, and that on one occasion, I believe in September or October, 1869, the Astronomer-Royal wrote reassuring the public that there was nothing extraordinary in the then forthcoming spring tides to occasion unnecessary alarm. If Mr. Saxby has discovered some law by which he can foretell the direction and force of the wind he will undoubtedly confer an inestimable boon by its publication, but from the following extract from the *Times* of November 5 he does not appear to claim any such knowledge:—"Capt. Saxby further states: 'If the wind should blow from a northerly quarter on either the 7th of November or 22nd of December next, very full tides may be reasonably expected.'" The spring tides about December 22 are slightly below average, and as no overflow has yet occurred with below-average spring tides, but little apprehension need be felt respecting them.

With respect to the actions of Venus and Jupiter; although theoretically they cause tides, the values have hitherto not been evaluated, being almost insensible.

The high tide of October 26th was entirely due to the northerly wind; the effect due to the maximum northern declination of the moon is very small in the Thames, and is more than counterbalanced by its effect in decreasing the value of the lunar semidiurnal tide.

Mr. Jenkins' statement respecting two great tides revolving through the year exactly six-and-a-half synodic months apart is merely on account of thirteen semilunations being very nearly equal to seven anomalistic months, and therefore the lunar perigee has again the same phase with respect to new or full moon. I may mention that ninety-nine semilunations exceed four years by about eighteen hours only, and also fifty-three anomalistic months by less than thirty-three hours. So that after a cycle of four years the perigean spring-tides fall very nearly on the same days of the year. This of course fails to take into account the variations due to the moon's declination.

The following table of the heights of the above-average spring-tides for London for next year may be useful not only to riverside owners and dwellers, but also to marine naturalists, who will on these days have unusually favourable opportunities at low-water of engaging in their pursuits. If at such times the barometer should be high the low-water level will be still further depressed. It will also act as a guide to tourists wishing to avail themselves of the best chances of witnessing the bore in rivers,

notably on the Severn, which, according to Mr. Alfred Tylor, F.G.S., is seen to best advantage with a rising sun from Stonebench Inn, about three miles below Gloucester.

1878.	Height above average. ft.	in.	1878.	Height above average. ft.	in.	1878.	Height above average. ft.	in.
Jan. 20 p.m.	0	4	April 17 a.m.	0	8	Sept. 1 a.m.	1	3
,, 21 a.m.	0	7	,, ,, p.m.	0	11	,, ,, p.m.	0	11
,, ,, p.m.	0	9	,, 18 a.m.	1	1	,, 2 a.m.	0	7
,, 22 a.m.	0	10	,, ,, p.m.	1	3			
,, ,, p.m.	0	11	,, 19 a.m.	1	3	,, 26 a.m.	0	1
,, 23 a.m.	0	10	,, ,, p.m.	1	0	,, ,, p.m.	0	7
,, ,, p.m.	0	7	,, 20 a.m.	0	9	,, 27 a.m.	1	0
,, 24 a.m.	0	4	,, ,, p.m.	0	5	,, ,, p.m.	1	4
			,, 21 a.m.	0	1	,, 28 a.m.	1	7
Feb. 18 a.m.	0	5				,, ,, p.m.	1	8
,, ,, p.m.	0	11	May 16 a.m.	0	1	,, 29 a.m.	1	7
,, 19 a.m.	1	4	,, ,, p.m.	0	2	,, ,, p.m.	1	4
,, ,, p.m.	1	5	,, 17 a.m	0	3	,, 30 a.m.	1	1
,, 20 a.m.	1	6	,, ,, p.m.	0	3	,, ,, p.m.	0	9
,, ,, p.m.	1	6	,, 18 a.m.	0	3	Oct. 1 a.m.	0	4
,, 21 a.m.	1	5	,, ,, p.m.	0	2			
,, ,, p.m.	1	3				,, 25 a.m.	0	2
,, 22 a.m.	0	10	July 31 p.m.	0	3	,, ,, p.m.	0	6
,, ,, p.m.	0	4	Aug. 1 a.m.	0	5	,, 26 a.m.	0	10
			,, ,, p.m.	0	7	,, ,, p.m.	1	0
Mar. 18 p.m.	0	3	,, 2 a.m.	0	9	,, 27 a.m.	1	2
,, 19 a.m.	0	9	,, ,, p.m.	0	9	,, ,, p.m.	1	4
,, ,, p.m.	1	3	,, 3 a.m.	0	9	,, 28 a.m.	1	2
,, 20 a.m.	1	6	,, ,, p.m.	0	6	,, ,, p.m.	0	11
,, ,, p.m.	1	9	,, 4 a.m.	0	3	,, 29 a.m.	0	8
,, 21 a.m.	1	9				,, ,, p.m.	0	4
,, ,, p.m.	1	7	,, 28 p.m.	0	1			
,, 22 a.m.	1	5	,, 29 a.m.	0	6	Nov. 24 a.m.	0	1
,, ,, p.m.	1	1	,, ,, p.m.	1	0	,, ,, p.m.	0	3
,, 23 a.m.	0	8	,, 30 a.m.	1	3	,, 25 a.m.	0	4
,, ,, p.m.	0	1	,, ,, p.m.	1	5	,, ,, p.m.	0	4
			,, 31 a.m.	1	5	,, 26 a.m	0	5
April 16 p.m.	0	4	,, ,, p.m.	1	5	,, ,, p.m.	0	3

From the above table it appears that the highest tides of the year will occur on March 20–21 and September 28. The heights will be found probably to exceed those of the Admiralty Tables, as I have employed larger factors in the necessary corrections to the semi-menstrual inequality.

As a London tide table appears to be a desideratum, I have been induced to publish one for next year, in which the "danger" tides will be distinguished in a new, bold, and unmistakable manner. EDWARD ROBERTS

3, Verulam Buildings, Gray's Inn, November 17

Rainfall in the Temperate Zone in Connection with the Sun-spot Cycle

THIS month's number of the *Nineteenth Century* contains an article on the connection of rainfall with the eleven years' cycle of sun-spots. It takes a carefully-selected area in which such a coincidence, if it existed, would be well marked. The great tract of water spreading southwards from Asia to the southern pole affords an arena for the undisturbed play of solar activity. It may readily be understood that any excess of solar energy has a more direct and uniform influence upon the rainfall gathered from this vast aqueous expanse, than it would have upon smaller areas of water intermingled with tracts of land, and cut off from each other by ranges of mountains, as in the European and American continents. Other reasons exist which would render solar influence a more directly potent factor in the rainfall gathered from the Indian Ocean than in that of the temperate zone. Without doing more than alluding to the fact that sun-spot activity is confined to a belt of considerable thickness on either side of the sun's equator, there are several well-ascertained causes which would render an excess of solar activity more directly felt in the equatorial regions of our earth than in those nearer the poles. While, therefore, I believe that the coincidence of a rain cycle and of a cycle of wind disturbances with the eleven years' cycle of sun-spots, has now been established as regards the Indian Ocean and the Madras rainfall, I am anxious to guard against the conclusion being pushed too far. The article in the *Nineteenth Century* proves much, but it would be a misfortune at this still early stage of the inquiry, if wider inductions were drawn from it than are justified by the evidence which it brings forward.

It seems right, therefore, to state that so far as my investigation of the rain returns of the temperate zone yet enables me to form an opinion, the cyclic coincidence of the rainfall with the eleven years' cycle of sun-spots, seems to shade off in extra-tropical regions until it ceases to exist at all. This opinion is based upon an examination of the returns of between one and two hundred stations in different parts of the world, but only with regard to one-third of them is the evidence sufficiently complete as to raise more than a presumption either for or against the existence of a cycle. Further, I have not yet been able, except in comparatively small groups of stations, to examine the monthly returns or to separate the winter from the summer rainfall. This separation forms one of the first essentials to arriving at a final opinion on the question. Subject to these remarks, I beg to state the facts with regard to the rainfall of the northern extra-tropical zone in India, Europe, and America. It is chiefly with the first and last-named countries that the present contribution will deal.

In my "Cycle of Drought and Famine," printed in India on the commencement of the late dearth, I mentioned that the rainfall which, in periods of minimum sun-spots, passes uncondensed over the Southern Presidency, might possibly "fall in the temperate zone. The excessive rain, if it takes place anywhere, will probably be found in India between the 22nd and 32nd degree of north latitude, to the south of the great Himalayan partition wall." The conjecture was based upon the configuration of the Indian continent, which, in its lower and middle regions, receives the rainfall gathered from a vast ocean, and is provided with a barrier at the upper end to arrest the rain-clouds on their further progress northward. Prof. Archibald's examination of the rainfall in Northern India now throws a clear light on this side of the question. He has published in the leading Calcutta paper, the *Englishman*, a series of carefully-compiled returns from stations within the ten degrees of latitude above mentioned. He shows that the rainfall of the sub-tropical region, from 22° to, say, 30°, is in some respects (but only in some respects) complementary to the rainfall of Southern India, and in a recent letter to me he thus summarises his conclusions:—First, the winter-rainfall of Northern India varies inversely with the sun-spots in a well-marked manner at all the stations. Second, the summer rainfall varies directly with the sun-spots, in a manner well marked in the north-western provinces, by no means marked in the lower provinces of Bengal, but sufficiently well marked when the returns of the several stations are combined.

Let us examine the meaning of these facts. The returns from Madras and Bombay (lately published in NATURE and elsewhere) prove that when the summer monsoon strikes Southern India, its aqueous burden varies directly with the sun-spots. Prof. Archibald's returns now show that the rainfall brought by the summer monsoon to Northern India also varies directly with the sun-spots. But they prove more than this. They show that the rain-clouds which, in years of minimum sun-spots pass over India without dropping their watery burden, are found, on their being stopped by the Himalayan partition wall, to be charged with a more than average surplus (so to speak) of moisture. In Northern India, therefore, the summer monsoon, on its passage up, brings, as in Southern India, a rainfall varying directly with sun-spot activity; but the winter rainfall, *i.e.*, the immediate rebound of the rain-clouds from the Himalayan barrier, varies inversely with sun-spot activity. I say the immediate rebound, for it must not be forgotten that the north-eastern monsoon (October to December), when it strikes Madras in its full development, after collecting its aqueous freight from the Bay of Bengal, follows the same law as the summer monsoon (May to September), and varies directly with the sun-spots.

Passing from the sub-tropical region of Northern India (22° to 32° lat.) to the temperate zone, we find the evidence of a cycle either very faint or altogether wanting. With regard to Europe, I am not yet prepared to offer any new facts. The existing evidence only amounts to this: (1) Mr. Baxendell, from observations for a comparatively short period but very carefully recorded and scrutinised, came to the conclusion that even at an English station, notwithstanding the manifold disturbing influences incident to our insular meteorology, changes take place in the rainfall as well as in the temperature and barometric pressure,

which correspond closely in their maxima and minima periods with those of sun-spots. (2) A more comprehensive survey of the European rainfall has so far failed to establish this correspondence. Dr. Jelinek's examination of fourteen stations, from 1833 to 1869, showed that the coincidence held good in fifty-two cases, but failed in forty-two. While frankly accepting this as evidence against a real coincidence, it should be remembered that a general law such as a common periodicity in sun-spot activity and terrestrial rainfall will be subjected to, and sometimes overruled by, the local surroundings of individual stations. (3) On the other side, Gustav Wex, from the recorded depths of the Elbe, Rhine, Oder, Danube, and Vistula, for six sun-spot cycles (1800–1867), found that the maximum amount of water occurred during periods of maximum sun-spots, while the minimum levels were reached in the periods of minimum sun-spots. The evidence, as regards Europe, is, therefore, conflicting; and it is safer for the present to reckon it as against a well-marked common periodicity. I hope at no distant date to submit the results of a new and more exhaustive examination of the European rain-registers.

I now proceed to the North American rainfall. Here, as in Europe, the question is complicated not merely by disturbing meteorological influences, such as the Gulf Stream, but by the uncertain value of the rain-returns. These are causes which even at a carefully supervised station render it difficult to estimate the number of inches yielded by long-continued or very violent snow-storms. At badly supervised stations, or in the case of private gauges where the supervision is apt to be of a still more haphazard character, these difficulties often suffice to render the returns quite worthless. Yet it is the latter class of records on which we have chiefly to depend in an attempt to deal with the American rainfall during a long series of years. Nowhere does meteorology now receive more careful and scientific study than in the Western Continent, but in many of the most valuable series the element of time is still necessarily wanting. The evidence hitherto received from America has, on the whole, been favourable to the existence of a common periodicity. Mr. Dawson, Geologist to the British North American Boundary Commission, found a correspondence, although by no means an absolute one, between the fluctuations of the great lakes and the sun-spot periods. This question has been lately revived and interpreted afresh by a distinguished meteorological observer in Northern India. Prof. Brocklesby's contributions to the *American Journal of Science* also point to a connection between variations in the sun-spot area and annual rainfall.

It was with a knowledge of these statements that I undertook a systematic inquiry into the American rain-returns. I ought at once to say that the result of that inquiry altogether fails to establish the existence of a common cycle, so far as concerns the temperate zone. I divided the American stations into four groups. The first group consisted of eleven stations in east coast or Atlantic States, lying between 40° and 45° N. latitude. The second group consisted of seven stations in Inland States, from 38° to 48°. The third group was intended to consist of stations in the West Coast or Pacific States, but I have obtained the returns (and those for a period altogether too brief) for only a single West Coast Station, San Francisco. I give them, however, for what they are worth. The fourth group consists of three coast-stations in the Southern States, between 30° and 33°; or just above the sub-tropical region with which Mr. Archibald's returns for the Bengal stations deal.

The results of the examination of the four American groups may be summarised thus: (1) Taken as a whole, the returns from the twenty-two stations do not exhibit any common periodicity between the rainfall and the sun-spots; nor do they disclose an eleven year's cycle corresponding to the one which I have shown to exist in the rainfall (at Madras and elsewhere) gathered from the Indian Ocean. (2) That as regards the three northern groups, stretching across the continent from 38° to 48° N. lat., the rainfall, so far as any symptoms of periodicity can be detected at all, tends to vary inversely with the sun-spots; but that it is impossible to discover any real periodicity whatever. (3) On the other hand, that as regards the southern group, between 30° and 33°, there are symptoms of a periodicity tending to coincide with the sun-spot variations; but that these symptoms are not sufficiently uniform in the small number of southern stations which I have examined, to justify any conclusion.

The calculations on which these results are based would occupy many pages, but their general line may be indicated in a few sentences. Thus the mean rainfall at the twenty-two stations during the years of *maximum* sun-spots for which the records have been obtained, was 37½ inches, while during the years of minimum sun-spots it was 39. The years of maximum sun-spots, together with the years immediately preceding, had a mean fall at the twenty-two stations of 40·2 inches; while the minimum years of sun-spots, taken together with the years immediately preceding, had an almost exactly equal rainfall of 40·1 inches. In the northernmost group of eleven Atlantic stations the mean rainfall of the years of maximum sun-spots was 39 inches, against an average of 41 inches in years of minimum sun-spots; in the second group of seven inland stations (38° to 48°) the mean rainfall of the years of maximum sun-spots was precisely equal to that of years of minimum sun-spots, being 33¾ inches in both; in the third group, San Francisco, the mean rainfall years of maximum sun-spots was 21 inches against 23½ inches in minimum years; in the fourth group of three southern stations (30° to 33°) the returns for the minimum and maximum years are broken; but taking these years and the preceding ones together, the mean rainfall of the years of maximum sun-spots with the years immediately preceding was 51 inches, against 48½ inches in the years of minimum sun-spots and immediately preceding ones.

The returns have also been examined by another method. I have shown elsewhere that the rainfall at Madras, and other stations around the Indian Ocean, follows a well-marked cycle of eleven years, with a maximum, minimum, and intermediate period, corresponding with the maximum, minimum, and intermediate period of sun-spots. The American stations not only fail to show such a correspondence, but as regards the three northern groups so far as any symptoms of periodicity exist, they point in the opposite direction. The fourth or southern group of stations, on the other hand, so far as they disclose a periodicity, tend to coincide with the periodical variations in the sun-spots. The following table will show this. The Madras rainfall in the tropics discloses a cycle closely corresponding with the eleven cycle of sun-spots; speaking generally, the American rainfall in the temperate zone discloses no such cycle; but the southern stations begin to furnish symptoms of such a cycle.

Table of Madras and American Rainfall Compared with the Eleven Years' Cycle of Sun-spots

Rainfall and sun-spots shown in the minimum, intermediate, and maximum groups of the eleven years' cycle.	*Minimum Group.* Mean of 11th, 1st, and 2nd years.	*Intermediate Group.* Mean of 3rd, 4th, 9th, and 10th years.	*Maximum Group.* Mean of 5th, 6th, 7th, and 8th years.	Remarks.
Eleven years' cycle of sun-spots (from Wolf's lists)	12·6	43·5	76·8	Common Periodicity well-marked.
	Inches.	Inches.	Inches.	
Eleven years' cycle of rainfall at Madras	40·3	49·0	53·5	
Eleven years' cycle of rainfall; mean of three stations around the Indian Ocean...	43·4	48·1	52·2	
North American Rainfall.				
Mean of eleven stations in East Coast States, 40° to 45° N. lat....	40·2	41·6	40·1	No common Periodicity.
Mean of seven stations in Inland States, 38° to 48° N. lat.	35·3	35·8	34·6	
San Francisco; West Coast Station, 38° N. lat.	22·9	19·9	22·3	
Mean of three stations in Southern States, 30° to 33° N. lat.	47·0	51·2	49·1	Symptoms of common Periodicity.

NOTE.—The sun-spot figures represent the relative numbers, reduced from Wolf. The rainfall is expressed in inches. The San Francisco returns deal with only twenty-one years, or not quite two complete cycles; much too short a period for any definite conclusion.

The records of the twenty-two American stations extend over brief periods compared with the Madras returns. Several of them disclose breaks or gaps; few of them have been kept with the minute care bestowed by the professional astronomical staff on the rain gauge at the Madras Observatory, and the value of most of the eighteen northern ones is rendered in some degree uncertain by snow-storms. It is probable, moreover, that better and much more complete returns are available to American meteorologists than I possess for the twenty-two stations which

I have examined. They will come to the criticism of my results with fuller materials than are available to me here, but so far as these materials enable me to form an opinion, the result is against the existence of a common periodicity in the sun-spots and in the American rainfall within the temperate zone.

Allanton, Lanarkshire, November 4 W. W. HUNTER

Contribution to the Sun-spot Theory of Rainfall

THE Lucknow Meteorological Observatory has been established since 1868, and regular observations have been recorded since that year under my superintendence.

In NATURE of December 12, 1872, Mr. Lockyer published a notice of Mr. Meldrum's discovery of the coincidence between the maximum and minimum sun-spot periods, and the maximum and minimum rainfall in certain places. After reading it I examined the annual rainfall at Lucknow from 1868 to 1872, and found that there was reason to believe that the rainfall at Lucknow followed the same cycle as that of the sun-spots. The figures were :—

1868	27·6 inches.
1869	41·9 ,,
1870	64·6 ,,
1871	65·0 ,,
1872	41·4 ,,

The equal amount of rainfall (41 inches) on both sides of the maximum fall of 1870 and 1871 was very striking, and as there was a rise in the rainfall from 1868 to 1870–71, and after that a decrease, and having just read Meldrum's discovery, I conjectured that the annual rainfall would continue to decrease till it reached its minimum. In my annual abstract, which I submitted to Government in April, 1873, and on the slender evidence of five year's rainfall, I ventured to state that if Meldrum's law be true, we had in Lucknow lately passed the period of maximum rainfall, and were descending towards a minimum, so that during 1877, 1878, and 1879 there would be a scarcity of rain, and in one of those years the minimum rainfall of the cycle would occur. I am now able to give the annual rainfall of almost a complete cycle, and the figures will speak for themselves :—

1867 was a sun-spot minimum period.		
1868	27·6	Inches of rainfall in Lucknow.
1869	41 9	
1870	64·6	
1871	65·0	
1872	41·4	
1873	35·1	
1874	51·4	
1875	43·5	
1876	23·6	
1877	11·7	
(Up to date October 22).		

This is October 22, 1877, and the total fall up to date has been only 11·7 inches, about a third of which fell in the months of January, February, and March. The fall during the rainy season of 1877 has been so small that great fear of a famine has been felt. I considered Meldrum's discovery so important that at the end of my annual abstract of meteorological observations for 1872, I inserted a long abstract of Mr. Lockyer's article in NATURE, in order to make the theory more widely known.

I believe meteorologists are on the track of a most important law. I would not expect the maximum and minimum rainfalls in every place to coincide with the sun-spot maximum and minimum so completely as that given above. Possibly in some places the figures might be reversed, owing to a changed direction in the water-bearing currents of the atmosphere; but that the changes occurring in the sun have a direct influence on rainfall there cannot, I think, be any doubt. E. BONAVIA

Lucknow, October 22

The Radiometer and its Lessons

I WISH that Prof. G. C. Foster had been more explicit in his answer to my letter; for as it is I cannot understand to what "variations of density" he refers. So far as I know there are no variations of density in the gas in question except those which arise from variations of temperature; but these variations of density certainly do not affect the rate at which heat diffuses into and through the gas, for this rate is independent of the density and for the same gas depend only on the absolute temperature and on the degradation of temperature in the direction in which the diffusion takes place. The variations of temperature do affect the rate of communication but only in proportion to the square root of the absolute temperature, and hence, in the case of the radiometer, only to an inappreciable extent.

It is obvious that the law of diffusion holds good only so long as the gas is undisturbed by convection currents. Such currents, which certainly exist, increase the rate at which heat is communicated to the gas, that is to say, the hot surface instead of being exposed to the action of still air is exposed to a wind which tends to increase the rate of cooling. But the velocity of the wind does not increase with the rarefaction, and the cooling effect of a wind of a certain velocity does increase with the density of the air. Hence, as I pointed out in my first paper, the motion of the air will favour the force resulting from the communication of heat less and less as the rarefaction is increased.

As regards Mr. Johnstone Stoney's theory. The post which brought me this week's NATURE brought me also a paper from Mr. Stoney, on which I venture to comment. In doing this, however, I may say that I have no wish to criticise what Mr. Stoney has written. The fact that Mr. Stoney has in no way referred to my work, although I preceded him by some two years, has relieved me from all obligation to discuss Mr. Stoney's theory; and I certainly should not do so now were it not that, as Prof. Foster has instanced this theory as disproving what I believe to be the truth, I feel bound either to show wherein it is wrong or acknowledge my inability to do so.

In the paper which I have just received,[1] Mr. Stoney starts with an assumption that, but for the effect of gravitation, "a flat stratum of gas in contact with a hot surface, A," and "everywhere subject to the same pressure" can exist in a state of equilibrium "except at the limits," without any passage of heat from the hotter to the colder part, although "within the stratum the temperature gradually decreases, from within outwards, from θ_1 the temperature of A to θ_2 the temperature of the surrounding gas."

In support of this assumption I cannot find that any proof is offered except that which is contained in the following portion of a sentence:—"We know, from familiar experiments, which show gases to be bad conductors of heat, that after the brief interval of adjustment a permanent state would ensue in which there would be no further change of density, or motion of heat, except by radiation."

Now this assumption and the statement in support of it—in both of which Mr. Stoney seems to have ignored the very existence of diffusion of heat in gases—are contrary to all experience as well as to the deductions from the kinetic theory of gases; for it follows directly from the kinetic theory, and has been abundantly established by experiment, that under no circumstances can there exist a variation in the temperature of a continuous layer of gas without heat diffusing from the hotter to the cooler part.

I think that I need say no more. This assumed condition of gas forms the base of all Mr. Stoney's reasoning, and although in a subsequent part of his paper he appears to me to have arrived at deductions which contradict his fundamental assumption, still this assumption may be held accountable for the anomalies which he has found. OSBORNE REYNOLDS

November 17

I BEG to call the attention of the readers of NATURE to the following passage at the commencement of Mr. Crookes's lecture at the Royal Institution on February 11, 1876, "On the Mechanical Action of Light":—

"To generate motion has been found a characteristic common, with one exception, to all the phases of physical force." [Illustrations are then given of the production of motion by heat, magnetism, electricity, gravitation, sound, and chemical force.]

"But *light*, in some respects the highest of the powers of nature, *has not hitherto been found capable of direct conversion into motion;* and such an exception cannot but be regarded as a singular anomaly.

"This anomaly the researches which I am about to bring before you have now removed; and, like the other forms of force, *light is found to be capable of direct conversion into motion*, and of being most delicately and accurately measured by the amount of motion thus produced."

I cannot but suppose that Mr. Crookes and Prof. Carey Foster have alike forgotten the existence of this passage. If it does *not* convey an interpretation of the phenomena of the radiometer which is now admitted on all hands to be wrong, and imply a claim to the discovery of "a new mode of force," I am incapable of understanding the meaning of words.

I may add that one after another of my eminent scientific

[1] "On the Penetration of Heat across Layers of Gas," *Scientific Transactions* of the Royal Dublin Society, November, 1877.

friends has assured me that I was perfectly justified in my statement on this point; and it was by one of these, who was present at the lecture in question, that I was informed of the very explicit statement made on that occasion by Mr. Crookes of the views he then held, which were universally understood in their plain common-sense meaning.

November 20 WILLIAM B. CARPENTER

Fluid Films

WITH reference to Mr. Sedley Taylor's interesting note on Fluid Films, allow me to say that if a drop of water, clinging to the outside of a glass goblet, be lightly dusted with lycopodium powder, and a fiddle-bow be drawn across the edge of the glass, the drop will exhibit vortices, rotating in opposite directions.

Highgate, N., November 19 C. TOMLINSON

Tuckey and Stanley.—The Yallala Rapids on the Congo

CAPT. TUCKEY is dead and gone and cannot answer for himself; it may therefore, perhaps, serve to clear his memory in some measure of a doubt about the correctness of his description of the Yallala Rapids in 1816, arising from the very different account of them given by Stanley sixty years afterwards, if I mention one of several facts in connection with American rivers.

The late Sir J. Franklin, in his first and disastrous overland journey to the Arctic Sea in 1821, describes the "Bloody Fall" on the Coppermine River as "a shelving cascade about *three hundred yards* in length, having a descent of ten or fifteen feet."

Between 1848 and 1851 this "fall" was visited five times; on one or other of such occasions the water was either at high spring flood, at low summer level, or at an intermediate elevation, yet under none of these conditions was the "fall" found to be more than *thirty yards long*, if so much, the height being about fifteen feet.

Franklin and the officers with him were most careful and correct observers, so that I can only attribute this wonderful change (from three hundred yards long to thirty) in the form of the cascade to the wearing away of the material forming the bed of the river, by the action of the water, assisted in a great measure by the large masses of ice and the stones carried down with it during the breaking up of the navigation in the course of thirty seasons, only half the interval of time between Tuckey's and Stanley's visits to the Congo.

Supposing a somewhat similar attrition, but in a less rapid manner, to have been going on at the Yallala Rapid, the description given by the former as he saw it may be equally correct as that of the latter when he visited it in its altered shape in 1877.

May I add that a cataract may become a fall or a series of falls, and *vice versâ*, according as the water in a river is in flood or at low level. J. RAE

Scientific Club, November 16

The Future of our British Flora

IT may interest Mr. Shaw to know that the stations given by Lightfoot in his "Flora Scotica, 1777," still exist (as far as I am aware, and I have visited by far the greater number of them) at the present day. Experience has led me to the conclusion that a plant however maltreated, does not become extinct unless the *natural conditions are changed*, as by the draining of a marsh, &c. I have over and over again found plants in stations where they were reported as "extinct years ago." Perhaps if Mr. Shaw visits his station for the "Lizard Orchis" (is this *Orchis hircina*, L.? if so it is, I fancy, new to Scotch botanists) in the course of a year or two he may find it in as large quantity as ever. As regards the maltreatment of plants, I agree with what Mr. Shaw says respecting professors of botany. Each teacher of the science ought to teach his students that it is a crime to exterminate a plant, and that they can best learn botany from the observation of the common plants of their district; there is great room for improvement in this respect.

While a student I was often disgusted by seeing rare plants torn up and then cast away as if they had been a handful of grass, or, worse still, put in the vasculum and forgotten till the next Saturday, when they were thrown away; and all this without a word of remonstrance from those who ought to have exercised authority, "that's villainous, and shows a most pitiful ambition in the man who uses it."

Provided we reform a little, I do not think that, judging of the future by the past we have any reason to expect a large decrease in the ranks of our native flora. I do not suppose any species given by Lightfoot 100 years ago has become extinct even in *his* stations, and on the other hand we have had a considerable number added to it since his time.

Edinburgh A. CRAIG-CHRISTIE

Selective Discrimination of Insects

IN continuation of the interesting observations of "S. B." on selective discrimination of insects in NATURE, vol. xvi. p. 522, permit me to send you the following notes from my journal, made in August last:—

"Watched by the roadside near Kew Bridge Station, several species of Hymenoptera, of the genus *Bombus* principally; one visited thirty flowers of *Lamium purpureum* in succession, passing over without notice all the other plants in flower on the same bank—species of *Convolvulus*, *Rubus*, *Solanum*. Two other species of *Bombus* and a *Pieris rapæ* also patronised the *Lamium*, seeking it out deep in the thicket, thrusting their probosces even into withered cups, although the *Rubus*' flowers were far more accessible and seemed much more attractive, being fresh and well-expanded.

"On the same bank several species of Diptera—*Syrphus* chiefly—were visiting the *Rubus*, ignoring the *Lamium*. On another bank, some distance removed from the first, I observed, however, that the diptera were visiting the *Lamium* (one species was very busy on the convolvulus, applying its proboscis to the external aspect of the anther) while the Hymenoptera, species of wasp, were giving their attention to the *Rubus*."

I am sorry not to be in a position to identify the species of Hymenoptera and Diptera, being unable to capture specimens of either. HENRY O. FORBES

Highgate, N.

The Earth-worm in Relation to the Fertility of the Soil

IN your number of the 8th instant there are some interesting remarks upon the habits, &c., of the common earth-worm. From frequent observations I fully concur with the remark that the worm does not consume living vegetation but only vegetable matter undergoing decomposition.

I am also rather inclined to the opinion that there are (or may be) two reasons for the drawing in to their holes dead leaves, &c., the one being, for use as food, and the other to protect the holes from a too plentiful supply of water.

In this same connection I may mention what I have not before seen mention of, namely, the little mounds of small gravel stones which the worms heap up around the entrance to their holes. These are very curious and may be partly to prevent the entrance of water; and also, as I think, partly for rubbing against the worm's slimy body, as fish do.

It is very remarkable the extent to which loose gravel-stones (some as large as a hazel-nut, and even larger) are removed from a gravel-walk from distances quite beyond a foot, leaving the walk pitted all over. I have never seen a worm in the act of moving these stones and it is difficult to imagine how it is done, but as it generally takes place in wet weather, it may probably be by an adhesion of the stone to the slimy body of the worm.

As regards fertilising effects, it would be interesting to know whether the earthy matter composing worm-casts had passed through the worm's body, as the writer supposes, for in that case it would probably have more fertilising properties than if consisting merely of the natural soil thrown up as by moles.

The remark by one of your correspondents as to his observation of a line of darker soil thrown up by worms from a substratum of ashes deposited a considerable time before, would almost make it appear that the mole-like action above referred to took place. The writer, however, repeats his conviction that the matter composing worm-casts has passed through its (the worm's) body.

31, Stockwell Park Road GEO. H. PHIPPS

Smell and Hearing in Moths

"J. C." seems to draw inferences that moths have not the power of smell but have that of hearing. I feel quite certain they possess the former, but am in doubt about the latter. For the purpose of catching moths I use a preparation of beer and

sugar boiled together, to which (after boiling) is added a little spirit, placing rags several folds thick, saturated in the preparation, upon garden-seats, low branches of trees, &c. I have in one evening taken as many as thirty-six moths (including red-, yellow-, crimson-underwing, swordgrass, angleshade, &c., &c.). What has attracted them unless smell? or what generally leads them to their food?

With reference to the sound of the glass, is it not the quick motion of the hand which disturbs the moth? E. H. K.

Carnivorous Plants

PROF. SERRANO FATIGATI, of Ciudad Real (Spain), has made some investigations upon two insect-feeding plants which he found during his last excursion to the province of Cordova, and on the general peculiarities of viscous plants during their flowering. The first of these plants is *Ononis natrix*; it grows at Sierra Palacios. The second appears to be *Silene viscosa*, and was found on the hill which connects the village of Belmery with the station. The experiments made upon these plants prove that when alive they were both covered abundantly with a viscous fluid, which in *Silene* was still visible after the specimens had been dried for four months. Prof. Fatigati has observed in several instances that every insect which touches their surface, and remains adherent to them, dies in a very few minutes. Remains of animals in different stages of decomposition may be seen on the plants he possesses.

The microscopical study of these plants has enabled the structure of their secretory glands to be examined. The glands of the plant *Ononis* are at the extremity of hairs composed of cylindrical cells, and are ovoid and multicellular. The protoplasm of the cylindrical cells always forms a parietal coating to the cell-wall. The glands of the *Silene* are simply conical epidermical protuberances, and are divided into two cells at the close of their development.

Prof. Serrano Fatigati has observed that in these species and in *Cistus ladaniferus* the secretion of the viscous fluid increases during their period of flowering; he is studying this matter, in order to ascertain whether this circumstance bears any connection with the production of heat and carbonic acid possessed by plants during the flowering period. FRANCISCO GINEZ

Esparteros 9, Madrid

OUR ASTRONOMICAL COLUMN

MINOR PLANETS.—Mr. J. N, Stockwell, of Cleveland, Ohio, who has had much experience in calculations relating to the small planets, draws attention to a curious circumstance connected with the observations of Gerda, discovered by Prof. Peters at Clinton, N.Y., on July 31, 1872. It had been supposed that this planet was observed again in 1873, 1876, and 1877, but on forming equations of condition for the correction of the elements, Mr. Stockwell found that the observations of 1873 are quite irreconcilable with those of the other oppositions, or that some incompatible conditions had been introduced into the equations. "The discovery of these incompatible conditions," he writes, "has been the occasion of an unusual amount of trouble and annoyance, and will be the source of future mortification, should the explanation at which I have arrived ultimately prove to be erroneous." Mr. Stockwell's conclusion is this, that notwithstanding the planet observed from September 27 to November 12, 1873, was very near the computed place of Gerda, it was really another body that was observed in that year. To decide this point he calculated an orbit upon the observations of 1873, which it appears are very well adapted to furnish reliable results, and finds the following elements, placing the elements of Gerda, as perturbed to the same date, in juxtaposition for the sake of comparison. The epoch is 1873, November 7·0 M.T. at Washington, longitudes from M.Eq. 1873·0:—

	PLANET OF 1873.	GERDA.
	° ′ ″	° ′ ″
Mean long.	35 4 57	35 47 14
π	213 14 38	208 19 29
☊	178 53 9	178 56 40
ι	1 36 3	1 36 19
ϕ	1 58 40	2 0 51
μ	613″·6390	614″·3842

It will be seen that four of the elements of the planet of 1873 are almost identical with those of Gerda, while the lines of apsides differ about five degrees. The actual distance of the planets from each other on November 7 would be 0·0188 of the earth's mean distance from the sun. Mr. Stockwell adds, "if there are really two planets moving in orbits so extremely near together, it must happen in the course of time, unless the mean distances are exactly the same, that they will approach each other so closely that their mutual perturbations will cause them to unite and form a single planet."

A similar case of near coincidence between the orbits of two minor planets is that of Fides and Maia, to which attention was first directed we believe by M. Lespiault, of Bordeaux. In 1876 the elements were as follow:—

	FIDES.	MAIA.
Epoch	July 27·0 Berlin M.T.	Oct. 4·5 Berlin M.T.
	° ′ ″	° ′ ″
Mean long.	326 33 33	27 37 21
π	66 27 20	48 8 26
☊	8 15 15	8 17 1
ι	3 6 49	3 5 40
ϕ	10 11 21	10 4 31
μ	826″·4417	824″·6400

Here, however, the planets are much further from each other than in the case of Gerda and the planet of 1873.

At present Gerda and its companion will not be favourably placed for observation, but in the ensuing year no doubt an effort will be made to decide if there are really two bodies revolving in such near proximity to each other. Questions of much interest may arise if this should prove to be the case.

The discoveries of minor planets during the present year now stand as follow:—

No. 170, Myrrha, January 10, by Perrotin, at Toulouse.
,, 171, Ophelia, January 13, by Borrelly, at Marseilles.
,, 172, Baucis, February 5, ,, ,,
,, 173, ... August 2, ,, ,,
,, 174, ... September 2, by Watson, at Ann Arbor, U.S.
,, 175, ... October 14, by Peters, at Clinton, U.S.
,, 176, ... November 5, by Paul Henry, at Paris.
,, 177, ... November 6, by Palisa, at Pola.
A planet, ... November 12, by Watson, at Ann Arbor.

We adopt Prof. Peters' name for No. 170, instead of the inappropriate one proposed in France.

THE COMET OF 1672.—Mädler has pointed out a distant resemblance between the elements of the comet of 1672 calculated by Halley, and those of the comet of 1812, which has been found to have a period of revolution of about seventy years, and which therefore might have been in perihelion in the former year. The comet of 1672 was observed by Hevelius from March 6 to April 21, and also by Richer off the coast of Africa during his voyage to Cayenne, from March 15 to the end of the month, though he only described its position roughly. The observations of Hevelius are published in the rare volume of his "Machina Cœlestis" (of which, by the way, the British Museum possesses two copies), and we believe in the small special publication issued at Dantzig in the same year, and entitled, "J. Hevelii, Epistola de Cometâ, anni 1672, Gedani observato, ad Henricum Oldenburgium."

Halley's orbit gives for three dates of observation by Hevelius, adopting his corrected times, the following positions:—

	G.M.T.	Right Ascension.	Declination.
	h. m.	° ′	° ′
1672, March 6, at	15 39	353 16	34 57 N.
,, ,, 15, at	7 44	18 2	37 25
,, ,, 29, at	8 8	52 21	30 21 N.

Without attempting an accurate reduction of the Dantzic observations, it may be seen that they agree sufficiently well with the positions deduced from Halley's orbit to render it probable that his elements would not be so far changed by a calculation from the improved places as to bring them materially closer to those of the comet of

1812, the re-appearance of which is shortly expected. We have already mentioned that sweeping-ephemerides have been prepared by Herr Mahn, of Strasburg, and may be found in "Vierteljahrsschrift der Astronomischen Gesellschaft, 12 Jahrgang, 2 Heft."

MR. DARWIN AT CAMBRIDGE

AS we intimated last week, the honorary degree of LL.D. was conferred on Mr. Charles Darwin at Cambridge on Saturday. The occasion was in many ways remarkable, and suggestive of reflections that must occur to all, and which need not be put formally into words. The university seems to have been conscious of the honour Mr. Darwin was doing it, and seldom, it is said, was a more exciting scene seen in the senate-house. To appoint a special congregation of the senate for the transaction of no other business but the conferment of a solitary degree, although it be *honoris causâ*, is only resorted to in exceptional and important cases. The step taken by the university evidently has met with general approval to judge by the tone of the assembly in the senate-house on Saturday. The building was packed, and the inevitable pastime of the undergraduates assumed a form extremely appropriate, however questionable its taste may have been.

The appearance of Mr. Darwin entering the senate-house by a side door, with the Master of Christ's, of which College Mr. Darwin is a member, was the signal for a burst of applause which was evidently the result of genuine enthusiasm, and was certainly thoroughly hearty. At two o'clock the Vice-Chancellor took his seat on the raised dais, and the business of the day began. Standing side by side with Mr. Darwin in the centre of the senate-house, Mr. Sandys, the Public Orator, commenced the delivery of the customary Latin oration. Interruptions from the galleries occasionally interfered with the orator's efforts to make himself heard, but the pleasant manner of his delivery, combined with great tact and judgment, helped to quiet the undergraduates' "chaff," and assisted him materially in getting through his task.

We have been favoured with a copy of the Public Orator's address, which our readers will no doubt read with interest, both on account of the elegance of its Latin, and for its neat summary of Dr. Darwin's work; indeed, in its way, it is somewhat of a literary curiosity.

"ORATIO AB ORATORE PUBLICO HABITA CANTABRIGIAE DIE XVII° NOVEMBRIS A. S. MDCCCLXXVII

"DIGNISSIME domine, domine Procancellarie, et tota Academia:—

"Meministis Horatianum illud, 'fortes creantur fortibus'; vix igitur necesse est commemorare viri huius de rerum natura optime meriti patrem fuisse medicum egregium, avum poetam quoque insignem. 'Doctrina sed vim promovet insitam'; iuvat igitur recordari pueritiam huius fovisse scholam celeberrimam Salopiensem; adulescentiam aluisse non modo Caledonicas illas Athenas, sed in hac etiam Academia Miltoni nostri Collegium. Tanti in laudem alumni, nisi fallor, ipsa paterni fluminis nympha, non immemor hunc primum patefecisse insularum corallinarum originem, illa inquam Sabrina quae Miltoni in carmine vivit,

> curalio nltida roseum caput exseret unda,

frontemque tam venerabilem sua praecinget corolla.

"Quanta cum voluptate accepimus insularum illarum circulos, sese e vadis sensim attollentes, quasi florum immortalium palmarumque victricium corona locos illos virides placidosque in Oceani campo designare, ubi priores insulae depressae et sepultae sunt. Quam facete describit, quo modo varios sensuum affectus exprimant indices illi vultus et ipsa tacitorum oculorum eloquentia; quo more apes, dum dulce illud nectar e flore delibant, quod continuandae floris stirpi utile sit, ipsae aliunde referant. Quam venuste explicat, quo modo captet Venus ipsa muscas; quali ex origine sint Veneris volucres, 'raucae, tua cura, palumbes'; quibus cantuum illecebris, quo splendore plumarum, concilientur volucrum amores. Quam familiariter, velut rex ille excellenti sapientia, de tot rebus disserit, quicquid volat, quicquid natat, quicquid serpit humi; quam varia eruditione disputat de fabuloso illo lepadum balanorumque marinorum genere, de montium igneorum miraculis, sed idem de gracili vitis pampino et lentis hederarum bracchiis in apricum enitentium; quanta liberalitate in patrocinium suum vindicat non modo 'aurea pavonum saecla,' sed etiam minus pulchram simiarum familiam. Qua de re quanquam poeta vetus dixit, 'simia quam similis nobis'; nobis tamen, viri Academici, cum oratore Romano, viro Academicae praesertim philosophiae dedito, gloriari licet, 'mores' esse 'in utroque dispares.'

"Illud certe extra omnem controversiam constat, pulchrum esse tantam rerum naturae varietatem contemplari, regiones remotas invisere, silvarum incaeduarum solitudinem penetrare, insularum prope ignotarum recessus perscrutari, varias denique animalium formas comparare inter se et distinguere; pulchrius, haec omnia accuratissime observata aliorum in usum voluptatemque litterarum mandare monumentis; omnium pulcherrimum, infinita talium rerum multitudine ad leges quam paucissimas revocata, ipsum fontem et originem omnium repetere. Quanta igitur laude vir hic dignus est, qui adhuc iuvenis, aliorum magis quam suo commodo, tot terras lustraverit, lustratas feliciter descripserit; qui maturiore aetate, tot generibus animantium et earum rerum quas terra gignit diligenter investigatis, illi praesertim legi constituendae operam dederit, qua docere conatus est, ita e perpetuo prope ad internecionem debellantium certamine aptissimam quamque novae stirpi propagandae speciem vivam victricemque superesse, ut tot species inter se diversae alia ex alia minutatim per immensam annorum seriem generari potuerint.

> 'Usus et impigrae simul experientia mentis
> paulatim docuit pedetemtim progredientes.
> sic unumquicquid paulatim protrahit aetas
> in medium ratioque in luminis erigit oras.
> namque alid ex alio clarescere et ordine debet
> *omnibus*, ad summum donec venere cacumen.'

"Tu vero, qui leges naturae tam docte illustraveris, legum doctor nobis esto.

"Duco ad vos CAROLUM DARWIN."

The conclusion of this oration was greeted with loud applause, and the proceedings ended with the Vice-Chancellor conferring the degree on Mr. Darwin in the usual formal manner.

In the evening the anniversary dinner of the Cambridge Philosophical Society was given in the Hall of Clare College. The president of the Society, Prof. Liveing, occupied the chair, and among the visitors present were Professors Huxley, Ramsay, Tyndall, Parker, Burdon Sanderson, Drs. Günther, Wilks, Pye Smith, Mr. Francis Galton, &c. Prof. Ramsay proposed the toast of the University of Cambridge, and Prof. Huxley responded to that of Mr. Darwin, who was unable to be present. In his speech Prof. Huxley sarcastically spoke of the University as reserving its highest honour till all other distinctions had been heaped on Mr. Darwin, that its own chaplet might crown the whole, and not be covered up. Prof. Huxley spoke of Mr. Darwin as the foremost among men of science, with one exception, since the days of Aristotle.

A special meeting of the Philosophical Society is to be held next Monday in the combination room of Christ's College, to consider the best means of making a permanent memorial of Mr. Darwin in the University. Would not a Darwin Professorship of General Biology be a very suitable memorial?

INTERNATIONAL GEOLOGICAL CONGRESS

AT the late meeting of the American Association for the Advancement of Science at Nashville, Tenn., Dr. T. Sterry Hunt presented a report on the above subject, of which at the time we gave a brief note. The following extracts, which have been sent us, will no doubt be more satisfactory to geologists:—

"The committee to arrange for an International Geological Exhibition and Congress, to be held in Paris in 1878, was appointed by this Association at Buffalo in August, 1876, and consisted of Messrs. W. B. Rogers, James Hall, J. W. Dawson, J. S. Newberry, T. Sterry Hunt, R. Pumpelly, and C. H. Hitchcock, together with T. H. Huxley for England, O. Torrel for Sweden, and E. H. von Baumhauer for Holland. At a meeting of the committee at Buffalo on August 25, 1876, James Hall was chosen chairman, and T. Sterry Hunt secretary. It was then agreed to prepare a circular setting forth the plan of an International Geological Exhibition, which should form a part of the general exhibition to be held at Paris in 1878, and indicating a scheme for the organisation of the geological collections to be sent thereto by the nations taking a part in that exhibition, and moreover, proposing an International Geological Congress to be held at Paris.

"The circular in accordance with this plan was duly prepared, and printed in English, French, and German, and before the end of the year had been sent by the secretary to the principal scientific societies and academies, as well as to the workers in geology throughout the world. The response to this invitation has been most gratifying. The Geological Society of France has formally recognised the great importance of the objects proposed, and promised its hearty co-operation, while private letters from its president to the secretary of the committee, and from Prof. Hébert to Prof. Hall, give cordial assurances of the same kind. Spanish and Italian geologists have translated and published the circular in their respective languages, and have communicated to the secretary their hearty approval of the plan. Prof. Capellini has, in this connection, published an interesting correspondence, calling attention to the fact that in 1874 he had laid the project of a similar International Geological Congress, to be held in Italy, before the Italian Minister of Agriculture, Industry, and Commerce.

"The Geological Society of London and the Geological Survey of Great Britain have also formally signified their approval of our objects, and the co-operation of Norway, Sweden, Russia, and Austro-Hungary, is promised. It is to be regretted that Germany has declined to take a part in the International Exhibition of 1878, but we trust that this will not prevent her geologists from joining in the proposed Congress. The director of the Geological Survey of Japan promises to aid in our work, and we have the same assurance from Brazil, where the circular has been translated into Portuguese. Chili and Mexico have also responded, and promise an ample representation of their geology at Paris next year; while Canada, both through her Geological Survey and in the person of Dr. Dawson, will probably be represented there.

"The Government of the United States has as yet failed to accept the invitation of France to take a part in the Exhibition of 1878, so that American geologists are not certain that they will be able to participate in the International Geological Exhibition of 1878. We are, however, assured that the Government is very desirous to have our country duly represented at Paris; and it is to be hoped that at the approaching extra session of the United States Congress, measures will be taken for accepting the French invitation, and appointing a commission, so that our people may secure a representation in Paris. I am assured, on all sides, that our geologists desire to contribute largely to the International Geological Exhibition, and even at this late day it will be possible to do much. In any event it is probable that several members of our committee will be present at the proposed Geological Congress. The precise date of this has not yet been fixed, though your secretary is now in correspondence with the Secretary of the Geological Society of France upon this point, and believes that with the co-operation of that body a time convenient to all will be agreed upon.

"It is recommended by the Standing Committee of the Association that, in addition to the names of Prof. J. P. Lesley, of Philadelphia, and Prof. A. C. Ramsay, director of the Geological Survey of Great Britain, already added to the International Committee, the presidents for the time being of the Geological Societies of France, London, Edinburgh, and Dublin, of Berlin, of Belgium, Italy, Spain, Portugal, and the Imperial Geological Institute of Vienna, be invited to form part of our Commission. T. STERRY HUNT

"Secretary of the International Committee."

Shortly after the presentation of the above report, the secretary received official notice that the Geological Society of France had, in co-operation with the above plan, appointed at Paris a local committee of organisation for the proposed Congress, constituted as follows:—Hébert, President; Tournouer and Albert Gaudry, Vice-Presidents; Bioche, Treasurer; Jannetaz, Secretary-General; Delaire, Sauvage, Brocchi, and Vélain, Secretaries; with the following: Belgrand Bureau, de Chancourtois, G. Cotteau, Damour, Daubrée, Delafosse, Delesse, Descloizeaux, Desnoyers, Fougué, V. Gervais, Gruner, De Lapparent, Mallard, Milne-Edwards, Pellat, Marquis de Roys and L. Vaillant, Members of the Committee.

A circular issued by this committee bearing date July 31, invites all those interested in geological, mineralogical, and palæontological studies to take part in the approaching congress, and to subscribe the sum of *twelve francs* each, which will give a card of admission to the Congress, and right to all the publications thereof. All those who intend to be present are at the same time invited to send, as soon as possible, a list of the questions which seem to them worthy of general discussion, as well as of the communications which they propose to make touching these questions. They are also invited to indicate the date which appears to them most convenient for the meeting of the Congress.

As regards an International Geological Exhibition, the Paris Committee of Organisation state that the difficulty of finding a suitable locality seems to them an obstacle in the way of realising this part of the programme. They hope, however, that there will be many special collections sent, and beg the exhibitors of such to give the committee due notice of these, in order that a special catalogue of them may be prepared.

The secretary of the International Committee desires, in this connection, to call attention to the fact that his circular did not contemplate the holding of an International Geological Exhibition apart from the universal exhibition, but, in the language of that circular, *the making as complete as possible the geological department of the universal exhibition.* It is certain that, as at all previous similar exhibitions, the different nations will contribute more or less of geological material, and it was conceived that such collections, extended and systematised in accordance with the plan set forth in the circular, would, while forming a part of the universal exhibition, without farther cost meet all the requirements of an International Geological Exhibition. To the accomplishment of this end it will only be necessary for the exhibitors of all nations to send a list of their geological contributions to the Local Committee of Organisation at Paris.

All correspondence relating to the Congress should be

addressed to Dr. Jannetaz, Sécrétaire-général, rue des Grands Augustins, 7, Paris, France; and all moneys sent to Dr. Bioche, at the same address.

THE MODERN TELESCOPE

THE gain to astronomy from the discovery of the telescope has been twofold. We have first, the gain to physical astronomy from the magnification of objects, and secondly, the gain to astronomy of position from the magnification, so to speak, of *space*, which enables minute portions of it to be most accurately quantified.

Looking back, nothing is more curious in the history of astronomy than the rooted objection which Hevel and others showed to apply the telescope to the pointers and pinnules of the instruments used in their day; but doubtless we must look for the explanation of this not only in the accuracy to which observers had attained by the old method, but in the rude nature of the telescope itself in the early times, before the introduction of the micrometer; the modern accuracy has been arrived at step by step.

FIG. 1.—A portion of the constellation Gemini seen with the naked eye.

Let us see what the telescope does for us in the domain of that grand physical astronomy which deals with the number and appearances of the various bodies which people space.

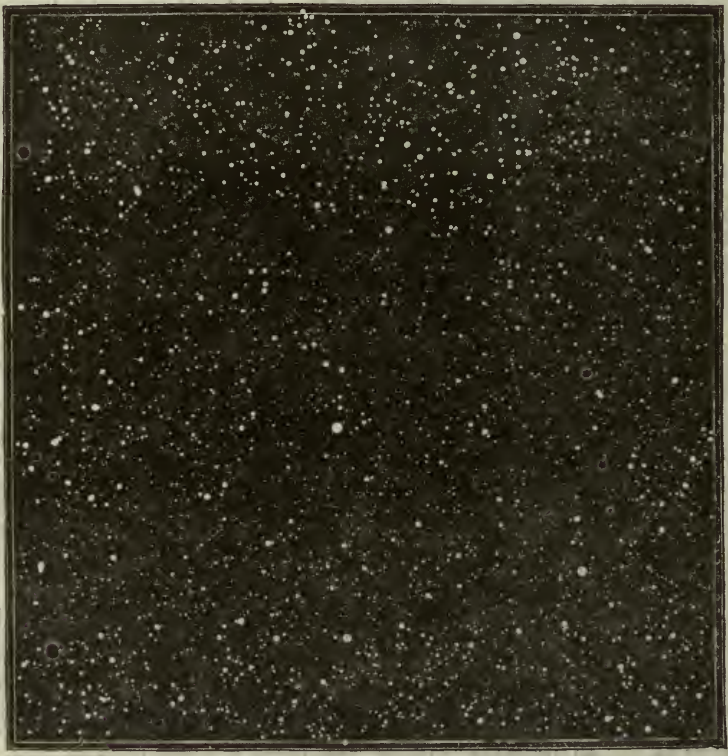


FIG. 2.—The same region, as seen through a large telescope.

Let us, to begin with, try to see how the telescope helps us in the matter of observations of the sun. The sun is about 90,000,000 of miles away; suppose, therefore, by means of a telescope reflecting or refracting, whichever we like, we use an eyepiece which will magnify say 900 times, we obviously bring the sun within 100,000 miles of us; that is to say, by means of this telescope, we can observe the sun with the naked eye as if it were within 100,000 miles of us. One may say, this is something, but not too much; it is only about half as far as the moon is from us. But when we recollect the enormous size of the sun, and that if the centre of the sun occupied the centre of our earth the circumference of the sun would extend considerably beyond the orbit of the moon, then one must acknowledge we have done something to bring the sun within half the distance of the moon. Suppose for looking at the moon we use on a telescope a power of 1,000, that is a power which magnifies 1,000 times, we shall bring the moon within 240 miles of us, and we shall be able to see the moon with a telescope of that magnifying power pretty much as if the moon were situated somewhere in Lancashire—Lancaster being about 240 miles from London.

It might appear at first sight possible in the case of all bodies to magnify the image formed by the object-glass to an unlimited extent by using a sufficiently powerful eyepiece. This, however, is not the case, for as an object is magnified it is spread over a larger portion of the retina than before; the brightness, therefore, becomes diminished as the area increases, and this takes place at a rate equal to the square of the increase in diameter. If, therefore, we require an object to be largely magnified we must produce an image sufficiently bright to bear such magnification; this means that we must use an object-glass or speculum of large diameter. Again, in observing a very faint object, such as a nebula or comet, we cannot, by decreasing the power of the eye-piece, increase the brightness to an unlimited extent, for as the power decreases,

the focal length of the eye-piece also increases, and the eye-piece has to be larger, the emergent pencil is then larger than the pupil of the eye and consequently a portion of the rays of the cone from each point of the object is wasted.

We get an immense gain to physical astronomy by the

FIG. 3.—Orion and the neighbouring constellations.

revelations of the fainter objects which, without the telescope, would have remained invisible to us ; but, as we know, as each large telescope has exceeded preceding ones in illuminating power, the former bounds of the visible creation have been gradually extended, though even now we cannot be said to have got beyond certain small limits, for there are others beyond the region which the most powerful telescope reveals to us ; though we have got only into the surface we have increased the 3,000 or 6,000 stars visible to the naked eye to something like twenty millions. This space-penetrating power of the telescope, as it is called, depends on the principle that whenever the image formed on the retina is less than sufficient to appear of an appreciable size the light is apparently spread out by a purely physiological action until the image, say of a star, appears of an appreciable diameter, and the effect on the retina of such small points of light is simply proportionate to the amount of light received, whether the eye be assisted by the telescope or not ; the stars always, except when sufficiently bright to form diffraction rings, appearing of the same size. It therefore happens that as the apertures of telescopes

FIG. 4.—The Nebula of Orion, reduced from Lord Rosse's Drawing.

increase, and with them the amount of light (the eye-pieces being sufficiently powerful to cause all the light to enter the eye), smaller and smaller stars become visible, while the larger stars appear to get brighter and brighter without increasing in size, the image of the brightest star with the highest power, if we neglect rays and diffraction

rings, being really much smaller than the apparent size due to physiological effects, and of this latter size every star must appear.

The accompanying woodcuts of a region in the constellation of Gemini as seen with the naked eye and with a powerful telescope will give a better idea than mere language can do of the effect of this so-called space-penetrating power.

With nebulæ and comets matters are different, for these, even with small telescopes and low powers, often occupy an appreciable space on the retina. On increasing the aperture we must also increase the power of the eye-piece, in order that the more divergent cones of light from each point of the image shall enter the pupil, and therefore increase the area on the retina, over which the increased amount of light, due to greater aperture, is spread; the brightness, therefore, is not increased, unless indeed we were at the first using an unnecessary high power. On the other hand, if we lengthen the focus of the object-glass and increase its aperture the divergence of the cones of light is not increased and the eye-piece need not be altered, but the image at the focus of the object-glass is increased in size by the increase of focal length, and the image on the retina also increases as in the last case. We may therefore conclude that no comet or nebula of appreciable diameter, as seen through a telescope having an eye-piece of just such a focal length as to admit all the rays to the eye, can be made brighter by any increase of power, although it may easily be made to appear larger.

Very beautiful drawings of the nebula of Orion and of other nebulæ, as seen by Lord Rosse in his 6-foot reflector, and by the American astronomers with their 26-inch refractor, have been given to the world.

The magnificent nebula of Orion is scarcely visible to the naked eye; one can just see it glimmering on a fine night; but when a powerful telescope is used it is by far the most glorious object of its class in the northern hemisphere, and surpassed only by that surrounding the variable star η Argûs in the southern. And although, of course, the beauty and vastness of this stupendous and remote object increase with the increased power of the instrument brought to bear upon it, a large aperture is not needed to render it a most impressive and awe-inspiring object to the beholder. In an ordinary 5-foot achromatic many of its details are to be seen under favourable atmospheric conditions.

Those who are desirous of studying its appearance, as seen in the most powerful telescopes, are referred to the plate in Sir John Herschel's "Results of Astronomical Observations at the Cape of Good Hope," in which all its features are admirably delineated, and the positions of 150 stars which surround θ in the area occupied by the nebula laid down. In Fig. 4 it is represented in great detail, as seen with the included small stars, all of which have been mapped with reference to their positions and brightness. This, then, comes from that power of the telescope which simply makes it a sort of large eye. We may measure the illuminating power of the telescope by a reference to the size of our own eye. If one takes the pupil of an ordinary eye to be something like the fifth of an inch in diameter, which in some cases is an extreme estimate we shall find that its area would be roughly about one-thirtieth part of an inch. If we take Lord Rosse's speculum of six feet in diameter the area will be something like 4,000 inches; and if we multiply the two together we shall find, if we lose no light, we should get 120,000 times more light from Lord Rosse's telescope than we do from our unaided eye, everything supposed perfect.

Let us consider for a moment what this means; let us take a case in point. Suppose that owing to imperfections in reflection and other matters two-thirds of the light is lost so that the eye receives 40,000 times the amount given by the unaided vision, then a sixth magnitude star—a star just visible to the naked eye—would have 40,000 times more light, and it might be removed to a distance 200 times as great as it at present is and still be visible in the field of the telescope just as it at present is to the unaided eye. Can we judge how far off the stars are that are only just visible with Lord Rosse's instrument? Light travels at the rate of 185,000 miles a second, and from the nearest star it takes some $3\frac{1}{2}$ years for light to reach us, and we shall be within bounds when we say that it will take light 300 years to reach us from many a sixth magnitude star.

But we may remove this star 200 times further away and yet see it with the telescope, so that we can probably see stars so far off that light takes 60,000 years to reach us, and when we gaze at the heavens at night we are viewing the stars not as they are at that moment, but as they were years or even hundreds of years ago, and when we call to our assistance the telescope the years become thousands and tens of thousands—expressed in miles these distances become too great for the imagination to grasp; yet we actually look into this vast abyss of space and see the laws of gravitation holding good there, and calculate the orbit of one star about another.

J. NORMAN LOCKYER

(*To be continued.*)

ZOOLOGICAL GARDENS[1]

THE lists and reports of the various zoological gardens now before us show that much progress has lately been made by these as by other institutions connected with natural history. For though zoological gardens are looked upon by many as a simple form of amusement there can be no question that, when rightly conducted, they are not only instructive in the highest degree, but also tend materially to advance the interests of the higher branches of natural science. All persons, therefore, who take an interest in the progress of science will be glad to see the number of zoological gardens increasing among the dependencies of this country and in other States.

Of the first of the five works on our list we need say but little. The Gardens of the Zoological Society of London, in the Regent's Park, are too well known to most of our readers to require a lengthened notice. The chief additions to their unrivalled menagerie are recorded every week in our columns. The volume now before us contains a catalogue of all the species of vertebrated animals, of which examples have been exhibited during the past fifteen years, arranged in systematic order. The various specimens are distinguished by letters, and the date and mode of acquisition of each individual are added. Thirty-five woodcuts, most of which have originally appeared in the Society's *Proceedings*, illustrate some of the more remarkable forms. The result shows that from the commencement of the year 1861 to the close of 1875, there have been obtained for the collection in the Regent's Park, examples of no less than 2,143 species of vertebrated animals. Of these 570 were mammals, 1,224 birds, 227 reptiles, 39 batrachians, and 83 fishes.

The catalogue of the animals in the newly-established Zoological Gardens at Calcutta, concerning the foundation and progress of which we have written at full length not long since,[2] is next upon our list. It is drawn up after

[1] (1) List of Vertebrated Animals now or lately living in the Gardens of the Zoological Society of London. Sixth Edition. 1877. (London: Longmans).
(2) List of Vertebrated Animals living in the Zoological Gardens, Calcutta, April, 1877. Printed at the Bengal Secretarial Press. 1877. 8vo.
(3) A Guide to the People's Park, Madras, with a description of the Zoological Collection contained therein. (Madras: Higginbotham and Co, 1876)
(4) The Fifth Annual Report of the Board of Directors of the Zoological Society of Philadelphia. Read at the Annual Meeting of the Members and Loanholders of the Society, April 26, 1877. 8vo. (Philadelphia, 1877)
(5) Report of the Director of the Central Park Menagerie, Department of Public Parks, City of New York, for year 1876. (New York, 1877: B. M. Lees, Printer, 210, Fulton Street.)

[2] NATURE, vol xvi. p. 28.

the fashion of the preceding, and has been prepared by Dr. John Anderson, the Superintendent of the Imperial Museum at Calcutta. It shows that though so recently in actual operation these gardens have already made considerable progress, and are able to show a good series of the better-known Indian animals for the instruction and amusement of the Calcutta public. Amongst others we may notice the Indian Otter (*Lutra leptonyx*) and the Isabelline Bear, as animals which have not yet reached the Gardens of the Zoological Society of London. Altogether there are 77 species of mammals in the collection, 120 of birds, and 17 of reptiles.

The "Guide to the People's Park" shows that Madras does not intend to be left behind the sister-city of Calcutta, and that she too will have a zoological garden. As its name imports, this little work is more of the nature of a "Guide" than a Catalogue. It appears that Madras is indebted to Sir Charles Trevelyan for the People's Park. Prior to 1859 the plot of ground which it now occupies formed "an immense swamp." In that year the enlightened governor of the day first suggested, and subsequently put into execution, the conversion of it into a park of about 116 English acres. (How glad would be the Council of the Zoological Society of London to have such an area at their disposal!) The collection of animals does not yet, it is true, appear to be very extensive; but space, at any rate, does not fail them, and there is, at all events, plenty of room for additions, which cannot be said of some of the sister institutions.

We must now turn to the western hemisphere, and see what our Anglo-Saxon relatives on the other side of the Atlantic have done in the way of zoological gardens. In this matter, we must say, our usually energetic cousins seem to have moved a little slowly. Such vast and wealthy populations as those of New York and Philadelphia might well have started zoological gardens for the instruction and amusement of their citizens years ago, and they would by this have been in possession of well-organised institutions. But although the subject has been mooted in both these cities for many years, it is only within these last few years, we believe, that anything very practical has been effected.

The Zoological Garden of New York forms a part of the Central Park of that city, and the report now before us is addressed by Mr. W. A. Conklin, the director, to the Board of Commissioners of the Department of Public Parks of New York. It gives us an account of the affairs of the Zoological Garden during the year 1876, and not apparently a very satisfactory one—since a reduction of the sum usually appropriated (by the City of New York, we presume) to the Park was made that year, which rendered it impossible to keep up the Gardens on their usual footing. It was resolved "not to receive any animal for exhibition in the menagerie unless the owner furnished the necessary food." This measure and the diminution of the sum expended in new purchases seem to have caused a sad decrease in the number of animals exhibited in 1876. In spite of this the number of visitors was larger than in any previous year, which, however, is accounted for by the concourse of visitors passing through New York to and from the Centennial Exhibition at Philadelphia.

While the Zoological Garden of New York is kept up out of public moneys that at Philadelphia is, like ours in London, the property of a private society, and appears to be in a much more flourishing condition. Here the "Centennial" told still more largely on the number of visitors than at New York, raising them to a grand total of more than 600,000 for the year ending April 30 last. The extra receipts from this source have not only enabled the society to make many important additions to its menagerie, but also to spend a considerable sum in improvements and new buildings. Amongst the latter we notice "a house for the accommodation of warm-climated (!) hay-eating animals" (qu. zebras and antelopes?) now under construction at an estimated cost of 18,000 dollars, which will apparently exceed in dimensions even the new lion-house of the Zoological Society of London. This is pretty well for a society only now issuing its *fifth* annual report. It is evident that in zoological gardens, as in other scientific institutions, Philadelphia means to "go-ahead" of her more populous neighbour.

NOTES

WE take the following from the *Times*:—The Royal Society medals for the present year have been awarded by the President and the Council as follows:—The Copley Medal to Prof. James Dwight Dana, for his biological, geological, and mineralogical investigations, carried on through half a century, and for the valuable works in which his conclusions and discoveries have been published. A Royal Medal to Mr. Frederick Augustus Abel, F.R.S., for his physico-chemical researches on gun-cotton and explosive agents. A Royal Medal to Prof. Oswald Heer, of Zurich, for his numerous researches and writings on the tertiary plants of Europe, of the North Atlantic, North Asia, and North America, and for his able generalisations respecting their affinities and their geological and climatic relations; and the Davy Medal to Robert Wilhelm Bunsen and Gustav Robert Kirchhoff, for their researches and discoveries in spectrum analysis. This is the first award of the Davy medal, which, as will be remembered, was founded by the proceeds of the sale of the service of silver plate bequeathed for the purpose by Sir Humphry Davy. The medals will be presented at the Society's anniversary meeting on the 30th inst.

A FEW days ago the French Minister of Public Instruction, by a decree which has not yet been published, appointed a Commission to deliberate with the members of the council of the Observatory of Paris, as to the improvements which are possible in the organisation of the establishment without interfering with existing decrees. Among the commissioners are Dr. Janssen, Director of the Meudon Physical Observatory, M. Hervé Mangon, President of the Meteorological Society of France, and M. Marie Davy, the Director of the Montsouris Observatory. M. Yvon Villarceau and M. Loewy have been appointed as councillors. The first meeting of the Commission took place last Saturday, under the presidency of M. Dumesnil, one of the heads of the ministry, representing M. Brunet. M. Yvon Villarceau, the astronomer delegate, read a long and elaborate report on the improvements which it was considered desirable to make in the establishment. The Commission came to no decision, and the meeting adjourned to Saturday, Dec. 1. Some of the members are desirous of separating the meteorological department from the observatory, and either transfer it to Montsouris or establish a Meteorological Institute; to accomplish this long desired change it would be necessary to suppress the decrees signed by M. Thiers and approved by M. Leverrier. The intentions of the Government are not to alter radically the existing state of things, which works satisfactorily, but to improve it as far as possible. Public opinion is strongly in favour of the organisation consecrated by M. Leverrier's administration.

TWO volumes of the French Transit of Venus Reports are now going through the press, and will be distributed in a very few days. The first is a *compte rendu* of the mission in China, commanded by Capt. Fleurian. The second is a *procès verbal* of the sittings of the Transit Commission, which was presided over by M. Dumas. It is known that M. Leverrier abstained from being present at its deliberations, the illustrious astronomer being one of the few opponents of the transit observation. He preferred the opposition of Mars or direct measurements as taken by Cornu in his experiments on the velocity of light.

THE French Government intends to send out an expedition to San Francisco in order to observe the next transit of Mercury, which will take place on May 6, 1878.

At the meeting of the Paris Academy of Sciences, on November 12, M. Faye presented the volume of the "Connaissance des Temps" for 1879. This publication has reached, according to M. Faye, the highest degree of perfection desirable, and the new volume is marked by two important improvements both due to M. Loewy. The first consists in a new method which enables longitudes to be calculated according to occultations of stars by the moon, and that with such facility that sailors will make use of them with great benefit. The second improvement consists in tables which enables the latitude to be obtained by observation of the polar.

The death of von Baer has made a foreign associateship in the Paris Academy of Sciences vacant, and MM. Bertrand, Fizeau, Becquerel père, Claude Bernard, Dumas, and H. St. Claire Deville, have been appointed a commission to prepare a list of candidates for the vacant "fauteuil."

A prize of 1,000 marks (50*l.*) is offered through Dr. Hermann J. Klein, of Cologne, for the best treatise on "The Development of Monistic Philosophy from Spinoza down to the Present Time." The treatise must be written in the German language, and must contain a complete account of the relation of Spinoza to the Cartesian philosophy, a description of the progress and changes in the monistic theory brought about by Leibniz, Schopenhauer, Lazarus Geiger, and Ludwig Noiré, and a clear definition of the differences between the materialistic and monistic theories. All details can be obtained from Dr. Klein. The term up to which treatises will be received is fixed for July 30, 1878.

By a recent will, M. Maujean has bequeathed to the French Institute the capital producing a sum of 1,200 francs, designed to form a biennial prize of 2,000 francs, to be awarded alternately by the Académie Française, and by the Académie des Sciences. To obtain it of the latter, it is necessary to have published the work which shall be pronounced the most useful to hygiene, considered in all its branches.

The Berlin Aquarium suffered, on November 13, the loss of what was certainly, from a scientific and from a financial standpoint, the most valuable zoological specimen in Europe, viz., the famous gorilla Pongo, whose human-like form and playful antics became so familiar to Londoners during the past summer. The visit to England, and stay in its warm moist climate, was regarded as having had the best effect on Pongo's health, when he returned to Berlin on September 21, and there was every prospect of the animal's being able to live through his second northern winter. Five weeks later, a lessening of appetite and slight diarrhœa were observed, but were not regarded by the physician as of sufficient importance to prevent Pongo's appearance in public. The consternation was great when a few days later, the gorilla died suddenly, without any apparent increase of dangerous symptoms. The loss to the Berlin Aquarium is no small one, as it had lately refused an offer of 2,500*l.* for the animal, and, taken in connection with the late deaths of their orang-outang and chimpanzee, will check somewhat the tendency to invest capital in anthropoidal apes. Not less severe is the loss to the scientific public, for no animal of late years has so attracted the attention of zoologists as Pongo, and theorists were looking forward with no slight degree of interest to the possibilities connected with his growth and education. After a dissection, which will probably reveal the cause of the sudden death, the skin will be handed over to the Berlin Anatomical Museum.

We have received from Dr. Aguilar the annual volume of the Observatory of Madrid for the last year, 1876. It is a little late in the day, but we may call attention to the long and interesting article on geographical discovery with which the book terminates, seeing that that commences so early, "2400 (?) años A. des J. C. Dispersion de las gentes despues del Deluvio. Del cáos consiguiente á tan immensa catástrofe surgen á poco tiempo los tres grandes reinos de Babilonia, Ninive y Egipto."

Already studied by two geologists, the Crimean peninsula has been recently visited by M. Ernest Favre, of Geneva. M. Hebert presented to the Paris Academy of Sciences, on Nov. 12, the results of this new examination, consisting of numerous sections on a very complete map.

Hachette and Co. are about to publish an important work of reference in Chemistry containing such important matter as the coefficients of dilatation, the specific weight of vapours, refrigerating mixtures, numerical documents on qualitative, quantitative, and spectral analysis, &c. We may state that the Smithsonian Institution are about to publish a similar work.

There are now "on view" at the Westminster Aquarium four Laplanders—two men and two women—who have with them reindeer, dogs, an Arctic fox, a tent, sledges, and numerous articles of dress of home manufacture. They have been brought to England by Mr. Carl Bock, through the enterprise of Mr. Farini, so well known as the "inventor" of Lulu's "upward bound," Zazel's "lightning flight," and Maraz's "eagle swoop." Any entertainment announced by one whose greatest successes hitherto have been to puzzle the public as to "how it is done" will naturally be looked upon with the same kind of suspicion that was bestowed on the "Egyptians" in the recent Lord Mayor's show. In some cases the public enjoys being puzzled, and this adds a zest to the enterprises of those who devise how to puzzle them. In the case of these Laplanders there does not appear to be the slightest ground for any suspicion as to genuineness. It will be recollected that Mr. Farini's whale at the aquarium was genuine, and when the *post-mortem* was held under the direction of Prof. Flower it was shown beyond doubt that it was not made of vulcanite and kept going by clock-work as was popularly supposed. We draw attention to the visit of these Laps because there is much of interest to be learnt from seeing them, and we do so with all the greater pleasure because the aquarium, looked at from a scientific point of view, has fallen from its high estate. We cannot pretend to make it a complaint that it is in the evening practically a large music hall with a miscellaneous entertainment by comic performers and sword swallowers. The place cannot be kept open without money, and if the public will not pay to go to an aquarium pure and simple, the management must provide what the public will take to, or shut up the place. But what we fear is that the management has been too much neglecting that part of the public, the minority certainly, who do care for an aquarium. Occasionally, especially during the control of Mr. Carrington, the aquarium has been in good order and well-stocked. It is again getting very unsatisfactory, perhaps because Mr. Carrington is in Naples. We gladly mentioned such recent improvements as throwing several tanks into one to make a place for large fish, and the removal of the seals to the whale tank, where their gambols in swimming can be better seen, and we have on several occasions recorded interesting arrivals, and if we could honestly do so we would gladly recommend the tanks generally as affording a good opportunity for studying the habits of the occupants. Though the Laps are not especially connected with aquarium objects the building affords a centrally located home for them. The performance, if it may be so called, through which they go, is an illustration of their quiet life, and happily there is no attempt to make it sensational. They show, among other things, how reindeer sinew is worked into a continuous thread, a process of interest to those who have examined collections from bone caves containing implements which it is believed were used either with such threads or strips of reindeer hide. The size of some of the eyes of the bone needles is more suggestive of thread than strips. Their monotonous singing on the syllables *wa wa wa*, if not

beautiful, has an interest of its own as representing their secular music, especially when contrasted with their capability for singing Lutheran hymns. Schaferius gives the translation of some of their love songs. Have these died out since his time? Mr. Bock says they have no secular songs. We are glad to know that the Zoological Society has given a friendly hand to Mr. Farini in offering a temporary home to five of his reindeer in the gardens. Mr. Bock states that the place from which he brought the party is Kautokeino, N. 69·1, E. 22·56.

A REPORT has recently been presented to the State Board of Health in Massachusetts by Dr. Nichols, regarding the health of people who work with sewing machines. From observations by the medical men engaged it is inferred that a healthy person of average strength who does not make a business of sewing with the machine, may work from three to four hours daily without much fatigue or perceptible injury to health. Among work people, on the other hand, one frequently meets with disorders of digestion, due to sedentary life and bad ventilation, also pains in the muscles of the trunk and the lower limbs, because these latter are always in motion. There occur also congestions of the ventral organs, weakness, and in some rare cases neuralgias of the legs and spinal irritations. It is recommended to the proprietors of works in which the sewing machine is used, to have (1) a good ventilation; (2) a shorter time for work, with periods of rest; (3) another motor force than that of the feet, *e.g.*, a steam engine.

AN Indo-Chinese Society has just been formed in Paris for promoting the study of Transgangetic India and developing the trade of France in that region.

THE Juvenile Christmas Lecture at the Society of Arts will be by Prof. Barff, on "Coal and its Components."

THE *Moniteur Universel* publishes an article on the manufacture of types for printing with hardened glass (*verre trempé*). It appears that the new types have worked admirably on the improved revolving press for continuous paper.

THE death is announced of Mdlle. Henrietta Cerf, who was born in Jamaica in 1810, and died in Brussels on the 22nd ult. Mdlle. Cerf, who for some years resided near Dinant, communicated various articles on the botany of Kent and Belgium to the *Phytologist*.

PRINCE BISMARCK'S study at Varzin has been connected with the Foreign Office at Berlin by a telephonic apparatus. The demand for these instruments is said to be immense in Germany.

A MONK of the monastery of Raigern, between Braun and Vienna, has completed a very curious mechanical work, a self-moving terrestrial globe, fourteen metres in diameter. A combination of wheels effects a revolution similar to that of the earth, and which lasts for three weeks. At the axis of the North Pole there are dials which indicate the days, months, &c.; above this axis is another smaller globe which shows the rotation of the earth around the sun. The large globe is set in motion by a dozen wheels. This ingenious mechanism has cost ten years' labour, and has only been achieved after many experiments. A map drawn upon the globe shows geographical details, and includes the most recent discoveries, routes of steamers, railways, telegraphs, mountain-heights, depths of the sea, &c.

WE have received a reduced photo-electrotype facsimile, by Mr. G. E. Emery, of Lynn, Mass., of the map which accompanied the narrative of the brothers Zeni, published at Venice in 1558. The Zeni it will be remembered made a voyage to the Arctic regions in the fourteenth century, and one of the problems of geography is to identify the places mentioned in their narrative and map. This has already been ably attempted by Mr. Major, and while Mr. Lynn's identifications agree in the main with those of Mr. Major, there are some important differences. "Icaria," *e.g.*, which Mr. Major makes out to be Kerry, Ireland, Mr. Lynn identifies with the Rockall Islands. The lost East Greenland Colony, the latter places on the east of Spitzbergen, apparently on Wiche Land, and most extraordinary of all, Crolandia, he maintains is the recently-discovered Franz-Josef Land. These two last identifications are very daring, and geographers will look with interest for Mr. Emery's reasons, which no doubt he will publish.

INTELLIGENCE has reached the Royal Italian Geographical Society that the Marquis Antinori, heading the Italian expedition of discovery in Africa, is dead. Chiarini, his fellow-traveller, is a prisoner in Abyssinia.

A SECOND edition of Capt. Luigi Gatta's Italian translation of Maury's "Physical Geography of the Sea" has just been published at Rome. It contains extensive and valuable footnotes by the translator. Capt. Gatta is, we understand, engaged in a translation of Lyell's "Principles of Geology."

DR. HARMAND, who has been exploring in Cochin China, has arrived in France, bringing with him, we believe, results of much value.

ON October 18, the first pioneers of the International African Exploration Society, consisting of the two Belgian officers, Capts. Crespel and Cambier, and the naturalist, Dr. Maes, left Southampton for Lake Tanganyika *viâ* Port Natal, on one of the vessels of the Union Mail Steamship Company. This Company, with praiseworthy generosity, conveys the first party entirely free, and will make a deduction of twenty per cent. in the fares of all subsequently sent out by the society. The royal auspices under which the society enters upon its field of activity have ensured to it support in a variety of directions. The Sultan of Zanzibar has promised to render the utmost assistance possible, and the commercial house of Roux de Fraissinet and Co., has instructed its widely-spread agencies on the east-coast to second the efforts of the exploring party. There seems to be no lack of funds in the treasury of the society. Among the late subscriptions are 3,000 francs from the Hungarian African Society, while the collections in France amount already to 32,000 francs. Belgium, small as it is, contributed 300,000 francs outright in June last, while yearly subscriptions to the amount of 100,000 were given in addition. There is every prospect that this magnificent united effort will succeed in solving some, at least, of the problems connected with the remaining *terra incognita* of equatorial Africa.

WE regret to record the untimely end of the well-known geologist and African explorer, Dr. Erwin von Bary, whose recent explorations have frequently been referred to in our columns. Dr. v. Bary started in August, 1876, from Tripolis, on his journey into the interior of the Sahara, supported partly by the Karl Ritter Endowment Fund, and partly by the Berlin Afrikanische Gesellschaft. The aim of this expedition was to make a thorough study of these almost unknown regions, with especial reference to topographical and geological questions, more particularly the age and formation of the great desert. The chief results of this first journey were the observations leading to the conclusion that the Sahara was not formerly the bed of an inland sea as hitherto supposed. The traveller returned from this very exhaustive and fatiguing tour to the Berber-town of Chat to recruit his impaired energies, and prepare for a more extended trip into the district of the Tuarej Hoggar, which has not as yet been visited by Europeans. Here he met the sad fate of so many African explorers, and died on October 2, from the effects of excessive exposure and privation. Von Bary's varied qualifications and complete devotion to the cause for which he perished, had led to high expectations among his fellow German geologists,

and a general feeling of regret is felt over his early death, away from home and friends. The French geologist, M. Largeau, is at present endeavouring to penetrate into the Tuarej region from the north, and the interest previously centred on von Bary's investigations will now gather about his efforts.

IN the spring of the present year we referred briefly to the attempt being made by Dr. J. M. Hildebrandt, under the auspices of the Berlin Academy of Sciences, to ascend the snow-covered summit of Mount Kenia. The question as to the permanent snow covering of the two equatorial mountains, Kenia and Kilimandscharo, has been a subject of so much controversy among geographers, that the results of this expedition have been looked for with great interest. It is with regret that we learn from a communication of Dr. Hildebrandt's, dated Suez, November 2, that he has been compelled to return, leaving the summit of Kenia still untrodden by the foot of a European. He left Mombassa on January 10 with forty attendants, and after two months of exhaustive travel amidst hostile tribes, reached Kitui, in Ukamba. Here, in full sight of Kenia, he was compelled to pause and retrace his footsteps, his followers utterly refusing to venture among the marauding tribes intervening between him and his journey's goal, and he himself being only saved by the swift application of an antidote from death by poison given by the natives. On reaching Zanzibar the physicians declared his health impaired to such an extent that restoration could only be hoped for in a more temperate clime. Dr. Hildebrandt has suffered unusually from the two invariable concomitants of the African explorer—sickness and the hostility of the aborigines, his two expeditions from Zanzibar in the spring and autumn of 1875 being both shortened and hampered by these causes.

HERR SCHÜTT, a civil engineer, has been despatched by the German African Society to St. Paul de Loanda to undertake an expedition through the region lately traversed so successfully by the hunter, Dr. Pogge.

ONE of the effects of the war in the east appears to be the discovery in out-of-the-way towns in Russia, of gems of unsurpassed size and beauty, which doubtless have been jealously hoarded by their possessors, and only brought to light in times, like the present, of national necessity. Some of these gems have naturally found their way to this country; perhaps the most remarkable are—an aquamarine, far superior to anything before seen in England, weighing over six ounces and a half, without the slightest blemish, and of a deep sea-green tint; also a topaz rivalling that purchased for the Grand Mogul at Goa for 11,260*l*. These two remarkable gems were received from Moscow by Mr. Bryce M. Wright, Mineralogist, of Great Russell Street, the possessor of the unique suite of diamonds called the "Bryce Wright Diamonds," valued at 21,000*l*.

WE are requested to state that in the abstract of Mr. Perkin's paper read at the meeting of the Chemical Society on November 1 the word "cumenyl" was, by a slip, written "cinnenyl" throughout the report.

THE additions to the Zoological Society's Gardens during the past week include a Common Squirrel (*Sciurus vulgaris*), European, presented by Mr. T. Massey, F.Z.S.; a Greater Sulphur-Crested Cockatoo (*Cacatua galerita*), from Australia, presented by Mr. F. Lablache; a Radiated Tortoise (*Testudo radiata*) from Madagascar, presented by Mr. H. Harrison; two Red-backed Squirrel Monkeys (*Saimaris œrstedi*), two Black-handed Spider Monkeys (*Ateles melanochir*), a Derbian Opossum (*Didelphys derbianus*) from Central America, a Bonnet Monkey (*Macacus radiatus*) from India, a Rufous-vented Guan (*Penelope cristata*) from Costa Rica, deposited; a Bay Antelope (*Cephalophus dorsalis*) from West Africa, received in exchange.

THE LIBERTY OF SCIENCE IN THE MODERN STATE[1]

WHEN the honourable request was addressed to me by our committee to deliver a lecture to the meeting upon this occasion, I asked myself whether I should not treat of a special department of the latest development of science, in accordance with that point of view to which I drew attention originally, and of which you were reminded by Prof. Klebs only the other day. But I decided this time to give expression to a more general want, principally because it seems to me that the time has come when a certain explanation must take place between science as we represent it and work in it, and general life as a whole, and because in the special history of the continental nations of Europe the moment is rapidly approaching when the mental fate of nations by decisions in the highest quarters may be determined perhaps for a long time to come.

It is not for the first time, gentlemen, that upon the occasion of a meeting of this Association I have been able, as a warning, to point out almost dramatic events happening in our neighbouring state. On a former occasion I could draw attention to occurrences which had just taken place beyond the Rhine, and which, however far they may apparently be removed from our task, yet concern the same contested domain after all, that namely upon which a decision must be made with regard to determining what position modern science is to occupy in the modern state. Let us be sincere—here we may perhaps be doubly so,—it is the question of ultramontanism and of orthodoxy, which moves us continually. I may say that I look forward with real fear to the events which will happen among our neighbours in the course of the next years. We here, at this moment, may look round with a certain pride and we may observe the course of things with a certain calmness. But to-day, when we are celebrating the fiftieth anniversary of this Association, it is certainly becoming to remember how great a change has taken place in Germany, and specially at Munich, since the days when Oken assembled German naturalists and physicians for the first time.

I would only refer shortly to two facts; they are well-known enough, but then they are also important enough to be mentioned again. The one is that when, in the year 1822, the handful of men who constituted the first meeting of the German Association of Naturalists met at Leipzig they thought it still so dangerous to hold a meeting of that description that it was really held in perfect secrecy. The names of the Austrian members could indeed be published only thirty-nine years later, viz., in 1861. The second fact which strikes us when we remember Oken is, that he, the valued and renowned teacher, the ornament of the Munich high school, died in exile in the same canton of Switzerland in which Ulrich von Hutten ended his life full of troubles and contests. Gentlemen, the bitter exile which oppressed the last years of Oken's life, which caused his death far away from those scenes where he had sacrificed the best powers of his life, this exile will remain the signature of the time which we have gone through. And as long as there is a German Association of Naturalists, we shall thankfully remember that this man bore all the signs of a martyr until the time of his death, we shall point him out as one of those who with their blood conquered and obtained for us the liberty of science.

Nowadays, gentlemen, it is easy to speak of the liberty of science in Germany; now we are perfectly secure even here, where, only a few decades back, the fear was great that a new change of things might perhaps produce the extreme reverse, and we can in all calmness discuss the highest and most difficult problems of life and the hereafter. The addresses which were delivered at the first and second general meetings certainly prove sufficiently that Munich is now a place which can bear to hear the representatives of science in the most perfect liberty. I was not able to listen to all these addresses, but I have since read those of Professors Haeckel and Nägeli, and I must say we cannot ask more than to be allowed to continue to discuss with such liberty.

If it were only a question of rejoicing over this possession I should indeed not have claimed your attention for that object. But, gentlemen, we have arrived at a point when it becomes necessary to investigate whether we may hope to retain securely for the future the possession which we actually enjoy. The fact that we are enabled to discuss, as we do to-day, is not a sufficient

[1] Address delivered at the Munich meeting of the German Association, by Prof. Rudolf Virchow, of Berlin.

security that it will always remain so for one who, like myself, has had many years' experience of public life. Therefore I think that our efforts should not only tend to claim the attention of all for the moment, but I believe we ought also to ask ourselves what we are to do to maintain the present state of things. I will tell you at once, gentlemen, what I would represent to you as the chief result of my observations, what I would like to prove here principally. I would like to show that for the present we have nothing more to ask, but that on the contrary we have arrived at the point when we must make it our special task to render it possible, *through our moderation, through a certain resignation with regard to personal opinions and predilections* that the favourable disposition of the nation towards us, which we now enjoy, does not change to the contrary!

In my opinion we are really in danger of doing harm to the future, by making use too amply of the liberty which the present state of things offers us, and I would warn you not to continue in the arbitrariness of personal speculation, which now claims prominence in many domains of natural science. The explanations which my predecessors have given you, those of Prof. Nägeli in particular, will yield a series of the most important points of view, with regard to the course and limits of natural knowledge, to all who read them, and it cannot be my task to repeat them. But I must point out in reference to them, and I would like to adduce a few practical instances from the experience of natural science, how great a difference there is between what we give out as real science in the strictest sense of the word, and for which alone we may in my opinion claim the totality of all those liberties which we may designate as liberty of science, or, if we express ourselves still more exactly, as *liberty of scientific teaching*,—and that larger domain, which belongs more to speculative expansion, which sets problems, and finds the tasks to which modern investigation is to be applied, and which anticipatively formulates a series of doctrines, which are still to be proved, and the truth of which must yet be found, but which in the mean time may be taught with a certain amount of probability, in order to fill certain gaps in knowledge. We must not forget that there is a limit between the speculative domain of natural science and that which is actually proved and perfectly determined. The demand is addressed to us that this limit shall be not only occasionally pointed out, but fixed with the greatest exactness, so that each single worker shall at all times be perfectly conscious of where the limit is drawn, and how far he may be requested to admit that what is taught is actual truth. That, gentlemen, is the problem which we have to work out *in ourselves*.

The practical questions which are connected with this, lie very near. It is evident that for whatever we consider to be secured scientific truth, we must demand the complete admission, into the scientific treasure of the nation. *This the nation must admit as part of itself*—it must consume and digest it, and continue to work at it. Just in this lies the double promotion which natural science offers to the nation :—On the one hand the material progress, that enormous progress which has been made in modern times. Everything which the steam engine, telegraphy, photography, chemical discoveries, the research into colours, &c., have produced, all this is essentially based on this —that we, the men of science, complete the doctrines entirely, and when they are perfectly complete and secure, so that we know with certainty that they are natural scientific truths, that we then give them to the nation at large ; then others can work with them as well, and can create new things, of which formerly nobody had any idea, of which nobody dreamt, which come into the world as perfect novelties, and which reform the condition of society and of states. This is the material significance of our labours. The mental importance, on the other hand, is similar. If I present the nation with a certain scientific truth which is completely proved, to which not the least doubt attaches, if I demand that everybody shall convince himself of the correctness of this truth, that he shall assimilate it, that it shall become part of his thought, then I suppose as a matter of course, that his conception of things generally must be affected by it. Each essentially new truth of this kind must necessarily influence the whole method of conception of man, the *method of thinking*.

If, for instance, to refer to a case in point which lies near, we consider the progress which has been made during recent years with regard to the knowledge of the human eye, beginning at the time when the single component parts of the eye were first anatomically separated, when these single and anatomically separated parts were first examined microscopically and their different arrangement shown, down to the time when we gradually learned to know the vital qualities and the physiological functions of the different parts, until at last, by the discovery of the retina-purple (*Sehpurpur*) and of its photographic properties, a progress was made of which but a year ago we hardly had an idea, then it is evident that with each progressive step of this kind a certain part of optics, particularly the doctrine of vision, is determined and changed. By this we learn in a perfectly certain manner how the action of light takes place in the interior of the human body itself, and that it is quite an outside organ of the human body, not the brain, but the eye which experiences this action. We learn by it that this photographic process is not indeed a mental operation, but a chemical phenomenon, which occurs by the help of certain vital processes, and that in reality we do not see the external things, but their images in our eye. We are thus enabled to gain a new analytical fact for the knowledge of our relations to the world outside of us, and to separate more distinctly the purely mental part of vision from the purely material part. Thus a certain part of optics, and through it one of psychology, is entirely reformed. Chemistry now steps in to investigate questions which up to the present were entirely out of its range, particularly the highly important questions, What is retina-purple? What substance is this? How is it formed, how decomposed, and how again formed? The solution of these questions will not fail to open an entirely new field for investigation ; let us hope that also on the field of technical photography we shall soon make some progress, that we shall learn how to produce many-coloured photographs. Thus a mixture of steps of progress is formed, which belong partly to the material and partly to the mental domain. And I therefore say, that with each true step of progress in natural knowledge a series of changes must necessarily take place in the internal relations of the human race as well as in the external ones, and nobody can prevent new knowledge from influencing him in a certain sense. Each new part of real knowledge works on in man, it produces new conceptions, new trains of thought, and nobody can avoid, after all, placing even the highest problems of the mind into a certain relation with natural phenomena.

But there is still another side of practical consideration which lies far nearer to us. Everywhere in the entire German Fatherland we are now occupied in remodelling educational affairs, in enlarging and developing them, and in determining their precise forms. The new Prussian educational law is on the threshhold of coming events. In all German states larger school-houses are being erected, new institutions are founded, the universities are enlarged, high schools and middle schools are established. At last the question arises, What is to be the principal tenor of what is taught? Where shall the school lead to? In what directions shall it work? If natural science demands, if we have been exerting ourselves for years to obtain an influence in our schools, if we demand that natural knowledge shall be admitted into education in a much larger measure, so that this fertile material be offered early to the youthful minds, in order to form the basis of a new conception, then we must indeed own that it is high time that we understood one another with regard to what we can and will demand. If Prof. Haeckel says that it is a question for pedagogues whether the theory of descent is now to form the basis of instruction, whether the plastidule soul is to be adopted as the basis of all considerations regarding mental phenomena, and whether the phylogeny of man is to be followed up into the lowest classes of the organic empire, and even beyond it up to spontaneous generation, then this is, in my opinion, a mere shifting of tasks. If the theory of descent is as certain as Prof. Haeckel thinks it is, then we must demand its admission into the school, and this demand is a necessary one. How could we imagine that a doctrine of such importance, which influences the conscience of everybody in so revolutionary a manner, which creates directly a sort of new religion, should not be entirely incorporated into the educational plan! How would it be possible to ignore such a revelation—as I may indeed call it—in our schools, and to kill it by silence as it were, or to leave the transmission of the greatest and most important steps of progress, which our conceptions have made in the whole century, to the option of the pedagogue? Indeed, gentlemen, that would be a resignation of the most severe kind, and in reality it would never be exercised. Every schoolmaster who might receive this doctrine in his mind would teach it as well, even unconsciously. How could he do otherwise? He would have to simulate altogether, he would have to rob himself at times of his own knowledge in the most artificial

manner, in order not to show that he knew and recognised the theory of descent, and that he knew exactly how man has originated and whence he comes. If indeed he did not know where man goes to, yet he would at least believe that he knew for certain how in the course of æons the progressive series shaped itself. Therefore I say that if we really did not demand the admission of the theory of descent into the educational plan, this would yet be accomplished of its own accord.

We certainly should not forget, gentlemen, that what here we express, perhaps still with a certain timid reserve, is propagated by those outside with a confidence increased a thousand-fold. For instance, I have once pronounced the phrase—in opposition to the doctrine then reigning of the development of organic life from inorganic matter—that each cell had its origin in another cell, indeed at that time with special reference to pathology, and principally with regard to man himself. I may remark here that in both relations I still to-day consider this phrase a perfectly correct one. But when I had pronounced this doctrine and had formulated the origin of the cell from the cell, others were not wanting who extended this phrase not only in the organic world far beyond the limits for which I had intended it, but who put it down as generally valid even beyond the limits of organic life. I have received the most wonderful communications both from America and Europe, in which the whole of astronomy and geology were based upon the cellular theory, because it was thought impossible that something which was decisive for the life of organic nature upon this earth should not be equally applied to the heavenly bodies, which were said to be round bodies after all, and which had shaped themselves into globes and represented so many cells flying about in universal space and playing a part there similar to that of the cells in our body.

I cannot say that the authors of these communications were all decided fools and simpletons; on the contrary, from some of their explanations I gained the idea that many an otherwise educated man, who had studied much and finally attacked the problems of astronomy, could not understand that the utility of heavenly phenomena should be based upon something else than the utility of human organisation, so that he, in order to gain a monistic conception eventually arrived at the supposition that the heaven must also be an organism, that indeed the whole world must be an organism of useful arrangement, and that no other principle but that of the cells could apply to it. I cite this only in order to show what shape things take outside, how "theories" are enlarged, and how our own doctrines may return to us in a form fearful to ourselves. Now only imagine how the theory of descent may be shaped to-day in the head of a socialist!

Indeed, gentlemen, this may seem ridiculous to many, but it is very serious, and I only hope that the theory of descent may not bring all those horrors in our country which similar theories have actually brought to our neighbours. Anyhow this theory, if carried through to its consequences, has an extremely dangerous side and that the socialists have a certain notion of it already, you will doubtless have remarked. We must make this quite clear to ourselves.

Nevertheless the matter might be as dangerous as possible, the confederates might be as bad as they could be, and yet I say, from the moment when we are convinced that the theory of descent is a doctrine perfectly proved, so certain that we could swear by it, that we could say, thus it is,—from that moment we must not hesitate to introduce it into general life, transmit it not only to every educated person, but teach it to every child, make it the basis of our whole conception of the universe, of society, and of the state, and found our educational system upon it. This I consider a necessity.

In saying this I am not at all afraid of the reproach, which to my astonishment has made a great noise in my Prussian Fatherland, while I was absent in Russia, I mean the reproach of *half-knowledge*. Strange to say, it was one of our so-called liberal journals which asked the question whether the great faults of our time, and socialism in particular, were not based upon the diffusion of half-knowledge. With reference to this I would like to state here, in the midst of the Naturalists' meeting, that *all* human knowledge is only piece-work. All of us who call ourselves naturalists, only possess pieces of natural science; none of us is able to come here and represent each science with the same right, or participate in the discussions of any scientific section. On the contrary, it is just because they have developed themselves in a certain one-sided direction, that we esteem the special scientific men so highly. On the other fields we are all in half-knowledge as it were. Oh! that we could only succeed in diffusing this half-knowledge more and more, if we could succeed in causing at least the majority of educated persons to progress far enough to be able to survey the principal directions which the single departments of natural science are taking, and to follow their development without meeting difficulties too great to be overcome, so that they would at least be aware of the general progress of science, if, indeed, they were not acquainted, at every moment, with the totality of all single and special proofs. We do not get much further ourselves. I, for instance, have honestly tried during my time of life to obtain chemical knowledge; I have even worked in a laboratory, but I feel thoroughly incompetent to sit down at some chemical meeting without preparation, and to discuss modern chemistry in all directions. Nevertheless I am able to penetrate, after a time, so far into any chemical novelty that it does not strike me as incomprehensible. But I must always first acquire this understanding, I have not got it to start with; and when I want it again I must acquire it again. That which honours me is the *knowledge of my ignorance*. The most important part is that I know perfectly well what I do *not* know of chemistry. If I did not know that then of course I should always be wavering to and fro. But as I imagine that I am tolerably well aware what I do not know, I say to myself every time I am obliged to enter a domain which is still closed to me: "Now I must begin again to learn, now I must study afresh, now I must do as anybody does who enters the domain of science." The great error, which is equally shared by many educated people, consists in not remembering that with the enormous extent of natural science and with the inexhaustible quantity of detailed material, it is impossible for any single person alive to command the totality of all these details. That we get far enough to know the *foundations* of natural science and the gaps which exist in our own knowledge, so that every time we find a gap of this kind we say to ourselves,—"Now you enter a domain which is unknown to you,"—that is what we must arrive at. If everybody was only sufficiently aware of this, many a one would beat his breast and own that it is a dangerous thing to draw general conclusions with regard to the history of all things when one is not even entirely master of the material from which these conclusions are to be drawn.

(To be continued.)

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—This term has witnessed the election of two new Natural Science fellows. Mr. A. M. Marshall, Senior in the Tripos of 1874, has been elected at his own College, St. John's. His able papers on Embryology have been an important addition to the researches which are making the British school again famous in this subject, and he is the first Doctor of Science in Comparative Anatomy in the University of London. Two of the newly-elected fellows of St. John's are taking to Medicine, viz., Dr. Marshall and Mr. McAlister, the last Senior Wrangler. At Trinity the open fellowship has been adjudged for the first time to a non-member of the College, Mr. J. N. Langley, B.A., of St. John's, whose services as Demonstrator of Physiology to Dr. Foster are most highly appreciated, while his originality and perseverance in research will, before long, be much more widely known than at present. I understand that the aid of Prof. Huxley was called in, giving the highest guarantee to the examination in Biology, and that several candidates showed themselves in every way worthy of a fellowship, especially in the original memoirs which were sent in before the examination.

The new buildings for anatomy and physiology are advancing to completion and are partially occupied, Mr. Balfour's two practical courses of Comparative Anatomy being accommodated in them. Dr. Foster will transfer much of his work here after Christmas. The new buildings will be almost too small as soon as completed, for Dr. Foster has fifty men and several ladies working in his elementary classes this term, a very large number when it is considered that this is voluntary and not prescribed work. It is but a just tribute to Dr. Foster's rare value as a teacher who makes his students think, who sacrifices his time most indefatigably for their interests, and who cultivates the powers of investigation developing in his pupils with all the care of a parent. Instead of engrossing authority to himself, he sets his senior pupils to lecture on the subjects they make a special study; thus during the present winter the advanced class will receive lectures from Dr. Gaskell, Mr. Langley, and Mr. Lea. Mr. Vines has returned from working in Germany

with Sachs, and is lecturing to a large class on Vegetable Physiology. Next year he will start the first practical course of botany, and, being unable to induce his college to provide apparatus for a laboratory, intends to furnish it at his own expense. Among other lectures in natural science Prof. Dewar's on Physical Chemistry are taking high rank. It is to be noted that Mr. Apjohn, the late lamented Prælector of Chemistry at Caius College, was to have received a fellowship this term by special vote of the whole of the fellows. The prælectorship is to be continued mostly in its old form, but it is worthy of note that the prosecution of original research is put prominently among the duties of the office, as well as the instruction of students from the University generally. There are nearly a score of candidates, including such well-known names as Mr. W. Noel Hartley, Dr. J. T. Bottomley, and Dr. Dittmar.

Prof. Clerk Maxwell greatly interested the Philosophical Society at its last meeting by an account of Henry Cavendish's unpublished writings and experiments on electricity. He was not generally known to have done much electrical work, and his papers were long in the hands of Sir W. Snow Harris, who is declared by Prof. Maxwell, after careful examination, to have made no use of Cavendish's work without full and adequate acknowledgment. These writings are left in a form quite fitted for publication, and will greatly advance the reputation of the great philosopher. His exactness, his candour, his grasp of the subject, his notable achievements with the small variety of instruments available in his time, were fully shown by the examples cited to the Society. Yet these were less than his remarkable insight into electrical laws, his correct conception of potential, his ideas of investigating the total charges of bodies, and the resistance of electrolytes. Prof. Maxwell thought that nobody had ever possessed so large and various a collection of condensers of known capacity as Cavendish, but his family taciturnity prevented his merits from being fully known. He trained himself to be his own galvanometer, and the general value of his results is remarkable when compared with those obtained by modern instruments.

In regard to university reform, it appears that in some colleges at least there is a danger of the non-resident fellows, who form the largest proportion of the governing body under the act, endeavouring to maintain at a very high number the fellowships to which no duties are attached; of course every such fellowship diminishes the funds available for definite association with the progress of research and education. Some men hold very strongly to the "start in life" theory of fellowships; viz., that they ought to receive three hundred a year for a number of years in order that they may gain three thousand a year in a profession the more speedily.

GLASGOW.—Mr. Gladstone has been elected Lord Rector of Glasgow University in succession to the Earl of Beaconsfield.

BERLIN.—The well-known botanist, Prof. Sachs, of Würzburg, has received a very flattering call to Berlin. Neither pains nor money seem to be spared by the Prussian Government in attracting to the capital the foremost talent of Germany; and certainly in this choice of a successor to Alexander Braun no change of policy is shown.

GÖTTINGEN.—The sum of 50,000 marks has recently been appropriated for the erection of a phyto-physiological institute in the Botanical Gardens.

GIESSEN.—In consequence of the late discussions excited by Prof. Mommsen's articles on the Ph.D. examinations in Germany, the University of Giessen has issued an announcement stating that for the future no faculty can bestow the title of Doctor, except on the basis of a thesis and oral examination.

DORPAT.—The winter attendance at the university is 853, of whom but seven are non-Russian.

BRUNSWICK.—On October 16 interesting ceremonies took place at the opening of the magnificent new buildings of the Carolo-Wilhelminum Polytechnic, in which representatives of the Government, and delegates from all the great German polytechnics, took part. The new edifices are of great extent, and richly equipped with all possible adjuncts for modern technical education, so that this well-known institution will be able to maintain its well-earned reputation. The Carolo-Wilhelminum is the oldest polytechnic in Germany, having been founded in 1745, and the list of its students embraces many distinguished names, such as Gauss, the mathematician, Christopher Codrington, the English commander at the naval victory of Navarino, &c.

SOCIETIES AND ACADEMIES

LONDON

Chemical Society, November 15.—Dr. Gladstone in the chair.—The following communications were made:—First report to the Chemical Society on some points in chemical dynamics, by Dr. Wright and Mr. Luff. An elaborate series of experiments was made to find out the temperatures at which the actions of carbonic oxide, hydrogen, and free amorphous carbon on oxide of iron or oxide of copper are first perceptible. The authors find that this temperature varies with the physical condition of the oxide used, that hydrogen acts, on a given oxide, at a lower temperature than carbon and carbonic oxide, at a lower temperature than hydrogen, and that a given reducing agent begins to act on copper oxide at a lower temperature than on iron oxide.—On the chemistry of cocoa butter, Part I.; two new fatty acids, by C. T. Kingzett. The first acid is a low acid of the series, $C_nH_{2n}O_2$, having the formula $C_{12}H_{24}O_2$, *i.e.*, lauric acid, but it melts at 57°·5. The second acid is a high acid having the formula $C_{64}H_{128}O_2$, crystallising in microscopic needles or granules, melts at 72°·2, and at a high temperature distils apparently unchanged. The author proposes for it the name of theobromic acid. It is pointed out that the usual statement in books, "that cocoa butter yields almost exclusively stearic acid" is entirely incorrect.—The third paper was on the influence exerted by time and mass on certain reactions in which insoluble salts are produced, by Mr. M. P. Muir. The author has taken solutions containing known quantities of calcium chloride and potassium or sodium carbonate mixed, allowed to stand for a certain number of minutes, and then estimated the quantity of calcium carbonate formed. He has arrived at the following conclusions:—That the greater portion of the chemical change takes place during the first five minutes; the reaction then decreases in rapidity. The relative masses of the salts exert an important influence. Thus if the mass of alkaline carbonate be four times that required, the action is completed in five minutes, but if an equivalent quantity only be present the action is not finished in forty-six hours. Potassium carbonate yields more calcium carbonate in a given time than sodium carbonate. An increase of temperature increases, whilst dilution, especially with solutions of potassium or sodium chloride, diminishes the rapidity of the action. Some experiments are given on the action of solutions of calcium sulphate and sodium chloride.

Entomological Society, November 7.—Prof. Westwood, president, in the chair.—Mr. McLachlan exhibited ten of the thirteen species of Lepidoptera collected by Capt. Feilden and Mr. Hart in Grinnell Land, between 78° and 83° N. lat, during the recent Arctic Expedition, and made some remarks upon the general insects of the Arctic Regions.—The Rev. A. Eaton also made some observations upon the same subject.—Mr. Meldola exhibited a five-winged specimen of *Gonepteryx rhamni*, taken in Norfolk by Mr. John Woodgate; likewise a gynandromorphic specimen of *Pieris brassicæ*, caught in Oxfordshire by Mr. J. B. Watson. The right half of the latter insect was female and the left half male.—Mr. H. Goss exhibited a gynandromorphic specimen of *G. rhamni*, captured in Sussex; in this insect also the right side was female and the left side male.—Mr. J. W. Douglas exhibited a specimen of *Polyphylla fullo*, Linn., which had flown on to a steamer at Antwerp, and been thus brought to this country. Mr. Douglas also exhibited a specimen of the rare *Tettigometra impressopunctata* and one of *Typhlocyba debilis*, both taken on Sanderstead Downs; and likewise, for comparison, an example of *T. tenerrima.*—Mr W. C. Boyd exhibited a larva of *Pieris rapæ* attacked by *Microgaster.*—The president read notes on exotic Coleoptera, and exhibited specimens of *Calometopus Nyassæ*, *Amblyodus Nicaraguæ* and drawings of other species.—Prof. Westwood also remarked upon an Indian *Mantis* (*Gongylus gongylodes*) which had been recently described by Dr. Anderson in the *Proceedings* of the Asiatic Society of Bengal for August, 1877, as being a simulator of a flower to a remarkable degree of perfection.—Mr. Wood-Mason also made remarks upon the same subject and upon stridulating organs in crustaceans with reference to a letter on this subject by Mr. Saville Kent in this journal (vol. xvii. p. 11). Mr. Wood-Mason likewise announced the discovery of a stridulating apparatus in a *Phasma.*—Sir Sydney Saunders read a note on the specific identity of the Hampstead *Atypus.* Mr. F. Enoch exhibited and made remarks upon a male and female of this spider.—The following papers were read:—Descriptions of new species of the coleop-

terous genus, *Callirhipis* (*Rhipidoceridæ*), in the British Museum, by C. O. Waterhouse.—Descriptions of a new genus and two new species of *Sphingidæ*, with remarks on the family generally, by A. G. Butler.—Descriptions of *Halticinæ*, by J. S. Baly.—Descriptions of new species of *Cleridæ*, with notes on the genera and corrections of synonymy, by the Rev. H. S. Gorham.

Royal Astronomical Society, November 9.—Dr. Huggins, F.R.S., in the chair.—A very large number of papers were presented.—Lord Lindsay was called upon to read Mr. Gill's report upon the expedition to Ascension to obtain the parallax of Mars, from which it appeared that in spite of meteorological difficulties and many causes of anxiety most satisfactory results had been obtained, and Mr. Gill had gone up a mountain to recruit his health.—Several important mathematical papers were then read; one by the Astronomer-Royal on the solar parallax, as deduced from telescopic observations of the transit of Venus, 1874.—Next a paper by Prof. Adams on the motion of the moon's node, and a paper by Mr. Neison on three small inequalities in the mean motion of the earth, and a small inequality in the mean motion of Mars. These were followed by three observational papers on the recent opposition of Mars; one by the Astronomer-Royal, read by Mr. Christie, giving the summary of what was seen at Greenwich both with the telescope and spectroscope; the next by Mr. N. E. Green, giving an account of his expedition to Madeira and what he saw of Mars with a fine 13-inch reflector. This paper was accompanied by a series of beautiful drawings of the planet by the author. The third paper, on Mars, was by Mr. John Brett, being a discussion of a series of telescopic observations made in Cornwall, the purport of which was to show that the generally received hypothesis of the physical condition of Mars was altogether fallacious, neither the snows nor the seas having any foundation in fact. This paper was also illustrated by a series of drawings.—Then followed a paper by Lord Lindsay, on a new form of spectroscope, and the meeting adjourned.

Anthropological Institute, November 13.—Dr. John Evans, F.R.S., president, in the chair.—The Rev. T. A. Bennett and F. V. Dickins were elected members.—An interesting series of casts of skulls made of papier-maché were exhibited, and a special vote of thanks was ordered to be sent to Prof. Bogdanow, of Moscow, by whom they were presented to the Institute.—Major-Gen. A. Lane Fox, F.R.S., exhibited some flint flakes from Egypt, and a note from Capt. R. F. Burton was read on the same.—The director then read a paper by Mr. H. H. Howorth, F.S.A., on the spread of the Slaves: Part I., the Croats.—This was followed by a paper on the Castilieri d'Istria, by Capt. R. F. Burton, H.M.'s Consul at Trieste.—Mr. Hyde Clarke, the President, Major-Gen. A. Lane Fox, and Mr. Moggridge took part in the discussions.

Institution of Civil Engineers, November 13.—Mr. George Robert Stephenson, president, in the chair.—The paper read was a review of the progress of steam shipping during the last quarter of a century, by Mr. Alfred Holt, M. Inst. C.E., of Liverpool.

Paris

Academy of Sciences, November 12.—M. Peligot in the chair.—M. Faye presented the volume of the *Connaissance des Temps* for 1879.—On some applications of elliptic functions (continued), by M. Hermite.—*Résumé* of a history of matter (fourth article) by M. Chevreul. This relates to the views of Lavoisier, Stahl, Scheele, Cavendish, and Priestley.—Observations on the principle of maximum work and on the spontaneous decomposition of hydrated bioxide of barium, by M. Berthelot.—On the limits of etherification, by M. Berthelot. In experimenting on etherification sixteen years ago he put aside a number of mixtures to be kept a considerable time, in order to ascertain the limit of the reactions produced at ordinary temperatures. The mixtures consisted of acetic acid and alcohol (equal equivalents), acetic acid and glycerine, tartaric acid and alcohol, valeric acid and alcohol. He has now examined these. The general laws of etherification are confirmed, and especially the identity of the limits of combinations between acids and alcohols, from ordinary temperatures up to 260°.—On the order of appearance of the first vessels in the shoots of some Leguminosæ (second part), by M. Trécul.—The Academy elected a commission to present a list of candidates for the vacancy among the Foreign Associates, caused by the death of M. von Baer.—On the numeration of globules of milk for the analysis of woman's milk, by M. Bouchut. A drop of milk is mixed with 100 drops of slightly saline water (distilled). A drop of the mixture is placed under the microscope, whose eye-piece is divided into squares; the number of globules in each square is counted, and the average taken; from this may be deduced the number in one cubic millimetre. The globules were thus counted in milk of 158 nurses, before, during, and after suckling. The average of globules is about 1,026,000 per cubic millimetre of milk, or a hundred and two milliards six hundred millions per litre; but between 800,000 and one million per cubic millimetre, the milk is considered of good quality. In one table are given the density and the quantity of butter corresponding to given numbers of globules of cow's milk.—New formulæ for the study of the motion of a plane figure, by M. Haton de la Goupillière.—On the migration of the puceron of the cornel tree and its reproduction, by M. Lichtenstein. This puceron comes from the roots of gramineæ, and returns to them. Its mode of reproduction is that termed by the author *anthogenesis*.—Observations on the subject of a recent communication from M. Fabre, by M. Millardet. The secretary announced a new biennial prize, founded by M. Maujean.—Discovery of a small planet at the Observatory of Paris, by M. Paul Henry.—Discovery of a small planet at the Observatory of Pola, by M. Palisa.—Observations of planets 125 and 176 made at the Paris Observatory (equatorial of the garden), by MM. Paul and Prosper Henry.—New stellar systems, by M. Flammarion.—On the equation with partial derivatives of the third order expressing that the problem of geodesic lines, considered as a problem of mechanics, supposes an algebraic integral of the third degree, by M. Lévy.—On the evolution of red corpuscles in the blood of oviparous vertebrates, by M. Hayem. The red corpuscles proceed from a peculiar colourless element, which from the first phases of development is distinct from the white corpuscles; the name of *hematoblast* is given it. The white corpuscles are foreign to the formation of the red, both in oviparous vertebrates and in the higher animals; but whereas in the latter the red corpuscles of new formation are coloured, whatever their minuteness, in the oviparous, the embryonic corpuscles are at first quite without hæmoglobin.—On the spots and crevices of pears, by M. Prillieux. These are due to the growth of a small parasitic champignon.—On the semi-diurnal variations of the barometer, by M. De Parville. He thinks it improbable that aqueous vapour has a preponderating influence in these variations.—On the quantities of heat liberated in mixtures of sulphuric acid and water, by M. Maumené. Sulphuric acid recently heated does not liberate, with water, the same quantity of heat as the same acid kept several months. This phenomenon, denoted as a tempering of liquids, seems to him a source of error in researches on thermo-chemistry not hitherto considered.

CONTENTS

THURSDAY, NOVEMBER 29, 1877

FLORA OF MAURITIUS AND SEYCHELLES

Flora of Mauritius and the Seychelles: a Description of the Flowering Plants and Ferns of those Islands. By J. G. Baker, F.L.S. (London: L. Reeve and Co., 1877.)

THIS compact volume of nearly 600 pages, adds another to the already long list of colonial floras prepared at Kew and issued under the authority and at the expense of the Colonial Government. It is arranged on the same plan as the other floras, many of them so well known, giving first, some general remarks on the physical geography and botany of the islands, and then that admirable outline of elementary botany prepared by Mr. Bentham, and which contains every definition necessary in descriptive botany, thus enabling the student to follow the technical descriptions given in the "Flora" itself. The work is almost entirely from the pen of Mr. J. G. Baker (the Orchids being by Mr. Le Marchant Moore, and the Palms and Pandani by Dr. I. B. Balfour), and is only another example of the indomitable industry so characteristic of Mr. Baker. The materials at the disposal of the author have been ample, and probably there is but little left to discover in Mauritius, the Seychelles, and Rodriguez, although many forms have not as yet been fully determined owing to the want of perfect specimens. Hence it is desirable that naturalists visiting the islands should endeavour to complete our knowledge of these imperfectly known plants. The smaller dependencies of Mauritius have not been explored botanically, hence there is probably a rich field for the investigator of these numerous islands. It is, moreover, all the more desirable to have these islands explored as the native flora of the islands already known has been completely altered by the introduction of cultivated plants and weeds as well as by the destruction of the native forests. Thus it is probable that in some of the undisturbed islands a rich native flora may be met with, or that some of the forms either rare or extinct on other islands, may yet be comparatively abundant.

Mauritius is about 39 miles by 35, and has an area of 700 square miles, or a little smaller than the County of Surrey. It is situated at a distance of about 500 miles from Madagascar and 100 miles from Bourbon, and is just within the Tropic of Capricorn. The northern part of the island is a low plain covered with sugar plantations. In the centre is an elevated plateau rising to about 1,500 feet above the sea-level, the great mass of the rocks being entirely volcanic. Outside the central plateau, and within a short distance of the sea, rise the three principal mountain ranges, the highest portions being from 1,900 to 2,900 feet in height. There are two small lakes in the central plateau, the Grand Bassin and the Mare aux Vacoas. There are six rivers, about ten to twelve miles in length, and numerous small rivulets. The climate is warm, and at Port Louis the mean annual temperature is 78° F. As a result, the vegetation has a decidedly tropical character. There are however, a few south temperate plants present, and also a number of the widely-spread temperate forms, as *Nephrodium filix-mas*, *Cardamine hirsuta*, *Juncus effusus*, *Convolvulus arvensis*, *Plantago major*, and *P. lanceolata*. Sugar is extensively cultivated in Mauritius. The increase in the cultivation of sugar has led to the destruction of the forests, which at one time covered the island to the water's edge. As a result of the destruction of the forests, the indigenous flora has almost become destroyed. The orchids, ferns, pandani, and the shade-loving plants, and the curious endemic trees and shrubs have, within 100 years, been either entirely exterminated, or else have become exceedingly rare and local. The native vegetation thus partly exterminated has been replaced by a number of introduced trees, shrubs, and weeds, to an extent only exceeded by the destruction of the indigenous flora of St. Helena. There seem to be about 269 introduced plants in Mauritius, and 869 undoubted native species, making a total flora of about 1,138.

The Seychelles are situated 900 miles north-east of Mauritius, in 3°–6° south latitude, and consist of a group of about thirty islands, most of them of very small size. The islands are entirely granitic. The largest of the group, Mahé, has an area of 30,000 acres; the best cultitivated and most populous is La Digue, with an area of 2,000 acres. The mountains range from about 1,500 to 3,000 feet in height. The seasons are similar to those of Mauritius. Cotton was at one time extensively cultivated, and the aboriginal forests were destroyed to make room for cotton plantations. Now cotton is hardly cultivated, the chief exports from the island being cocoa-nut oil and fibre. The vegetation is wholly tropical; the few temperate species found in Mauritius being absent from the Seychelles. The number of flowering plants and ferns from these islands is 338. Five genera of palms and one genus of Ternstroemiaceæ are endemic. The endemic palms are mostly well known, and belong to the genera Deckenia, Nephrosperma, Roscheria, Verschaffeltia, Lodoicea, and Stevensonia. The total number of endemic species is sixty. The rest of the flora consists chiefly (250) of widely distributed tropical plants, and between twenty and thirty are of characteristic Mascarene types. The flora was expected to have been much richer in endemic forms from the isolated position and peculiar geological construction of the islands than it has proved to be after the most careful examination.

Rodriguez is situated 300 miles to the north and east of Mauritius, and is an island about eleven miles by five, with the hills in the interior reaching an elevation of little over 1,000 feet. The rock is entirely volcanic, and the climate similar to that of Mauritius. The flora must have undergone great changes, as the earliest records of the island state that it was entirely wooded. The plants of the island number about 202 wild flowering plants and ferns, nearly all collected by that rising young botanist, Dr. I. B. Balfour, one of the staff of the Transit of Venus Expedition to Rodriguez. Of the 202 wild species, thirty-six are peculiar to the island; and there are three endemic monotypic genera, one Mathurina having been discovered and described by Dr. I. B. Balfour.

The total number of species as given by Baker may be thus summarised:—There are 1,058 native species in the "Flora," 869 natives of Mauritius, 338 natives of Seychelles, and 202 native in Rodriguez; 269 are naturalised in these islands, thus giving a total number of 1,327 species included in the "Flora of Mauritius and the Seychelles." The distribution of the species in the flora

is also interesting. Thus there are 304 endemic species, 232 Mascarene species, *i.e.*, plants confined to Bourbon, Mauritius, Madagascar, and the Comoros; 66 African but not Asian, 86 Asian but not African; 145 common to Asia and Africa; and 225 common to the Old and New World. If we take the percentages we have the following results:—29 per cent. endemic, 22 per cent. Mascarene, 21 per cent. common to the Old and New World, 14 per cent. common to Asia and Africa, 8 per cent. Asian but not African, and 6 per cent. African but not Asian. From this it is evident that one-half of the wild plants of the flora are restricted to the Mascarene Archipelago.

The orders containing the greatest number of species are the following:—Orchidaceæ, 79; Gramineæ, 69; Cyperaceæ, 62; Rubiaceæ, 57; Euphorbiaceæ, 45; Compositæ, 43; Leguminosæ, 41; Myrtaceæ, 20. There also 168 species of Filices, but it is rather unfair to consider the Filices as an order equivalent say to the Euphorbiaceæ or Myrtaceæ in the above enumeration.

The descriptive part of the flora is elaborated in the same manner as the colonial floras already published, and is, as already mentioned, almost entirely the work of Mr. Baker, with the exception of the Orchids, Palms, and Pandani. Any one acquainted with Mr. Baker's work will know that any detailed notice of the descriptive part of the present volume is superfluous.

W. R. McNab

OUR BOOK SHELF

Die Geologie. Franz Ritter von Hauer. (Vienna: A Holder, 1877.)

It is a good sign both of the progress of geological study in Austria and of the value of this manual by the director of the Austrian Geological Survey, that a second edition of the work has been called for within three years of the date of its publication. A sample of the revised issue which has been sent to us fully bears out the description on its title-page that it is enlarged and improved. The original work, besides its clearly-expressed introductory chapters on general dynamical and mineralogical geology, is especially a valuable repertory of information regarding the structure and palæontology of the Austro-Hungarian monarchy. In the new edition, Ritter von Hauer is evidently doing his best to keep his manual abreast of the time. The book is well-printed, but the author is still in the hands of a very poor wood-engraver. The new cuts are as rude and feeble as ever.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Fritz Müller on Flowers and Insects

The enclosed letter from that excellent observer, Fritz Müller, contains some miscellaneous observations on certain plants and insects of South Brazil, which are so new and curious that they will probably interest your naturalist readers. With respect to his case of bees getting their abdomens dusted with pollen while gnawing the glands on the calyx of one of the Malpighiaceæ, and thus effecting the cross-fertilisation of the flowers, I will remark that this case is closely analogous to that of Coronilla recorded by Mr. Farrer in your journal some years ago, in which parts of the flowers have been greatly modified, so that bees may act as fertilisers while sucking the secretion on the outside of the calyx. The case is interesting in another way. My son Francis has shown that the food-bodies of the Bull's-horn Acacia, which are consumed by the ants that protect the tree from its enemies (as described by Mr. Belt), consist of modified glands; and he suggests that aboriginally the ants licked a secretion from the glands, but that at a subsequent period the glands were rendered more nutritious and attractive by the retention of the secretion and other changes, and that they were then devoured by the ants. But my son could advance no case of glands being thus gnawed or devoured by insects, and here we have an example.

With respect to *Solanum palinacanthum*, which bears two kinds of flowers on the same plant, one with a long style and large stigma, the other with a short style and small stigma, I think more evidence is requisite before this species can be considered as truly heterostyled, for I find that the pollen-grains from the two forms do not differ in diameter. Theoretically it would be a great anomaly if flowers on the same plant were functionally heterostyled, for this structure is evidently adapted to insure the cross-fertilisation of distinct plants. Is it not more probable that the case is merely one of the same plant bearing male flowers through partial abortion, together with the original hermaphrodite flowers? Fritz Müller justly expresses surprise at Mr. Leggett's suspicion that the difference in length of the pistil in the flowers of *Pontederia cordata* of the United States is due to difference of age; but since the publication of my book Mr. Leggett has fully admitted, in the *Bulletin* of the Torrey Botanical Club, that this species is truly heterostyled and trimorphic. The last point on which I wish to remark is the difference between the males and females of certain butterflies in the neuration of the wings, and in the presence of tufts of peculiarly-formed scales. An American naturalist has recently advanced this case as one that cannot possibly be accounted for by sexual selection. Consequently, Fritz Müller's observations which have been published in full in a recent number of *Kosmos*, are to me highly interesting, and in themselves highly remarkable.

Charles Darwin

Down, Beckenham, Kent, November 21

You mention ("Different Forms of Flowers," page 331), the deficiency of glands on the calyx of the cleistogamic flowers of several Malpighiaceæ, suggesting, in accordance with Kerner's views, that this deficiency may be accounted for by the cleistogamic flowers not requiring any protection from crawling insects. Now I have some doubt whether the glands of the calyx of the Malpighiaceæ serve at all as a protection. At least, in the one species, the fertilisation of which I have very often witnessed, they do not. This species, *Bunchosia gaudichaudiana*, is regularly visited by several bees belonging to the genera Tetrapedia and Epicharis. These bees sit down on the flowers gnawing the glands on the outside of the calyx, and in doing so the under side of their body is dusted with pollen, by which, afterwards, other flowers are fertilised.

There are here some species of Solanum (for instance *S. palinacanthum*) bearing on the same plant long-styled and short-styled flowers. The short-styled have papillæ on the stigma and apparently normal ovules in the ovary, but notwithstanding they are male in function, for they are exclusively visited by pollen-gathering bees (Melipona, Euglossa, Augochlora, Megacilissa, Eophila, n. g., and others), and these would probably never insert their proboscis between the stamens.

In a few months I hope to be able to send you seeds of our white-flowered violet with subterranean cleistogamic flowers. I was surprised at finding that on the Sêrra (about 1,100 metres above the sea) this violet produced abundant normal fruits as well as subterranean ones, while at the foot of the Sêrra, though

it had flowered profusely, I could not find a single normal fruit, and subterranean ones were extremely scarce.

Acccording to Delpino the changing colours of certain flowers would serve to show to the visiting insects the proper moment for effecting the fertilisation of these flowers. We have here a Lantana the flowers of which last three days, being yellow on the first, orange on the second, purple on the third day. This plant is visited by various butterflies. As far as I have seen the purple flowers are never touched. Some species inserted their proboscis both into yellow and into orange flowers (*Danais crippus, Pieris aripa*), others, as far as I have hitherto observed, exclusively into the yellow flowers of the first day (*Heliconius apseudes, Colænis julia, Eurema leuce*. This is, I think, a rather interesting case. If the flowers fell off at the end of the first day the inflorescence would be much less conspicuous; if they did not change their colour much time would be lost by the butterflies inserting their proboscis in already fertilised flowers.

In another Lantana the flowers have the colour of lilac, the entrance of the tube is yellow surrounded by a white circle; these yellow and white markings disappear on the second day.

Mr. Leggett's statements about *Pontederia cordata* appear to me rather strange, and I fear that there is some mistake. In all the five species of the family which I know the flowers are so short-lived, lasting only one day, that a change in the length of the style is not very probable. In the long-styled form of our high-and Pontederia the style has its full length long before the flowers open. In my garden this Pontedaria is visited by some species of Augochlora collecting the pollen of the longest and mid-length stamens; they are too large to enter the tube of the corolla, and have too short a proboscis to reach the honey; they can only fertilise the long-styled and mid-styled forms, but not the short-styled.

Among the secondary sexual characters of insects the meaning of which is not understood, you mention ("Descent of Man," vol. i., p. 345) the different neuration in the wings of the two sexes of some butterflies. In all the cases which I know this difference in neuration is connected with, and probably caused by, the development in the males of spots of peculiarly-formed scales, pencils, or other contrivances which exhale odours, agreeable no doubt to their females. This is the case in the genera Mechanitis, Dircenna, in some species of Thecla, &c.

FRITZ MÜLLER

Blumenau, St. Catharina, Brazil, October 19

The Radiometer and its Lessons

PROF. OSBORNE REYNOLDS's letter in NATURE (vol. xvii. p. 26) has directed attention prominently to the circumstance that two hypotheses have been submitted to the scientific world as explanations of the force and motions which Mr. Crookes had shown to exist—one by Prof. Osborne Reynolds, the other by myself.

Prof. Osborne Reynolds's explanation is based on the fact that when a disc with vertical sides is heated on one side and exposed to a gas, a convection current sets in, which draws a continuous supply of cold gas into contact with the hot surface of the disc. As this cold gas reaches the disc it is expanded, and thus its centre of gravity is thrown further from the disc. Accordingly, the disc, if freely suspended, will move in the opposite direction so as to keep the centre of gravity of the gas and disc in the same vertical line as before, and, if not freely suspended, will suffer a pressure tending to make it move in that direction. If I have understood Prof. Reynolds aright, this is both a correct and full description of his explanation as last presented.

My explanation, on the other hand, is based on *molecular* motions which go on in the gas without causing any molar motion, and is independent of convection currents. Prof. Reynolds is therefore, I conceive, fully justified in denying that my theory has supplied any deficiency in his explanation. As he points out, the two explanations are incompatible; if either is correct, the other is wholly wrong.

It is easy to apply comparative tests to the rival hypotheses by making a selection from Mr. Crookes's incomparable experiments, from the experiments by Mr. Moss and myself, and from instances of compressed Crookes's layers in the open atmosphere; but it is not easy to make the choice so as to bring the abundant evidence within the compass of a letter.

These tests might take various forms, of which perhaps the most direct is to ascertain whether the force is affected by variations in the convection current, as required by Prof. Reynolds's hypothesis, or is independent of convection, but increased when the heater and cooler are brought nearer together, as required by mine.

To test this Mr. Crookes mounted a radiometer in a receiver consisting of two unequal bulbs connected by a large tube. The movable portion could be transferred from one bulb to the other through the tube. In the small bulb the convection current is most impeded, and at the same time the heater and cooler are closest together. Mr. Crookes found that the motion of the radiometer was more rapid in the small bulb than in the large one, in conformity with my theory, and in opposition to Prof. Reynolds's. The same is the uniform drift of a vast number of other experiments by Mr. Crookes, and of those by Mr. Moss and myself, from which it appears that whenever the heater and cooler are made to approach there is an increase in the force, and that the force is not appreciably affected by variations of the convection current, or by its suppression.

This may also be proved, and quite conclusively, by observations not requiring apparatus. Drops in the spheroidal state and the drops which are often seen floating on the surface of volatile liquids, as, for example, the drops which run about on the surface of the sea in certain states of the weather when water drips from an oar, are supported by Crookes's layers of air intervening between them and the liquid beneath. Similarly a red-hot copper plate will float on water, supported on a Crookes's layer, and many other instances of a like kind might be adduced. In such cases, where the film of air is thin and for the most part horizontal, it is manifest that there is no opportunity for those convection currents to arise which are required by Prof. Reynolds's hypothesis, while in all of them there are the peculiar molecular motions of my theory.

The absence of convection currents which could produce an appreciable effect may also be proved in those radiometers of which the arms whisk round at a very rapid speed, and in many other cases that would take too much space to describe here.

Again, a tangential force which may be rendered considerable is an immediate consequence of my theory, but has no place as a consequence of Prof. Reynolds's. Now its presence has been verified by Mr. Moss and myself, and by Mr. Crookes in an exquisitely beautiful apparatus suggested for this purpose by Prof. Stokes, as well as, in a less degree, in all Mr. Crookes's apparatus with curved or crumpled discs.

Hence Prof. Osborne Reynolds's hypothesis is not the explanation of Crookes's stress. It alleges a cause which is in certain cases a *vera causa*, but not *the* cause of what is to be explained. So far as I can form a judgment, its merit was collateral, and not intrinsic. It was the first attempt at a reduction of the observed phenomena to known physical laws. Though not accounting for them, it was sufficiently plausible to attract the attention of Prof. Reynolds and other physicists. It thereby had the important effect of suggesting Dr. Schuster's most valuable experiment, which was the first that established the cardinal fact that the forces within a radiometer case are balanced.

The conclusion to which we are thus led by a purely *experimental* inquiry is supported by an examination of the chief *theoretic* assertions of Prof. Osborne Reynolds's letter, viz., 1. That an essential part of my explanation "is contrary to the law of the diffusion of heat in gases;" and 2. "That the force arising from the communication of heat from a surface to adjacent gas of any particular kind depends only on one thing, the rate at which heat is communicated, and to this it is proportional."

Both of these statements have been set down by Prof. Osborne Reynolds in error; the first from not observing that the ordinary laws for the propagation of heat through a gas do not apply to compressed Crookes's layers; and the second from a misapprehension of the actual agency at work in radiometers and other similar apparatus. I will proceed to establish these two positions.

1. So long as a gas is in its ordinary state the distribution of the velocities of the molecules is the same in all directions, and when heat is imparted to the gas it does not disturb this uniformity of structure. The heat simply increases the mean velocity, and the actual velocities continue to be distributed about

their mean value according to the well-known exponential law, and are alike in all directions. But the gas of a compressed Crookes's layer is not in the ordinary state; it is under constraint, as I have elsewhere shown, owing to the proximity of the heater and cooler between which it is confined. In consequence of this constraint there are what I have described as processions going on in the layer of gas: in other words, *the velocities of the molecules at any situation within the layer are not alike in all directions, but are greatest in the direction of the cooler, least in the direction of the heater, and of intermediate values in lateral directions.* The heat in crossing the layer from the heater to the cooler maintains this polarised molecular structure, and if the flow of heat is increased it does not simply increase the mean velocity of the molecules, but also augments the disparity of the velocities in different directions.

Now the ordinary laws of the communication of heat to and through gas are based on the opposite supposition that when heat reaches any portion of the gas all the molecules of that portion are equally affected, that though their mean velocity is increased the laws of the distribution of the velocities about that mean, and in different directions, are not changed. Hence Prof. Osborne Reynolds has fallen into an error in applying the ordinary "law of the diffusion of heat in gases" to the case of compressed Crookes's layers. The law employed by Prof. Reynolds does not prevail unless there is sufficient room in front of the heater for the development of a complete *unrestricted* Crookes's layer; Crookes's force only presents itself when the thickness of that layer is restricted by a cooler.

The transmission of heat across Crookes's layers is made the subject of investigation in a memoir which I laid before the Royal Dublin Society last May, which has recently been printed in the *Transactions* of that body, and of which a copy will shortly appear in the *Philosophical Magazine*. The law proves to be entirely different from any of the laws for the propagation of heat hitherto known, and I have therefore called this mode of transferring heat by a new name—*the penetration of heat*. Moreover, the results of theory had been verified by anticipation more than thirty years before by MM. De la Provostaye and Desains, in two elaborate experimental investigations into what we now know to have been the penetration of heat; so that our knowledge of its laws, which are entirely different from the laws of the diffusion of heat, quoted by Prof. Reynolds, already stands on both a deductive and experimental basis.

2. Prof. Osborne Reynolds further states that with each gas the force depends only on one variable, viz., the rate at which heat is communicated by the heater to the adjacent gas, and that it is proportional to this rate. Probably owing to a mere slip on Prof. Reynolds's part, he has here omitted a second variable, viz., the temperature of the gas, which is implicitly contained in the equation of his first paper to which he refers. With this, however, I have no concern; what I have to point out is that in making the statement, whether in an amended or in its actual form, Prof. Osborne Reynolds has overlooked the fact that the machinery of Crookes's stress consists of a cooler as well as of the heater and intermediate gas, and that *a sufficient proximity of the cooler is essential.* Accordingly, the true expression for the force (of which I hope to publish an investigation made some time ago, as soon as my health will allow) is not so simple as Prof. Reynolds supposes, but is a function of the temperatures of the heater and cooler, and of the rate at which heat reaches the cooler by penetration, in addition to the single variable which one Prof. Osborne Reynolds admits. The vice of the mathematical reasoning, on which Prof. Reynolds bases his statement, is that it starts from a kinetic expression for the pressure of gas, which is only true when the mean of the squares of the velocities of the molecules is the same in all directions. Accordingly, his discussion does not reach the phenomenon it professes to explain;—it is irrelevant to the case of compressed Crookes's layers, in which the gas is polarised, and where the degree of polarisation is itself a function of Prof. Reynolds's variable along with other thermal variables.

Thus, in all parts of his inquiry, Prof. Osborne Reynolds has been led into error by having regarded the gas of compressed Crookes's layers as gas in its ordinary state; in other words, because he has not had a glimpse of that peculiar molecular structure in the gas, which is the real source of Crookes's stress. From a review of the whole subject I think myself justified in submitting that the only discovery which brought with it any knowledge of the cause of Crookes's stress and of penetration, was the discovery that gas could assume this polarised condition; and I must say that it does not appear to me that to this discovery Prof. Osborne Reynolds has in any degree contributed.

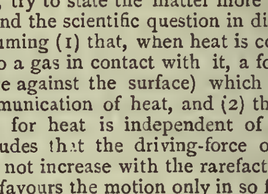
Dublin, November 15 G. JOHNSTONE STONEY

Postscript, November 23.—Prof. Osborne Reynolds has written a further letter to NATURE (vol. xvii. p. 61), in which he says:—"The fact that Mr. Stoney has in no way referred to my work, although I preceded him by some two years, has relieved me from all obligation to discuss Mr. Stoney's theory." I am sorry Prof. Osborne Reynolds should have thought me capable of discourtesy or inattention to the claims of a fellow-worker, and fortunately I am not conscious of being liable to the imputation. I became acquainted with Prof. Reynolds's paper in the interval between the publication of my first and second papers, but did not refer to it in my second paper because I found on reading it that Prof. Reynolds's explanations of Crookes's force were all erroneous (viz., the evaporation of mercury or other vapour, and heat communicated to diffused particles of gas, or to gas brought by convection currents); because the mathematical analysis with which he supports his hypotheses is irrelevant to the problem with which he is dealing; and finally, because for the purposes of my investigation I had no occasion to point out these mistakes, inasmuch as Prof. Reynolds had not approached the subject of polarised layers of gas and their mechanical properties, which was the subject matter of my papers.

I ought to add a word in reference to the criticism of my memoir on penetration, which is contained in Prof. Osborne Reynolds's last letter. He seems to overlook a condition laid down in the second paragraph of my memoir, which disposes of the criticism, viz.: "Let us further regard this gas as *a perfect non-conductor of heat.*" Your mathematical readers will at once perceive that this condition is a legitimate simplification of the problem, because the diffusion or conduction of heat in gases is very sluggish compared with penetration, the phenomenon with which I was dealing.

IT appears from Prof. Osborne Reynolds's last letter that my wish to make my note to NATURE (vol. xvii. p. 43) a fortnight ago short, led me to make it obscure. I will therefore, with your permission, try to state the matter more clearly.

As I understand the scientific question in discussion before us, it is this:—Assuming (1) that, when heat is communicated from a solid surface to a gas in contact with it, a force arises (equivalent to a pressure against the surface) which is proportional to the rate of communication of heat, and (2) that the conducting power of a gas for heat is independent of its density, Prof. Reynolds concludes that the driving-force on the vanes of a radiometer does not increase with the rarefaction of the air, but that rarefaction favours the motion only in so far as it lessens the opposing force due to convection-currents. I, on the other hand, while admitting Prof. Reynolds's premisses, do not admit his conclusion. On the contrary, I believe that, in the radiometer, rarefaction must increase the rate of communication of heat, and hence also the force. To see how this may be, let A B represent the thickness of a stratum of gas contained between two parallel solid surfaces, whose temperatures, measured from any zero, are represented respectively by A C and B D. Then, I imagine, the flow of heat through the gas will take place *as though* there were, in contact with each solid surface, a layer of gas whose temperature is throughout the same as that of the contiguous solid, and whose thickness is equal (or at least proportional) to the mean length of path of the molecules. The virtual thickness of the stratum of gas, whose conductivity comes into account in determining the rate of transmission of heat, is then the actual thickness diminished by the aggregate thicknesses of these two layers. For example, if A a and B b represent the thicknesses of the hot and cold layers respectively, the virtual



thickness of the stratum across which conduction takes place is $a\,b$, and the distribution of temperature from side to side of the whole quantity of gas is given by the ordinates of the

broken line $C\,c\,d\,D$. If now the gas is rarefied, the mean length of path of the molecules, and consequently the thickness of each of the layers of uniform temperature, is increased, and the thickness of the stratum across which true conduction takes place is diminished. If, for example, the thicknesses of the layers become $A\,a'$ and $B\,b'$, the thickness of the conducting stratum is reduced to $a'b'$, and the distribution of temperature is represented by the ordinates of the broken line $C\,c'd'D$. The rate of flow of heat in the two cases will be proportional conjointly to the inclination of the line cd or $c'd'$ to AB, and to the conductivity of the gas; but as the latter factor does not vary with density, the result is proportional to the former only. It is evident that if this view of the matter is approximately correct rarefaction must increase the rate of transmission of heat across a stratum of gas whenever the increased length of path of the molecules, resulting from rarefaction, bears an appreciable proportion to the thickness of the stratum, but that it will have no sensible effect of the kind when the stratum of gas is very thick or the rarefaction itself very small.

I ought to acknowledge that precisely this mode of representing the effect of rarefaction occurred to me only as I was thinking how I could comply with Prof. Osborne Reynolds's wish that I should be "more explicit." When I wrote my last note I had in mind a somewhat different mode of action whereby it seemed that an equivalent result to that here pointed out would be brought about. The further consideration which Prof. Reynolds's letter in this week's NATURE has caused me to give to the subject has, however, led me to think that the view given above is not only clearer, but also a nearer approach to a correct representation of the facts than the one I had previously adopted. But apart from the accuracy of any particular explanation of *how* such a result can occur, the experimental evidence seems to me to prove conclusively that the force in the radiometer *does* increase (up to a certain point) with rarefaction. The action of convection currents depends to so great an extent on such conditions as the size and shape of the envelope and the position of the fly, and they must be so much disturbed as soon as the vanes begin to move, that if they played the essential part which I understood Prof. Reynolds to attribute to them, I cannot think that the effect of rarefaction would present anything like the degree of regularity that has been actually observed.

November 24 G. CAREY FOSTER

Mr. Crookes and Eva Fay

THE precise nature and grounds of the attestation given by Mr. Crookes to Eva Fay's "mediumship" appear in an article entitled "Science and Spiritualism" in the *Daily Telegraph* for March 13, 1875, embodying a communication made by Mr. Crookes himself to the *Spiritualist* of the preceding day.

The readers of NATURE will be able to judge for themselves by the following extracts from this article, whether Eva Fay was not fully justified in announcing her "mediumship" to the American public as having received Mr. Crookes's "endorsement."

"In the *Spiritualist* of yesterday, Mr. William Crookes, F.R.S., prints an account of a *séance* at his house in which Mrs. Fay exhibited some remarkable phenomena while under severe scientific conditions. The sitting took place on Friday evening, February 19, in the presence of several well-known men of science; and, on Mr. Crookes's suggestion, the medium was so placed as to form part of an electrical current connected with a galvanometer, indicating on a graduated circle the exact deflection produced by the current. In each hand Mrs. Fay held the terminal of a wire, and the fact that she kept continuous hold of the terminals was guaranteed by the amount of deflection of the galvanometer needle, and by the flashes of light which accompany each change of position or break of contact. This method was agreed to by the *savants* present, as affording absolute certainty that the medium could not remove her hand or body from the wires, whether in a trance or otherwise, without the fact being made known by the galvanometer. The sitting was held in a well-lighted drawing-room, the medium thus 'tied down by electricity' being screened by a curtain. What followed is thus described by Mr. Crookes:—

"We commenced the tests at 8.55 P.M.; the deflection by the galvanometer was 211 deg., and the resistance of Mrs. Fay's body 6,600 British Association units. At 8.56 the deflection was 214 deg., and at this moment a handbell began to ring in the library. At 8.57 the deflection was 215 deg. A hand came out of the cabinet on the side of the door farthest from Mrs. Fay."

A number of other occurrences of the like kind are then recorded; the hand reappearing from time to time, and presenting different members of the party with books and other articles severally appropriate to each, of which Mr. Crookes considered it impossible that Mrs. Fay could herself have gained possession.

He adds:—"Before Mrs. Fay came to the house that evening, she only knew the names of two of the guests who would be present; but during the evening the intelligence at work displayed an unusual amount of knowledge about the sitters and the labours of their lives."

The entire extract (which I should have reproduced in full if the space of NATURE had permitted) would show that—1. It *is* true that Mr. Crookes gave his *public attestation* to the genuineness of the so-called spiritualistic manifestations which occurred in his house through the "mediumship" of Eva Fay.

2. It *is* true that Eva Fay went back to the United States armed with Mr. Crookes's *public attestation* of the genuineness of the performances which took place at his house.

3. It *is* true that Mr. Crookes wrote a letter to a gentleman in the United States, giving a similar attestation, which letter was published in *facsimile* in an American newspaper.—The only thing that was *not* true in my statement, was that (through having mislaid the slip containing it) I spoke of this letter as having been addressed to Eva Fay herself, and having been written before her departure.

4. It *is* true that Eva Fay's *public* performances in London were imitated at the time by Messrs. Maskelyne and Cooke; and further, that her business agent *spontaneously offered* Mr. Maskelyne to expose (for a sum of money) the tricks by which she cheated "the F.R.S. people."—If NATURE thinks it worth while to admit into its columns the full particulars of that offer, Mr. Maskelyne is quite ready to furnish them. His general assertion of the fact has been long before the public ("Modern Spiritualism," p. 122), and has remained unchallenged, so far as I am aware, until now.

5. It *is* true that the whole *modus operandi* of Eva Fay's public "manifestations" in the United States has been publicly exposed in New York and Boston by Mr. Washington Irving Bishop, as stated in *Fraser's Magazine* for the present month.

It was not only in entire ignorance of these proceedings, but under the influence of a report in circulation among the Fellows of the Royal Society—that "Mr. Crookes had given up Spiritualism," that I expressed to Mr. Crookes, on the occasion of his receiving the Royal Medal, my desire to "bury the hatchet." But I most assuredly did not consider myself thereby pledged to keep silence in regard to any further proceedings of the like kind; and only learned at the beginning of the present year that Eva Fay had been trading on the "endorsement" given her by "Mr. Crookes and other Fellows of the Royal Society," which she naturally "improved" into that of "the Royal Society of England."

November 19 WILLIAM B. CARPENTER

Potential Energy

WILL you permit me to express a certain amount of scepticism as to the reality of Mr. O'Toole's troubles on this subject? That some statements made in the text-books quoted are not clear—that by an ingenious collocation of isolated passages from different authors some absurd conclusions may be drawn—we admit, but it may be doubted whether a Publius with the keen critical power of Mr. O'Toole would not be able to eliminate these errors or ambiguities by a reference to the context. In support of this position let us take the points raised by Mr. O'Toole in the order adopted by him.

A.—*Potential E., as meaning Energy* in posse.

The "doctors" quoted, with one exception, represent potential E.—not as energy *in posse*, but as kinetic energy *in posse*—a very different thing. Just as gold coin—though certainly not money *in posse*—may correctly be called silver coin (another form of money) *in posse.*

But it is said this name—and certain phrases employed by the doctors—imply that potential E. is "energy of about-to-supervene motion, or that it does not perform work except through the resulting E. of motion." Mr. O'Toole is so distressed because poor Publius is susceptible to this impression, that I feel some hesitation in asking what is wrong in it? How can work be done without motion? How can the potential E. of a system change without a change in the configuration—*i.e.*, motion of the system? Where is the mistake in the conception of potential E. continuously changing into kinetic energy, and this into work, as suggested by poor "P. M.," who was so summarily treated by this terrible O'Toole that I quake in my shoes as I think of my fate.

The exception mentioned above is an extract from Clerk Maxwell, which is certainly erroneous, and from which Mr. O'Toole gets a good deal of fun. We will not suggest that the addition of a single word would make the passage correct, for we should be told that text-books ought to be perfect. But it is only just to mention that the error occurs in an explanation of the name; in the definition of the thing the error does not occur; nay, it is expressly contradicted.

After this is it not unkind to condemn those doctors who drop the name "potential E." and replace it with such phrases as "E. of repose," &c., implying that the energy in question is not due to motion? By-the-by where is the bull in "passive energy"? and what is the "action" that may be confounded with kinetic energy?

B.—*Potential E. as meaning "Energy related to Potential Functions."*

The word Potential may be used in a second sense. This of itself is a trouble to Mr. O'Toole; but—remembering that your readers may not sympathise with his undisguised antipathy to verbal skylarking—he hastens to add that the two meanings are not only heterogeneous but incompatible. "Surely there is no occasion to stop to prove this." Please do, Mr. O'Toole; we should like to hear you prove something.

It may be noted that in this opinion and in paragraph 9 he appears to differ from Thomson and Tait. (See their definition of Potential, *Nat. Phil.*, vol. i., § 485).

C.—*Potential E. as meaning "Energy of Potency"*

It appears from a foot-note that "potency" may mean a force. If so, it is strange that the O'Toole—who, throwing off his thin disguise, at the end of his letter undertakes the "duty" of a doctor, and tells us that potential E. should be the "energy of a force"—it is strange that Dr. O'Toole should object to the name on this ground.

But the remarks under this head are chiefly interesting, as indicating the *modus operandi* of our pseudo-Publius. He does not trouble to examine the definitions of "potential energy." He only looks for explanations of the word "potential." Finding scant material in the doctor's utterances, he resorts to his dictionary, hunts up the different meanings of "potential," adds to these their antitheses, and rends his phantoms to pieces. It is scarcely a parody upon his letter to say—we won't trouble about what a civil engineer is, but let us examine the meaning of *civil*. Now civil has —— meanings: (A.) polite, (B.), &c. Therefore "civil E." means "polite E.," and "civil E." used as a *distinguishing* title cannot mean anything else than this, that the other E. is unpolite E.

As to the whereabouts of Potential Energy.

"We shall now pass from the perplexities connected with this unlucky name, 'potential E.,' to consider the behaviour of our teachers towards the thing itself." At last Mr. O'Toole will deign to discuss the definitions given by the doctors. Nay, he wanders away into an examination of such rash—but perhaps not inexcusable—phrases as "the potential E. of a raised weight," &c. The proper remedy for the troubles arising on this point is "to use words discreetly and consistently." But this is not sufficiently heroic. A local habitation must be found for this "potential E.," although it would seem as vain to inquire into the whereabouts of potential E. as into the whereabouts of Mr. O'Toole's scientific erudition. It is proposed to lodge this E. in the forces, and perhaps it won't do much harm, as we don't know where the forces are. It is proposed, moreover, to substitute "energy of tension" for "potential E." This done, the doctor's millennium will have come. Never mind about altering your conception of this kind of energy; call it by another name; give it a *weisnichtwo* lodging. There will be no more "confusion about fundamental principles;" there will be no more slips of the pen or tongue; there will be no more puzzled Publii; and last, but not least, there will be no more O'Tooles to bother the doctors. Well may "verbal skylarking" be despised. What is it beside such gigantic fun as this?

And yet I am sceptical. We started by hearing that it was "principally—though not entirely—the doctors who were to blame for this confusion about fundamental principles." Is this proved? Is not another cause indicated in the letter of of "E. G." (vol. xvii. p. 9)? And shall the doctors expect to be rightly understood when Dr. O'Toole's amanuensis admits (vol. xvi. p. 520) that Dr. O'Toole himself has been misapprehended upon almost every point by one reader at least?

H. W. LLOYD TANNER

Cirencester, November 13

Smell and Hearing in Moths

IN NATURE (vol. xvii. p. 72) your correspondent "E. H. K." observes: "'J. C.' seems to draw inferences that moths have not the power of smell, but have that of hearing. I feel quite certain they possess the former, but am in doubt about the latter. . . .

"With reference to the sound of the glass, is it not the quick motion of the hand which disturbs the moth?"

May I draw the attention of both your correspondents to some experiments of mine on this subject which were published in NATURE about a year ago? These experiments, I remember, were quite sufficient to prove to me that moths have the power of hearing shrill notes; and, until I read the query of "E. H. K." above quoted, I thought that my account of these experiments must have been equally conclusive to any one who read them. On now referring to that account, however, I find that I there omitted to state one of the experiments which was resorted to for the purpose of avoiding the possible objection which "E. H. K." now advances. This experiment was a very simple one, consisting merely in making a sudden shrill whistle with my mouth by drawing the breath inwards, so as not to disturb the air in the neighbourhood of the insect. The latter, however, always responded to this as to other sounds in the way described, although throughout the experiment I took care not to move any part of my body.

GEORGE J. ROMANES

It was because of my knowledge of facts like those named by "E. H. K." that I was surprised at the apparent inability of moths to smell ammonia. Being no physiologist, I ventured to draw no inferences; but it occurred to me to wonder whether the sense of smell differs in kind with different organisations; whether, for instance, some substances strongly odorous to us may be quite inodorous to insects, and *vice versâ*.

As to the experiment on hearing, I do not think it was the movement of the hand which startled the moths. It may conconceivably have been the vibration of their wings set up by the sound; but the experiment can easily be repeated with variations by any one interested in the subject.

J. C.

Loughton

Meteorological Phenomenon

THIS morning at about a quarter before ten the sky here presented a most unusual appearance. The air was calm and the sun shining, but not brightly, through a slight veil of cirro-stratus. The sky was mostly covered with fibrous clouds of cirrus or cirro-stratus (I am not quite sure which I ought to call it), the fibres being quite parallel to each other, but in two different strata; those of one stratum were approximately from north-east to south-west, those of the other from north-west to south-east—so that they seemed to cross each other like the threads of a woven fabric. I think the fibres from north-east to south-west were the highest, but am not quite sure, though it seemed the same to another who was looking on with me.

JOSEPH JOHN MURPHY

Old Forge, Dunmurry, Co. Antrim, November 25

OUR ASTRONOMICAL COLUMN

STELLAR SYSTEMS.—M. Flammarion, in various notes communicated recently to the Paris Academy of Sciences, has been drawing attention to stars which appear to be affected with a common proper motion, or a motion similar in amount and in its direction. Several of his cases, however, are by no means to be styled "Nouveaux systèmes Stellaires." Thus the large and uniform proper motions of the southern stars ζ^1 and ζ^2 Reticuli, to which he refers in the *Comptes Rendus* of November 5, were the subject of remark in NATURE, vol. xi. p. 328. That there was a probability of a common proper motion in these stars would be evident to any one who inspected the columns in the British Association Catalogue, published in 1845, but as Taylor had not observed them, and the comparison was consequently dependent upon Lacaille and Brisbane only, there was a possibility of mistake. The first confirmation of the large proper motion of the B.A.C. in ζ^1 was afforded in Jacob's "mean places of 1440 stars"—from the Madras observations 1849-53, and

the earliest proof of a common translation in space was given by the same observer from the Madras observations 1853-58, which formed a part of vol. xxviii. of the *Memoirs* of the Royal Astronomical Society. Not having seen any distinct reference to the very large and uniform motions of these stars in astronomical treatises, we adverted to them in NATURE as above.

Again, the common proper motions of Regulus and Lalande 19749, mentioned by M. Flammarion in the same communication have been long remarked. The same may be said in the case of 9 and 10 Ursæ Majoris, one of the systems to which he refers in a paper presented to the Academy on November 12. Any one who has carefully utilised the very valuable fourteenth volume of the Dorpat observations must have been familiar with this case, and, we may add many similar ones, though the proper motions involved may be to smaller amount. This volume contains Mädler's laborious work upon 3222 of Bradley's stars, of which he gives positions reduced to 1850, and where all the catalogues available at the time and considered deserving of confidence were brought to bear. Not the least important feature in this work is the addition of two columns, not usually found in catalogues, containing the amount of secular proper motion in arc of great circle (r) and the angular direction of this motion (ϕ) counted from north round by east to 360°. On p. 155 we have—

For 9 ι Ursæ Majoris ... $r = 52''\cdot5$ ... $\phi = 238^\circ\cdot9$
„ 10 „ ... $r = 52''\cdot6$ ... $\phi = 238^\circ\cdot5$

But, as we have stated, other similar cases are readily detected by an inspection of these columns. For instance: in γ and 58 Tauri, distant 35′, where $r = 13''$, $\phi = 97^\circ$; in 66 and 68 Draconis, distant 43′, $r = 13''\cdot5$, ϕ about 69° and for wider stars, in 26 and 34 Pegasi, distant 4° 25′, where $r = 30''$, $\phi = 84^\circ$; in η and 10 Arietis, distant 5° 11′, $r = 15''\cdot5$, $\phi = 86^\circ$, with other neighbouring stars, moving in nearly the same direction, and again in μ and 54 Aquilæ, distant 5° 13′, $r = 27''$, $\phi = 121^\circ$. The list might be largely increased.

It is nevertheless to be expected that the researches which M. Flammarion is so industriously following up with respect to stellar systems may lead to a considerable addition to our knowledge of them, in cases which are not thus easily discovered from existing catalogues, particularly by determining the proper motions of stars, not yet submitted to such investigation.

THE MINOR PLANETS.—A letter from Prof. Watson, of Ann Arbor, U.S., to M. Yvon Villarceau, dated November 5, deranges the ordinal numbers of the small planets given in this column last week, from No. 175 onwards. It appears that on October 1 he discovered a planet 10m., which he duly notified by telegraph to the Smithsonian Institution, but by some unexplained circumstance the information was not transmitted by cable to the Observatory of Paris, as usual with such discoveries. Supposing this object to be really a *new* planet, it will be No. 175, and the subsequent discoveries mentioned last week will be on the same supposition, advanced a unit. Elements of No. 172 appear in *Astron. Nach.*, No. 2,176, and of No. 176 in the Paris *Bulletin International* of November 25.

THE CORDOBA OBSERVATORY.—Within the last few days, Mr. John M. Thome, the zealous co-operator with Dr. B. A. Gould in the important astronomical work carried on for several years past at the Observatory of the Argentine Republic, has visited this country on his return to Cordoba from the United States. We have seen in his hands proofs of the charts of the Argentine "Uranometria," which are on a much larger scale than those of Argelander, Heis, and Behrmann. They have been engraved in New York. This work is expected to be soon published; also large lunar photographs taken at Cordoba. All the stars in the "Uranometria" have been meridionally observed.

CARL VON LITTROW

CARL LUDWIG VON LITTROW, whose death has been announced during the past week, was born at Kasan on July 18, 1811. His father, Joseph Johann von Littrow, the eminent astronomer, afterwards Director of the Imperial Observatory at Vienna, was at that time Professor of Astronomy in the University of Kasan, where he founded an observatory. The son was educated under the father's direction, and in 1831 was appointed assistant at the Observatory at Vienna, of which institution the elder Littrow had taken the superintendence in 1819, removing thence from Ofen. In 1835 he first appeared as an astronomical writer, having in that year published an account of Hell's Journey to Wardoe and of his Observations of the Transit of Venus in 1769 at that place, from the original day-books; also a History of the Discovery of General Gravitation, by Newton, and Treatises upon Comets, more especially on Halley's, which was then appearing. In 1839 he published at Stutgard a Celestial Atlas, and a work which in the Catalogue of the Pulkova Library is called a Translation of Airy's "Populäre physische Astronomie," by which is most probably intended the well-known Treatise on Gravitation published by the Astronomer-Royal in 1834, though elsewhere Littrow's work is stated to refer to the history of Astronomy during the early part of the nineteenth century, presented to the British Association in 1832.

In 1842 Carl von Littrow succeeded his father as Director of the Observatory of Vienna, and the establishment has continued in vigorous activity under his charge. He has principally devoted the energies of the Observatory to equatorial astronomy, following up with diligence the observations of comets and planets, and with the aid of able assistants determining their orbits. Some of the most complete cometary discussions have emanated from the Observatory of Vienna while under his charge. The *Annalen der Sternwarte in Wien*, have been continued, and valuable astronomical work is contained in them, as for instance in the first volume of the third series, which appeared in 1851, where we have the positions of the stars in Argelander's Northern Zones reduced by Oeltzen to 1842, the epoch for which elements of reduction were given in the Bonn volume. Littrow was a frequent contributor to the publications of the Vienna Academy. In one of his memoirs—"Bahnähen zwischen den periodischen gestirnen des Sonnensystems," printed in the *Sitzungsberichte* of the Academy for January, 1854, he applied an original process of investigation of the points of nearest approach amongst the orbits of the small planets discovered up to that time, and the orbits of the periodical comets—a troublesome work in which mechanical aid was introduced; the result was the discovery of many close approximations of planets with planets, planets with comets, and of comets with comets; amongst the latter near approaches of Biela's comet to the orbit of Halley's in 35° and 198° heliocentric longitude. When interest was excited relative to the expected return of the comet of 1556, which at that period was supposed to have been previously observed in 1264, Littrow was the means of bringing to light an unknown treatise by Heller, which, with the chart of Fabricius, has allowed of a much improved determination of the orbit, and similarly he made known interesting particulars with reference to the remarkable observation by Steinheibel and Stark of a rapidly-moving black spot upon the sun's disc on February 12, 1820. Littrow was a constant contributor to the columns of the *Astronomische Nachrichten.* The names of Hornstein, Oeltzen, Weiss, Schulhof, and others are well known in connection with the work of the Vienna Observatory during Littrow's direction. His death occurred on the 16th inst.

Von Littrow's wife, Auguste Littrow-Bischoff, is one of the best known Austrian authoresses of the present time. The genial qualities of the astronomer and his wife made

them the centre of a large and admiring circle, and their residence was one of the most favourite gathering-places of the literary and scientific celebrities of Vienna.

BACTERIA[1]

IN a short paper communicated to the Royal Society at the close of last session, Prof. Tyndall did me the honour to criticise certain words reported to have been used by me at a meeting of the Association of Medical Officers of Health in January last. Although I am much indebted to him for the opportunity he has thus afforded me of discussing an important subject before this Society, I cannot refrain from expressing my regret that he should have thought it desirable to quote at length, and thus to place on permanent record in the Society's *Proceedings*, the expressions used on the occasion above mentioned. I regret this because these expressions occur in an abbreviated and incomplete abstract of a hastily prepared discourse not intended for publication.

As, however, I am well aware that Prof. Tyndall's purpose in his communication was not to criticise the language, but the erroneous views which the language appeared to him to contain, I shall make no further reference to the quotation; but shall regard it as the purpose of the present paper, first to reply to the reasoning embodied in his last communication, and secondly to corroborate certain statements previously made by me, to which he has taken exception in the more extended memoir published in the 166th volume of the *Philosophical Transactions.*

It will be my first object to enable the Fellows of the Royal Society to judge how far the views I entertain differ from those which have been enunciated here and elsewhere by Prof. Tyndall. Biologists are much indebted to him for the new and accurately observed facts with which he has enlarged the basis of our knowledge, as well as for the admirable methods of research with which he has made us acquainted. As regards the general bearing of these facts on the doctrine of Abiogenesis, I imagine that we are entirely agreed. So far as I can make out, the difference between us relates chiefly to two subjects, namely, the sense in which I have employed the words "germ" and "structure," and the extent of the knowledge at present possessed by physiologists as to the structure and attributes of the germinal particles of *Bacteria.*

Although Dr. Tyndall, in the title of his paper, refers to my "views of ferment," yet as he makes no further allusion to them, I will content myself with stating that in the passage quoted, the first sentence (from the words "In defining" to the word "living") has nothing to do with the following sentences, having been placed in the position which it occupies in the quotation by the abstractor. The paragraph ought to begin with the words "Ten years ago."

Of the meaning which attached itself to the word "germ" in the days of Panspermism a correct idea may be formed from the following passage from M. Pasteur's well-known memoir "Sur les Corps organisés qui existent dans l'Atmosphère." "There exist," says he, "in the air a variable number of corpuscles, of which the form and structure indicate that they are organised. Their dimensions increase from extremely small diameters to one-hundredth of a millim., 1·5 hundredth of a millim., or even more. Some are spherical, others ovoid. They have more or less marked contours. Many are translucent, but others are opaque, with granulations in their interior. . . . . I do not think it possible to affirm of one of these corpuscles that it is a spore, still less that it is the spore of a particular species of microphyte, or of another, that it is an egg or the egg of a particular microzoon. I confine myself to the declaration that the corpuscles are evidently organised; that they resemble in every respect the germs of the lower organisms, and differ from each other so much in volume and structure that they unquestionably belong to very numerous species." Such are the "germs" of M. Pasteur, and such is the conception of a germ which was entertained by informed persons up to 1870, and is very generally entertained up to the present moment.[1] It is obvious that these "corpuscules organisés" were, if they had any relation to *Bacteria*, not bacterium germs in Dr. Tyndall's sense, but "finished organisms," and yet it was of these that M. Pasteur said that it was "mathematically proved" that they were the originators of the organisms which are developed in albuminous liquids containing sugar, when exposed to the atmosphere.

With reference to the word "structure" I would point out that in the passage quoted from my lecture it is distinctly stated that the bacterial germ is endowed with structure in the molecular sense, but not in the anatomical sense. The meaning of the expression "anatomical structure" was, naturally, not defined, considering that the persons whom I was addressing might be supposed to be familiar with it. As, however, my failing to do so has apparently led to some uncertainty as to my meaning, I must, to avoid future misunderstandings, define more completely the difference between the two senses in which the word was used by me.

The anatomical sense of the word structure may be illustrated by referring to its synonyms, to the English words texture and tissue, to the Greek word *ἱστίον*, and to the German word *Gewebe*, from which two last the words in common use to designate the science of structure, viz., histology and *Gewebelehre* are made up. What I have asserted of the germinal particles of *Bacteria* is, that no evidence exists of their being endowed with that particular texture which forms the subject of the science of histology. In biological language there is a close relation between the words structure and organization, the one being an anatomical, the other a physiological term; either of these words signifies that an object to which it is applied consists of parts or structural elements, each of which is, or may be, an object of observation. As the observation is unaided or aided, the structure is said to be macroscopical or microscopical. The biologist cannot recognise ultra-microscopical structure or organisation except as matter of inference from observation, *i.e.*, from observing either that other organisms, which there is reason to regard as similar to the object in respect of which structure is inferred, actually possess visible structure, or that the object can be seen to possess structure at a later period of its existence. As instances in which the existence of structure is inferred the following may be mentioned:—The protoplasm of a Rhizopod is admitted to have structure because, although none can be seen in the protoplasm itself, the complicated form of the calcareous shell which the protoplasm makes or models can be seen. By analogy therefore other organisms which are allied to the Rhizopod are inferred to have structure, and from these, or from similar cases, the inference is extended to all kinds of cells, with respect to which it is taught by physiologists that although, in certain cases, no parts can be distinguished, the living material of which they consist is nevertheless endowed with structure or organisation. Similarly, we assume, that a *Bacterium* possesses a more complicated structure than we can actually observe, because in other organisms which are allied with it by form and life history, such complications can be seen. Again, in all embryonal organs we admit the existence of structure before it can be seen, because in the course of

[1] "Remarks on the Attributes of the Germinal Particles of *Bacteria*, in reply to Prof. Tyndall," by J. Burdon-Sanderson, M.D., LL.D., F.R.S. Paper read at the Royal Society, November 22.

[1] Before I became aware that the contaminating particles of water are ultra-microscopical I myself was engaged earnestly in hunting for germs both in water and air. The search has been continued by others up to a much later period. Those who desire information on the organised particles of the atmosphere will find the subject exhaustively treated by Dr. Douglas Cunningham in a report entitled "Microscopical Examinations of Air," lately issued by H.M. Indian Government.

development we observe its gradual emergence. So far, inference of the existence of structure from historical evidence is justifiable; but if we were to carry this inference back to the ovum itself, and say that the characteristic structures of nerve, of muscle, or of gland, exist in the ovum at the moment after impregnation, every physiologist would feel the assertion to be absurd.

In the familiar comparison of the origin of the elephant with that of the mouse, in which the perfect anatomical similarity of the ova in the two species is contrasted with the enormous difference of the result, we should be justified in saying that the difference of development is the expression of structural difference between the primordium of the one and the primordium of the other; but inasmuch as it is not possible to indicate any anatomical distinction, it is understood that structural difference of another kind is meant, namely, difference of molecular constitution. In other words, we assume that the potential difference between the one and the other is dependent on an actual difference of molecular structure. Whether this is accompanied with an anatomical difference, such as we might expect to be able to see if we had more perfect instruments, we do not know.

From the moment that it is understood that the word structure means anatomical structure, the argument used by Dr. Tyndall loses its relevance. After referring to the "germ limit," he says, "some of those particles" (by which, I presume, is meant atmospheric particles) "develop into globular *Bacteria*, some into rod-shaped *Bacteria*, some into long flexile filaments, some into impetuously moving organisms, and some into organisms without motion. One particle will emerge as a *Bacillus anthracis*, which produces deadly splenic fever; another will develop into a *Bacterium*, the spores of which are not to be microscopically distinguished from those of the former organism; and yet these undistinguishable spores are absolutely powerless to produce the disorder which *Bacillus anthracis* never fails to produce. It is not to be imagined that particles which, on development, emerge in organisms so different from each other, possess no structural differences. But if they possess structural differences they must possess the thing differentiated, viz., structure itself." Throughout this passage it is evident that it is not anatomical but molecular structure that is referred to.

In the other passages relating to the subject, I venture to think that Dr. Tyndall has overlooked the distinction made by me between anatomical organisation and molecular structure. When, for example, he speaks of "germ structure" in the passage quoted from his Liverpool Address, he evidently refers to molecular structure exclusively, for he gives ice as his first example, and argues that as ice possesses structure so do atmospheric germs—a proposition which I should not have thought of questioning.

The experimental evidence which we have before us goes to prove that in all the known cases in which *Bacteria* appear to originate *de novo*—that is to say in liquids which are at the moment of their origin absolutely free from living Bacteria—they really originate from "particles great or small," which particles are therefore germs in the sense in which that word is used by Prof. Tyndall. To illustrate the views I myself entertain, and always have entertained on this question, I need only refer to my paper on the origin of *Bacteria*, published in 1871. The experiments made by me at that time brought to light the then new fact, now become old by familiarity, that all exposed aqueous liquids, even when absolutely free from visible particles, and all moist surfaces, are contaminated and exhibit a power of communicating their contamination to other liquids. As regards water and aqueous liquids in general, I insisted on the "particulate" nature of the contaminating agent, and coined for the purpose the adjective I have just employed (which has been since adopted by other writers), at the same time pointing out that the particles in question were ultra-microscopical, and consequently that their existence was matter of inference as distinguished from direct observation. Dr. Tyndall has demonstrated by the experiments to which I have already alluded, that the ordinary air also contains germinal particles of ultra-microscopical minuteness. Of the completeness and conclusiveness of those experiments I have only to express the admiration which I, in common with all others whose studies have brought them into relation with the subject, entertain. That such particles exist there can be no question; but of their size, structural attributes, or mode of development, we know nothing.

Prof. Tyndall, I am sure by inadvertence, has accused me of assuming that there is some relation between the limit of microscopical visibility and what he calls the molecular limit, by which I presume to be meant the size of the largest molecule. Nothing that I have said or written could justify such a supposition. My contention is not that the particles in question are of any size which can be specified, but, on the contrary, that we are not in a position to form any conclusion as to their size, excepting that they are so small as to be beyond the reach of observation. Dr. Tyndall has taught us, first, that the optical effects observed when a beam of light passes through a particulate atmosphere are such as could only be produced by light-scattering particles of extreme minuteness; and, secondly, that by subsidence these particles disappear, and that the contaminating property of the atmosphere disappears with them. He has thus approximately determined for us the upper limit of magnitude, but leaves us uncertain as to the lower; for we have no evidence that the particles which render the atmosphere opalescent to the beam of the electric lamp may not be many times larger than those which render it germinative. Consequently, the fact that the air may be rendered sterile by subsidence, while affording the most conclusive proof that germinal matter is not gaseous, leaves us without information as to the size of the particles of which it consists.

Of each germinal particle, whether inhabiting an aqueous liquid or suspended in the atmosphere, it can be asserted that under conditions which occur so frequently that they may be spoken of as general (viz., moisture, a suitable temperature, and the presence of dead proteid matter, otherwise called organic impurity), it produces an organism. If, for the sake of clearness, we call the particle *a* and the organism to which it gives rise A, then what is known about the matter amounts to no more than this, that the existence of A was preceded by the existence of *a*. With respect to A we know, by direct observation, that it is an organic structure; but inasmuch as we know absolutely nothing as to the size and form of *a*, we cannot even state that it is transformed into A, much less can we say anything as to the process of transformation.

Considering that it is admitted on all hands that there exist in ordinary air particles which are potentially germs, it might at first sight appear needless to inquire whether or not this fact is to be regarded as carrying with it the admission that they must necessarily possess the other attributes of organised structure. Very little consideration, however, is requisite in order to become convinced that this question stands in relation with another of fundamental importance in biology—that, namely, of the molecular structure of living material.[1] It is not necessary for my present purpose to do more than to indicate the nature of this relation. As regards every form of living matter, it may be stated that, quite irrespectively of its morphological characteristics, which, as we have seen,

[1] The reader who is interested in this subject will find it discussed with great ingenuity by Prof. Pflüger, in his paper "Ueber die physiologische Verbrennung in den lebendigen Organismen," *Pflüger's Archiv*, vol. x. p. 300.

must be learnt by the application of the various methods of visual observation at our disposal, it possesses molecular structure peculiar to itself. We are certain of this, because the chemical processes of which life is made up are peculiar, that is, such as occur only in connection with living material. Even the simplest instance that we can mention, that of the elevation of dead albumin into living (a process which in the case now before us must represent the very earliest step in the climax of development) is at the present moment beyond the reach of investigation; for as yet we are only beginning to know something about the constitution of non-living proteids. But this want of knowledge of the nature of the difference between living and non-living material in no wise impairs the conviction which exists in our minds that the difference is one of molecular structure.

The sum of the preceding paragraphs may be stated in few words. Wherever those chemical processes go on, which we collectively designate as life, we are in the habit of assuming the existence of anatomical structure. The two things, however, although concomitant, are not the same; for while anatomical structure cannot come into existence without the simultaneous or antecedent existence of the kind of molecular structure which is peculiar to living material, the proof is at present wanting that the vital molecular structure may not precede the anatomical. At the same time it must be carefully borne in mind that there is no evidence of the contrary. It is sufficient for my purpose to have shown that the existence of organised particles endowed with anatomical structure in the "atmospheric dust" has not been proved. I do not dispute its probability.

Before leaving this subject I may be permitted to add a word as to the bearing of this discussion on a question which, to myself, is of special interest—that of *contagium vivum*. According to the view which these words are understood to express, the morbific material by which a contagious disease is communicated from a diseased to a healthy person consists of minute organisms, called "disease-germs." In order that any particle may be rightly termed a disease-germ two things must be proved concerning it, viz., first, that it is a living organism; secondly, that if it finds its way into the body of a healthy human being, or of an animal it will produce the disease of which it is the germ. Now there is only one disease affecting the higher animals in respect of which anything of this kind has been proved, and that is splenic fever of cattle. In other words, there is but one case in which the existence of a disease-germ has been established.

Comparing such a germ with the germinal particles we have been discussing, we see that there is but little analogy between them, for, first, the latter are not known to be organised; secondly, they have no power of producing disease; for it has been found by experiment that ordinary *Bacteria* may be introduced into the circulating blood of healthy animals in considerable quantities without producing any disturbance of health. So long as we ourselves are healthy, we have no reason to apprehend any danger from the morbific action of atmospheric dust, except in so far as it can be shown to have derived infectiveness from some particular source of miasma or contagium.

I now proceed to the second part of my communication, which relates to Prof. Tyndall's serious, but most courteously-expressed, criticisms of my experiments on spontaneous generation.[1]

The fact that Dr. Tyndall blames me for incautiously vouching for is, "that in boiled and hermetically-sealed flasks *Bacteria* sometimes appear in swarms." From multiplied experiments he concludes that this is not true, and infers that I who vouched for it was incautious. The paper referred to was one in which I, as a bystander, gave an account of certain experiments which Dr. Bastian performed in my presence. So far as relates to the fact above quoted, these experiments were, to my mind, absolutely conclusive; but inasmuch as I was unable to admit with Dr. Bastian that they afforded any proof of spontaneous generation, I followed them as soon as practicable by a series of experiments (NATURE, vol. viii. p. 141) (the only ones which I myself ever made on this subject), in which I tested the influence of two new conditions, viz., of prolonged exposure to the temperature of ebullition, and of exposure for short periods to temperatures above that of ebullition at ordinary pressure. The experiments accordingly consisted of two series, in the first of which a number of retorts or flasks charged with the turnip-cheese liquid, *i.e.* with neutralised infusion of turnip of the specific gravity 1017, to which a pinch of pounded cheese had been added, and sealed hermetically while boiling, were, after they had been so prepared, subjected to the temperature of ebullition for longer or shorter periods. In the second series the period of ebullition was the same in all cases, but the temperature was varied by varying the pressure at which ebullition took place.

The conclusion arrived at, as expressed in the final paragraph of the paper, was, that in the case of the turnip-cheese liquid, the proneness of the liquid to produce *Bacteria* can be diminished either by increasing the temperature employed to sterilise it, or if the ordinary temperature of ebullition be used, by prolonging its duration.

I did not think it necessary after 1873 to occupy myself further with the subject for two reasons, first, that I had accomplished my object, which was to show that as a ground for believing in spontaneous generation the turnip-cheese experiment was a failure; but secondly, and principally, because in the meantime the subject had been taken up by the most competent living observers, who had in every particular confirmed the accuracy of my results. I conclude this paper by referring shortly to some of these researches.

The first was made by P. Samuelson under the direction of Prof. Pflüger[1] in 1873. Its purpose was to ascertain whether it is true that certain liquids can be boiled for ten minutes without being sterilized, and secondly, to determine the influence of prolonged periods of exposure. The flasks employed were charged with the neutral turnip-cheese liquid, and sealed while boiling in the way already described. Some were subjected to the temperature of ebullition for ten minutes, the rest for an hour, the result being that whereas those heated for the longer periods remained without exception barren, an exposure of only ten minutes was followed, in the majority of cases, by an abundant development of *Bacteria*.[2] At about the same period a similar series of experiments was made under the direction of Prof. Hoppe-Seyler at Strasburg. The results were essentially the same.[3]

[1] The expressions referred to are the following:—"I have worked with infusions of precisely the same specific gravity as those employed by Dr. Bastian. This I was especially careful to do in relation to the experiments described and vouched for, I fear incautiously, by Dr. Burdon-Sanderson, in vol. vii. p. 180 of NATURE. It will there be seen that though failure attended some of his efforts, Dr. Bastian did satisfy Dr. Sanderson that in boiled and hermetically-sealed flasks *Bacteria* sometimes appear in swarms. With purely liquid infusions I have vainly sought to reproduce the evidence which convinced Dr Sanderson. . . . . I am therefore compelled to conclude that Dr. Sanderson has lent the authority of his name to results whose antecedents he had not sufficiently examined." *Phil. Trans.*, vol. clxvi. p. 57. In the abstract of a lecture delivered at the Royal Institution, January 21, 1876, similar words occur, as also in a letter to NATURE, dated February 27, 1876, in which Dr. Tyndall, after remarking that the experiments of Dr. Bastian, witnessed by me, were too scanty and too little in harmony with each other to bear an inference, suggests that I should repeat them.

[1] "Ueber Abiogenesis," von Paul Samuelson aus Königsberg, *Pflüger's Archiv*, vol. viii. p. 277. The paper is designated as a report of experiments made "im Auftrag und unter der Leitung des Geh.-Rath Prof. Pflüger." I refer in the text only to those experiments which were virtually repetitions of my own. The research actually extended over a wider field.

[2] "Als Resultat dieser Versuchsreihe, ergab sich eine massenhafte Entwickelung von Bacterien in den meisten nur 10 Minuten lang gekochten Flüssigkeitsmengen nach 3–4 Tagen" (*loc. cit.* p. 283).

[3] "Ueber die Abiogenesis Huizinga's," von Felix Putzeys aus Lüttich (aus dem chemisch-physiologischen Laboratorium des Herrn Prof. Hoppe-Seyler), *Pflüger's Archiv*, vol. ix. p. 391. In a note appended by Prof. Hoppe-Seyler to this paper he states that he has recommended its publica-

During the next year the second question which I had attempted to solve, viz., the influence of temperatures above 100° C., was taken up with much greater completeness by Prof. Gscheidlen, of Breslau.[1] After a *résumé* of the proofs already given by his predecessors, that certain fluids are not sterilised by boiling; and, secondly, that as means of sterilising such liquids the action of prolonged exposure and that of increased temperature may be regarded as complementary to each other, he proceeds to relate his own researches, the purpose of which was rather to fill up defects in the evidence than to establish new conclusions.

The flasks employed were capable of containing 100 cub. centims. (three and a half oz.); they were charged in the usual way with the turnip-cheese liquid, and exposed for short periods in chloride of calcium baths, of which the strengths were carefully adjusted so as to obtain the requisite temperatures. It was thereby definitely proved that whereas the germinal matter of *Bacteria* can stand a temperature of 100° for five or ten minutes it is destroyed by temperatures varying from 105° to 110°.[2]

In an appendix to my first paper, published in NATURE in the autumn of 1873, I showed that the solution of diffusible proteids and carbo-hydrates employed by Prof. Huizinga, of Groningen, in the first of the valuable series of experiments[3] published by him, relating to the subject of spontaneous generation, require a temperature above that of ebullition under ordinary pressure to sterilise them. This observation has since been established by Prof. Huizinga himself on the basis of very carefully made experiments,[4] by which he has proved at the same time that the liquids in question are rendered completely incapable of producing *Bacteria* without extrinsic contamination by exposing them to higher temperature. The only points of difference between us, either as regards method or result, are, first, that the sterilisation limit (Grenze zur Bacterienerzeugung) fixed by me was too low—the true limit being 110° C.—and secondly, that the experiments from which I had inferred that the liquids in question had been sterilised at lower temperatures than this were, in Prof. Huizinga's opinion, rendered inconclusive by the fact that my flasks were sealed hermetically, whereas in his exchange of air was allowed to take place during the period of incubation, through a septum of porous porcelain. To this last objection I might perhaps have thought it my duty to answer, had it not been shown by the subsequent researches of Gscheidlen to have no bearing on the question at issue. As regards the limit of sterilisation I can entertain no doubt as to the accuracy of Huizinga's measurements, and am quite willing to accept 108° C. as the lowest temperature which could be safely employed under the conditions laid down by him.

It will be understood that in bringing these facts before the Society my only purpose is to show, as I trust I have done conclusively, that the statements which Dr. Tyndall in 1876 characterised as incautious, and which he virtually invited me to retract, had been two years before confirmed in every particular by experimenters of acknowledged competence.

tion notwithstanding that the results obtained were mere confirmations of those of former observers; adding "für den wissenschaftlichen Fortschritt hat nicht die Priorität des einen oder des anderen Beobachters, wohl aber die Zahl, Mannigfaltigkeit, und Zuverlässigkeit der Beobachtungen eine hohe Wichtigkeit."

[1] "Ueber die Abiogenesis Huizinga's," von Richard Gscheidlen, *Pflüger's Archiv*, vol ix p 163

[2] "Es folgt aus den eben angegebenen Versuchen, nach meiner Meinung, dass in Huizinga's Gemengen die Bacterien einer Temperatur von 110° 5 10 Minuten lang zu widerstehen vermögen, nicht aber einer von 105°-110° in eingeschmolzenem Glasrohre während der nämlichen Zeit" (*loc. cit.* p. 167). Here the author clearly fails to make the necessary distinction between *Bacteria* (which, as is well known, lose their vitality at a much lower temperature) and the material out of which they spring. The mixtures referred to were either the cheese and turnip liquid or solutions containing peptones and grape sugar, to be immediately referred to. As affording an elegant demonstration that in the turnip-cheese liquid it is the cheese and not any other constituent which contains the resistant element, the following form of experiment is worthy of notice:—A tube A drawn out and closed at both ends is fused into the open mouth of a second tube B, of which the opposite end is drawn out and closed in a similar manner. In this way a compound tube is formed which is divided by a conical septum into two chambers A and B A small knob of glass having been previously introduced into the chamber B, the septum can be easily broken by shaking the tube. With tubes so prepared two experiments are made. In Experiment 1, compartment A is charged with infusion of cheese, sealed. and then exposed to a temperature of 110° before it is united to the compartment B. In like manner B is charged with neutral decoction of turnip, so that when the compound tube is complete it contains cheese in one compartment, turnip in the other. If, after boiling for ten minutes, it is placed in the warm chamber its contents remain barren. In Experiment 2 the experiment is varied by simply omitting the preliminary heating of A. The compound tube is boiled as before, but now its contents promptly give evidence that the conditions are present for an abundant development of *Bacteria*.

[3] Prof. Huizinga's papers on the Question of Abiogenesis are four in number. The references are as follows:—*Pflüger's Archiv*, vol. vii. p. 225, vol. viii. pp 180, 551; vol. x. p. 62.

[4] The solution employed in these experiments was neutral, and contained, in addition to the requisite inorganic salts, 2 per cent. of grape sugar, 0·3 per cent. of soluble starch, 0·3 per cent. of peptones, and 1 per cent. of ammonic tartrate. As in my experiments, the flasks were heated in a Papin's pot, of which the temperature was 102° C. Even after half an hour's exposure to this temperature all the flasks became in two or three days "stark trübe und voll Bacterien," third paper, p. 555, January, 1874.

DIFFUSION FIGURES IN LIQUIDS[1]

In making some experiments on the mixture of liquids entering into another liquid at the extremity of a tube of small diameter, a phenomenon presented itself which attracted my attention as both new and singular. A certain quantity of coloured alcohol, remaining in suspension in the centre of a body of water, assumed, by spreading gradually out, a form resembling that of a shrub having its trunk and its branches terminated by leaf-like expansions. I sought to reproduce the phenomenon, believing at first that this mode of diffusion was purely accidental; but the phenomenon always recurring very nearly in the same manner, I devised a mode of experimenting which enabled me to study it more advantageously.

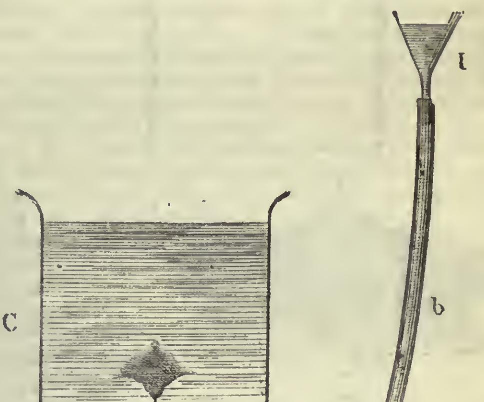


FIG. 1.—Apparatus of Prof. Martini.

C (Fig. 1) is a sort of cylindrical funnel of glass, to the neck of which is fitted a small capillary thermometrical tube T, about eight centimetres long. The capillary tube communicates by means of a caoutchouc tube *a b*, with a

[1] From an article in *La Nature* by Prof. Tito Martini, of Venice.

small funnel I, which may be raised or lowered at pleasure by means of its support. Pour into I a certain quantity of alcohol coloured say with a red solution of aniline. The liquid will traverse the capillary tube, from which it will flow unless prevented by compressing the india-rubber tube with a small pincers. This being done, fill with water the vessel C about three-fourths full; then by means of a funnel whose lower extremity reaches a little below the middle of the water, introduce a liquid denser than water, a concentrated solution of sea-salt or a thick syrup, until the vessel is filled up. Sulphuric acid may also be used, and in that case a less volume of liquid will suffice.

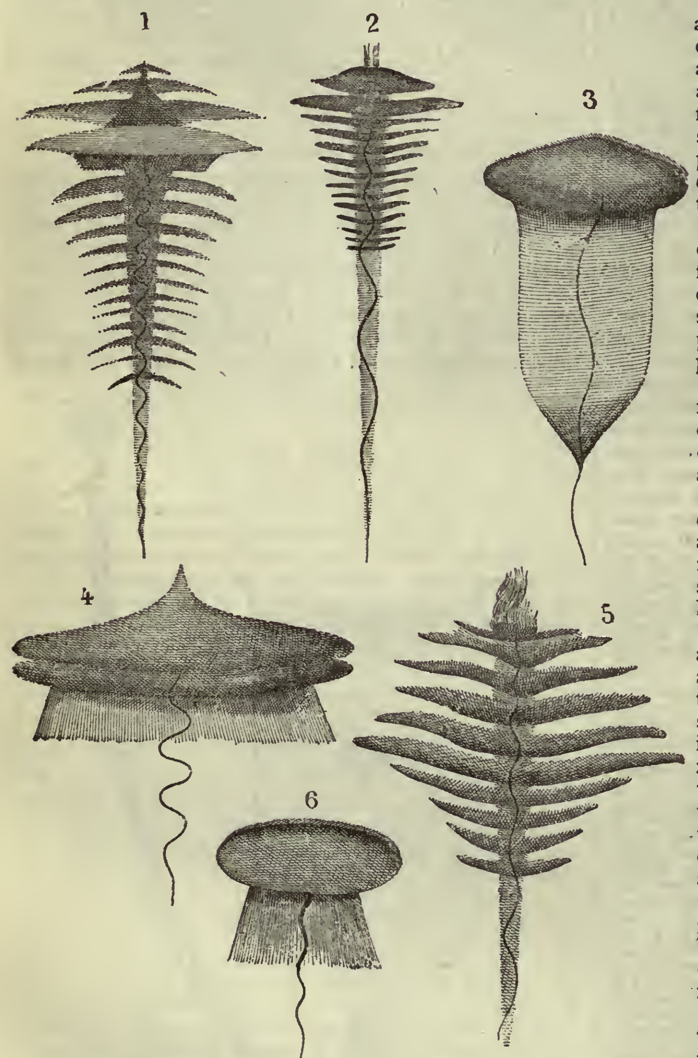


FIG. 2.—Experiments of Prof. Martini on the diffusion of coloured liquids in a sirupy liquid.

The liquid more dense than water will collect at the bottom of C; and there will thus be two layers of liquid superposed, the exact separation of which may be observed after being allowed to stand for an hour. If at the end of that time we raise the funnel I to a suitable height and relieve the pincers which compress the tube *a b*, the coloured alcohol which flows from the extremity of the capillary tube will enter the liquid in the vessel C, forming an ascending vein which usually has a spiral form. The alcoholic vein traverses the thickest layers of the liquid and is stopped at the boundary which separates the denser from the less dense part which floats above. At the point where the column of coloured alcohol is arrested, it will be seen to agglomerate into a mass at first formless; but, gradually, that mass elongates and extends, then is seen to throw out fluid threads in the form of foliage, sometimes similar to the petals of a flower, sometimes analogous to the leaves of a tree. After an hour the coloured alcohol has assumed a stable and regular figure. That figure varies in form with the liquids employed; it sometimes resembles a flower, sometimes a shrub, and sometimes it takes the form of a parasol of bright and vaporous colours, which add to its beauty.

The figure, so far as its form is concerned, attains its maximum of development three hours or more after the fluid vein begins to flow; but after that time the leafy expansions dilate more and more, and approach each other so as to form a mass of continuous layers, which remain suspended in the midst of the liquid. This happens even when the inflow has been arrested, either by applying the pincers to the india-rubber tube, or even by lowering suitably the funnel, I. It should also be remarked that around the vein of ascending liquid there very often forms a very fine tube, which assumes the aspect of the stalk of the flower, or rather the trunk of the liquid shrub; from different points of that stalk expansions in the form of leaves will be seen to proceed.

In order that the experiments I have devised may be successful, the tube through which the coloured liquid enters the vessel ought to be capillary, the flow ought to be gentle, and the apparatus maintained in a state of complete rest. It is necessary, moreover, to be careful first to expel the air from the india-rubber tube, since air-bubbles disturb the formation of the phenomenon. The following is a succinct *résumé* of some of the results I have obtained with different liquids:—

Colours of Aniline Solution.—I made use of aniline red, brown, green, and violet, dissolved in alcohol, being careful that the solution was not too concentrated. The forms obtained in sugared, salted, and acidulated water, are those represented in Fig. 2, Nos. 1 and 2. The figures obtained resemble, as will be seen, leaf-like expansions; the ramifications are turned downwards in sugared water (No. 1); in salt water, on the contrary, they are always raised, and at the commencement even more so than is shown in the figure. When acidulated water is used, the aniline colours are modified by the action of sulphuric acid; the green becomes pale yellow, the red becomes brown, and the violet acquires a beautiful green colour; but in all cases the shrub-like figure No. 2 is formed with perfect regularity.

Litmus. Aqueous Solution.—With this solution we obtain in acidulated water the figure represented in No. 3 (Fig. 2), which resembles a small parasol. Looked at from above, it has the aspect of a disc from the periphery of which proceed many equidistant rays very close to each other. In the salt water the same aqueous solution gives a different figure. In general, when aqueous solutions are employed to form the figures a space of time is required longer than that which is necessary in the case of alcoholic solutions.

Alcoholic Solution.—With this solution there are formed in salt or sugared water, figures analogous to Nos. 1 and 2; in acidulated water there is produced a shrubby appearance similar to No. 2.

Lake.—The aqueous solution of lake forms in salt water a figure similar to that of No. 4; in acidulated

water Fig. 3 is produced, but more delicate and more regular than that obtained with litmus.

Azure Blue.—The aqueous and alcoholic solutions of azure-blue or pearl form figures similar to those already described. In acidulated water we obtain a very regular spheroidal nucleus of a very dark blue, surrounded by a spheroidal layer with an inferior stem (No. 6).

Cochineal.—The aqueous solution forms in acidulated water the figure No. 3, regular, like that of litmus and of lake. In salt water, cochineal, not being soluble, is precipitated and the phenomenon is not produced.

Iodine.—The alcoholic tincture of iodine forms, in sugared, salt, or acidulated water, beautiful figures almost identical with those of the colours of the aniline solution.

Bichromate of Potash.—To make the experiments with bichromate of potash succeed I changed the arrangement of the experiment on account of the very great density of the solution in comparison with the density of water. I fill the vessel in the usual manner, then I place above the vessel a small funnel, fitted with a capillary tube which partly enters the liquid. The aqueous solution of bichromate of potash being poured into the small funnel, flows out, forming a small descending spiral, which usually is arrested in the division between the more and less dense parts of the liquid. In acidulated or salt water two very beautiful figures are formed resembling those of Nos. 2 and 5, but reversed.

The various experiments described above have been repeated several times for each colour, and I have always obtained the same results. This persistence of form shows that the phenomenon is regulated by a law which I shall seek to discover. I believe I may conclude from these first attempts that the form of the figure depends on the liquid in which the colour is dissolved, more than on the colour itself. By employing other acids and other salts, not such, however, as precipitate the colour, it is probable that other figures would be obtained.

TRACES OF EARLY MAN IN JAPAN

SO much interest is felt in the origin of the Japanese, that any information regarding earlier races in Japan will interest the readers of NATURE.

The discovery and examination of a genuine kjockkenmoedding, or shell heap, enables me to give positive evidences regarding a prehistoric race who occupied this island. Whether autocthonous or not it would of course be impossible to say. On my first ride to Tokio, in June of this year, I observed, from the car window, near a station called Omori, a fine section of a shell heap, which was recognised as such at once, from its resemblance to those I had often studied along the coast of New England. On September 16, accompanied by Messrs. Matsumura, Matsura, and Sasaki, three intelligent Japanese students, I made an examination of it, and a few days afterwards, in company with Dr. David Murray, Superintendent of Public Instruction, and Mr. Vukuyo, with two coolies to do the heavy digging, made an exhaustive exploration of it.

The deposit is composed of shells of various genera, such as Vusus, Eburna, Turbo, Pyrula, Arca, Pecten, Cardium, two strongly marked species of Ostrea, and curiously enough, *Mya arenaria*, not to be distinguished from the New England form, as well as other genera. These shells, so far as I know, still live in the Bay of Yedo. The heap is about 200 feet wide, and varies from a foot to five or six feet in thickness, with a deposit of earth above, at least three feet in thickness. It is now nearly half a mile from the shore of the Bay, though in accordance with the usual position of these heaps in other parts of the world, it must have been formed near the shore, and this fact indicates a considerable elevation of the land since the deposits were made. I may add that other evidences of a geological nature indicate a wide-spread upheaval in past times.

The peculiarities of the typical shell-heap, such as fragments of bones, rough implements worked out of horn, and pieces of pottery, are all here. The heap, however, is marked by certain features which render it peculiar.

First, the immense quantity of pottery and its diversity of ornamentation, some of it extremely ornate, but very rude.

Second, the absence of bone-implements, the few found—eight or ten in number—being of horn, with the exception of an arrow-head of diminutive proportions, made of the tusk of a wild boar. All the implements are very simple; two of them are like blunt bone awls, with the end very obtuse, and a constriction worked around the end. Another one is made from the natural termination of a deer's antler. A few fragments of horn were found which had been cut off at the ends.

Third, the entire absence of flint flakes, or stone implements of any kind, if we except a small stone adze found near the top of the heap, and made out of a soft sandstone. The frequent occurrence of isolated tusks of the wild boar would seem to indicate that these teeth were used for implements, and one piece of antler, having a hole in the end, is worked in the form of a rude handle. By far the most common bones found were those of the deer and wild boar, and curiously enough Steenstrup shows the same proportion in the Danish shell heaps. No human bones have yet been found.

An analysis of the red pigment found on some of the pottery shows it to be cinnabar. With its removal from the shore, its elevation above the level of the sea, the absence of stone implements, and the great thickness of the earth deposits above, we have reasons for believing that the deposit is of high antiquity.

Through the intelligent interest manifested by Mr. Kato and Mr. Hamao, Director and Vice-Director of the Imperial University of Tokio, every facility for a thorough investigation of these deposits will be given me.

Tokio, Japan, September 21 EDWARD S. MORSE

NOTES

IT is proposed to hold the next annual meeting of the Association for the Improvement of Geometrical Teaching (under the presidency of Dr. Hirst) at University College, Gower Street, on January 11, 1878, at 10.30 A.M. Four resolutions are to be submitted to the Association:—1. That in the opinion of this Association it is both reasonable and expedient that candidates at all examinations in elementary geometry should be required to give evidence of such ability as is necessary for the solving of easy geometrical exercises; and that the secretaries of the Association be instructed to send a copy of this resolution to the leading examining bodies of the country. The other resolutions relate to the proposed formation of sub-committees for drawing up a syllabus of (1) Solid Geometry, (2) Higher Plane Geometry (Transversals, Projection, &c.), (3) Geometrical Conics. It may be in the recollection of our readers that the report of the British Association Committee (in 1876, published at the time in NATURE) was highly favourable to the work of this Association.

THE dissection of the Berlin gorilla was performed last week by Prof. Virchow and Prof. Hartmann in the presence of several prominent Berlin physicians, and it was ascertained that the sudden death of the animal was caused by acute inflammation of the bowels, the same disease which carries off young children so rapidly. The dissection explains the cause of his previous illnesses and supplies valuable information with regard to the treatment of anthropoidal apes. The button of a glove, iron wire, and pins were found in Pongo's stomach.

DURING the past week the Emperor of Germany received a deputation of the members of the German Expedition for observing the transit of Venus, who presented him with a handsomely-mounted album containing copies of all the photographs taken during the transit.

BERN celebrates on December 12 the 100th anniversary of the death of its famous citizen, Albert Haller, who was equally renowned as physiologist, botanist, and poet.

THE New York *Nation* informs us that news has been received of the death of the Rev. James Orton, professor of natural history at Vassar College, and well known as the author of "Comparative Zoology" and "The Andes and the Amazons." Prof. Orton made his first expedition to South America in 1867, crossing the Andes eastward from Peru, and descending the Napo to the Marañon. His second expedition in 1873 was the reverse of the former one, beginning with the ascent of the Amazon. He was on his way home from a third expedition when he died, September 25, on board a small schooner on Lake Titicaca. He was greatly esteemed by all who knew him.

THE *New York Tribune* states that Mr. Edison, the inventor of many improvements in telegraphy, is hard at work in the endeavour to make the telephone record the sounds it transmits. His apparatus at present consists chiefly of a steel point attached to the disk of a telephone and pressing lightly on a strip of paper passed beneath the point at a uniform rate. The vibrations of the disk are thus recorded, and can be translated. Mr. Edison has already achieved some success in this attempt, but as yet finds difficulty with the more delicate vibrations. The invention suggests an ultimate possibility of recording a speech at a distance, verbatim, without the need of shorthand.

NOT one of the designs sent in in competition for the monument to Spinoza at the Hague has satisfied the judges. A new term for receiving designs will therefore be fixed.

ANOTHER letter from Mr. Stanley appears in the *Telegraph* of Thursday last, in which he gives many interesting details of his journey down the Lualaba-Congo, but does not add essentially to what we already know from previous letters. It will be well at present to rest satisfied with the fact that he has solved a great geographical problem; discussion will be appropriate and to some purpose when we are in possession of the full details. In the December number of Petermann's *Mittheilungen* that keen geographer discusses the bearings of Stanley's discovery, and on the basis of the earlier letters identifies the Lualaba-Congo with the discoveries of Browne, Barth, Nachtigal, and Schweinfurth; but on the map which accompanies the paper he carries the great river north to about 4° N. lat. In a postscript on Stanley's own map Dr. Petermann seems to think that his identifications may require modification. Dr. Petermann cannot find terms strong enough in which to speak of the merit of Stanley's work. He calls him "the Bismarck of African exploration," who has united the *disjecta membra* of previous explorations as Bismarck has made one great empire out of a number of isolated states. He is evidently inclined to place Stanley alongside of Columbus.

THE December number of Petermann's *Mittheilungen* contains a long paper on the Iquique earthquake of May 9 last, in which much valuable data are given on the earthquake and on the wave which was simultaneous with it over so wide a stretch of the Pacific Ocean.

THE *Daily News* correspondent at Rome writes that no news has arrived there as to the death of the African explorer, the Marquess Antinori, the inference being that he is still alive. A long letter has been received by the Italian Geographical Society from Signor Matteucci who, with Signor Gessi, is bound for Inner Africa; the two expect to be in Khartoum in the beginning of December. They were splendidly equipped before leaving Italy.

DR. SCHWEINFURTH, the celebrated African traveller, who has been staying at Berlin since the beginning of August, will shortly return to Africa, as he finds that the European climate no longer agrees with his health. At present he has left Berlin for Weimar.

AT the Geographical Society, on Monday night, Commander Musters, R.N., read a paper on Bolivia, in which he gave much valuable information about a country, its products and its people, about which we are extremely ignorant. Commander Musters lived in the country for a considerable time. Mr. Clements R. Markham read a paper on the still unexplored parts of South America. The facts is we are almost as ignorant of Central South America as, until recently, we were of Central Africa, and there is here a practically virgin field for a second Stanley, if not indeed for Stanley himself.

IN a recent number we referred to the preparations which are being made for Prof. Nordenskjöld's expedition to the Arctic regions next summer. The *Handels och Sjöfarts Tidning* of Gothenburg publishes further details, giving the plan of the expedition as presented to the King of Sweden by Prof. Nordenskjöld. We now learn that the steamer *Vega* is being fitted up at the royal wharves of Carlskrona, and will take provisions for two years. The Professor intends to leave at the beginning of July next, and his staff will consist of four scientific men besides himself, four Norwegian sailors who are well acquainted with the Arctic Sea, a ship's officer, eighteen marines, and a ship's doctor. The first halt will be made at the mouth of the Yenisei River; then the expedition will proceed to Cape Tscheljuskin, and try to penetrate as far as possible in a north-easterly direction.

MR. G. J. HINDE, of Toronto, Canada, writes us that a shock of earthquake, unusually severe for that part of the world, occurred along the valleys of the St. Lawrence and Ottawa Rivers, Lakes Champlain and St. George, and through New Hampshire, Vermont, and Western Massachusetts, at or near 2 A.M. of Sunday, the 4th instant. The limits along which it has been noticed are Pembroke on the Upper Ottawa to the north-west, Montreal on the east, Boston and Providence to the south-east, and Toronto to the west. The shock appears to have been most severe on the line of the Ottowa valley between Pembroke and Montreal, and between Ottawa city and Cape Vincent on the St. Lawrence, following in a general direction the outcrops of the Laurentian range. It was but very slightly felt at Toronto, but at Montreal the shocks are stated to have lasted twenty seconds, and to have shaken movable articles about the rooms.

THE following grants in aid of researches have been made this year by the Committee of Council on the report of the Scientific Grants' Committee of the British Medical Association:—Mr. Gaskell, in aid of a research on the reflex action of the vascular system and muscles and reflex vasomotor action generally, 30*l.*; Mr. Langley, in aid of a research on the changes produced in the salivary glands by nerve influence, 25*l.*; Dr. Rutherford, F.R.S., for a continued research on the action of Cholagogues, 50*l.*; Drs. Braidwood and Vacher, for engravings for illustrating the third report on the life history of contagium, 40*l.*; Mr. Pye in aid of a continued research for the investigation of the relation that the retinal circulation bears to that of the brain, 8*l.* 15*s.*; Mr. Bruce Clarke, in aid of a continued research on syncope and shock, 10*l.*; Mr. A. S. Lee, Heidelberg, in aid of a research on the quantitative determination of digestive products obtained by the action of pancreatic ferment upon the various albumens, 25*l.*; Dr. McKendrick, Glasgow, in aid of a continued research into the antagonism of drugs, 30*l.*; Dr. McKendrick, Glasgow, in aid of an investigation into the dialysis of

blood (renewed), 10*l.*; Dr. John Barlow, Muirhead Demonstrator of Physiology, Glasgow, in aid of an experimental investigation into the changes produced in the blood-vessels by alcohol, 10*l.*; Dr. Joseph Coats, Dr. McKendrick, and Mr. Ramsay, the committee upon the investigation of anæsthetics, 50*l.*; Dr. McKenzie, a research on pyæmia, 25*l.*; Mr. Callender, F.R.S., Dr. J. Burdon Sanderson, F.R.S., Dr. T. Lauder Brunton, F.R.S., and Mr. Ernest Hart, the committee appointed for the investigation of the pathology and treatment of hydrophobia, 100*l.* Total, 413*l.* 15*s.*

TELEGRAPH warnings are to be employed all over Paris for giving alarms of fires to all the fire-engine stations. The alarm is given by breaking a small pane of glass facing the streets, being a variation of the system employed on railways for signalling the engine-driver or guard.

IN the November session of the Berlin Geographical Society, Baron v. Richthofen was re-elected president. The evening was chiefly occupied by an address from Dr. Nachtigal, on the results of Stanley's lately accomplished expedition, which he regarded as the most prominent event among ater African explorations. Prof. Orth gave a short description of a new method of cartography.

LIEUT. DE SEMELLÉ has intimated to the Paris Geographical Society that he intends to cross Africa from west to east, ascending the Niger and Binué, making for Lakes Albert and Victoria, and reaching the east coast at Mombasa or Malinda. He states that he has already obtained sufficient resources.

THE chemists of Berlin have been occupied lately in analysing the wares of the wine merchants, and no little excitement has been caused by the discovery that the entire stock of one of the largest houses dealing in wines for medicinal purposes, consisted entirely of artificially prepared mixtures of spirit and sugar solutions, flavoured with various herbs.

AT Leipzig a "General German Anti-Adulteration Society" has been formed, which has for its main object the prevention of the adulteration of food. A periodical is to appear, or has already appeared, as the organ of this society. At some fifty other German towns branch societies are being established. All political or religious matters are excluded from the programme of the society, while one of its statutes prescribes the special prosecution of the makers and sellers of so-called secret remedies and medicines.

IN evidence of the interest now being taken by Spain in scientific subjects we may draw attention to the *Boletin de la Institucion libre de Enseñanza* (Madrid, 1877), the first five numbers of which, from March 7 to June 17, now lie before us. We notice Geometria y morfologia natural, Prof. De Linares; Investigacion de los propiedades ópticas, Prof. Calderon; La religion de los Celtas españoles, Prof. Costa; Principios y Definiciones de la Geometria, Prof. Jimenez; Precipitacion de los metales puros por los sulfuros naturales, Prof. Quiroga. There are accounts of papers read at meetings under the headings "Resúmenes de Enseñanzas," and "Conferencias." The *Boletin* is in shape not quite so large as NATURE, and each number contains four pages.

THE Minister of Instruction in the cabinet chosen by Marshal MacMahon last week is M. A. E. A. Faye, the well-known astronomer, who is spoken of as Leverrier's probable successor. M. Faye is at present in his sixty-third year, and is chiefly known through his discovery of the comet named after him, in 1843. Since that time he has devoted his attention principally to the consideration of the problems of physical astronomy, the solar constitution, &c. His most important works are "Leçons de Cosmographie," 1852; and a translation of Humboldt's "Cosmos." M. Faye is probably the best known in what is ironically termed the *cabinet des inconnus.* French politics allure an unusually large number of scientific men. Naquet, the chemist, is now a leader of the radical wing of the Republican party, Dumas and Scheurer-Kestner are life members of the senate, and Wurtz was proposed as a candidate for the senate a few weeks since.

THE communication of the city of Moscow with the river Volga, leaving the railway out of account, was, up to the present, only possible in the spring of each year, on account of the shallowness of the Moskwa River. The boats were drawn by horses from Moscow to Kolomna on the river Oka, which falls into the Volga at Nishni-Novgorod, and this means of communication, on account of the great time it occupied, not to mention its cost, was a very imperfect one. A series of locks has recently been constructed on the Moskwa River, and tug steamers are now running between the capital and the Oka.

WE have already referred to the proposed introduction of the telephone into the German telegraphic service. Dr. Stephan, the enterprising Postmaster-General of the German empire, who has brought the German postal service to such efficiency, and fairly created the present international telegraphic system, appears to have definitely settled the question of the practicability of the general introduction of the new method. For the past few weeks the telephone has been in constant use between the General Post Office and the General Telegraph Office in Berlin, and has superseded the telegraphic communication between Berlin and some of the neighbouring villages. The results have been so satisfactory that a few days since a consultation of leading telegraphic officials was held to make arrangements for the establishment of a large number of telephonic stations. Since the equipment of these stations is so inexpensive, and the long and costly preliminary training of a telegrapher is avoided, it can easily be understood with what readiness the new invention is put into practical use. Interesting in this connection is the recent adoption of the telephone by Prince Bismarck. He has caused, as we stated last week, the establishment of a telephonic means of communication between the Chancellor's office in Berlin and his country residence at Varzin, in Pomerania, 230 miles distant; and finds that he is perfectly able to give instructions and receive reports without leaving his favourite castle. No subterranean wires, but the ordinary telegraphic wires on poles, are used for this purpose.

A SERIES of researches on the compressibility of liquids has recently been described by M. Amagat in the *Annales de Chimie et de Physique.* Among other results, the compressibility is found to be far from depending on the volatility of liquids, as might be supposed. The presence of sulphur, chlorine, bromine, and probably also iodine, tends to diminish the compressibility (a fact sufficiently explained by the corresponding increase of density). With regard to alcohols, the compressibility diminishes from the first member of the series, methylic alcohol, at least at 100°. At 14° common and methylic alcohol have nearly the same compressibility; and at zero the common alcohol is perhaps more compressible than methylic alcohol. Of the ethers, ethyl-acetic ether is more compressible at 14° and at 100° than methyl-acetic ether (an inverse order to that of the densities, which decrease as you rise in the series. With regard to hydrocarbons, the compressibility decreases regularly both at ordinary temperature and at 100° as you descend in the series.

A MICROSCOPICAL study has recently been made by M. Prillieux, of a disease of fruits, and especially of pears, which consists in the appearance of spots, then of crevices, issuing in complete disorganisation. From the facts described, it appears that the cause of this evil is a fungus, the spores of which are developed on the skin of the fruit with the appearance of a thin filament. At a certain time this filament penetrates the epider-

mis and produces a mycelium, which develops in the very mass of the fleshy tissue. Later there appear, in addition, fructiferous filaments, which bear about twenty-five spores each. The cells of the fruit, on passage of the parasite, are destroyed, and it is thus that the crevices are formed.

THE diffusion which takes place between two gases separated from each other by an absorbent film (*e.g.*, a soap film) was studied a short time ago by Prof. Exner, of the Vienna Academy. He has recently extended his inquiry to the case of vapours from easily volatile liquids, using the same apparatus as for permanent gases. The experiments were made with sulphide of carbon, chloroform, sulphuric ether, benzine, alcohol, and oil of turpentine, and they show that the diffusion from such vapours follows the same laws as those of gases, *i.e.*, that it depends both on the coefficient of absorption of the film and on the density of the gas being directly proportional to the former, and inversely proportional to the square root of the latter. Thus it appears that the greater or less distance of a gas from its liquefaction point has at least no influence on this kind of diffusion.

IT is reported that Herr Josef Albert, the eminent Munich photographer, has made the highly important invention of photographing the natural colours of objects by means of a combination of the ordinary photographic process with a photographic printing press constructed by the same gentleman some time ago. The images are stated to be so perfect that not the least improvement with the brush is required, as the finest shades of colours are faithfully reproduced. The secret of the invention is said to be based on the separation of white light into yellow, blue, and red rays, and in the artificial application of the same colours in the printing press. The first negative is taken upon a plate which is chemically prepared in such a manner that it only receives the yellow tints or shades of the object; this is then passed through the printing press, the roller of which is impregnated with a yellow colouring matter. On the print only the yellow tints reappear more or less distinctly; the object is then again photographed, and this time a negative is prepared, which only receives the blue shades and tints; a second printing press has its roller impregnated with some blue colour, and the plate of course gives a print with only the blue tints reproduced. In the same manner a third print is obtained which only shows the red shades and tints. The final manipulation now consists in printing the three images upon the same plate, when the three colours intermingle and the natural colours and shades of the objects are obtained. We need hardly point out the enormous importance of this invention.

A PAMPHLET just published by the Director of the Paris National Library contains some interesting statistical data respecting one of the finest libraries in the world. It has been found that the library contains 86,774 volumes on catholic theology, 44,692 volumes on the science of languages, 289,402 volumes on law, 68,483 volumes on medicine, 441,836 volumes on French history, and 155,672 volumes of poetry. The works on natural science are not yet catalogued. During 1876 the library received no less than 45,300 *French* additions and purchased 4,565 foreign books.

THE additions to the Zoological Society's Gardens during the past week include two Black-eared Marmosets (*Hapale penicillata*) from South America, presented by Miss Quain; a Black-backed Jackal (*Canis mesomeles*) from South Africa, presented by Capt. Fulton, s.s. *Taymouth Castle*; a Common Boa (*Boa constrictor*) from South America, presented by Miss Alice Leith; a Brown Tree Kangaroo (*Dendrolagus inustus*) from New Guinea, a Slow Loris (*Nycticebus tardigradus*) from Malacca, a River Jack Viper (*Vipera rhinoceros*) from West Africa, purchased; a Green Monkey (*Cercopithecus callitrichus*) from West Africa, deposited.

THE LIBERTY OF SCIENCE IN THE MODERN STATE [1]

II.

IT is easy to say: "A cell consists of small particles, and these we call plastidules; plastidules, however, are composed of carbon, hydrogen, oxygen, and nitrogen, and are endowed with a special soul; this soul is the product or the sum of the forces which the chemical atoms possess." Indeed this is possible; I cannot judge of it exactly. This is one of those points which are yet unapproachable for me; I feel there like a navigator who gets upon a shallow, the extent of which he cannot guess. But yet I must say that before the properties of carbon, hydrogen, oxygen, and nitrogen are defined to me in such a manner that I can understand how, through their combination a soul results, I cannot admit that we are justified in introducing the plastidule soul into the educational programme or to ask generally of every educated man that he should recognise it as a scientific truth to such a degree as to operate with it logically, and to base his conception of the universe upon it. This we may really not ask. On the contrary, I think that before we designate such theses as the expression of science, before we say this is modern science, we ought first of all to complete a whole series of lengthy investigations. *We must therefore say to the schoolmasters, do not teach this.* This, gentlemen, is the resignation which in my opinion, those ought to exercise who deem such a solution in itself to be the probable end of scientific investigation. We can certainly not differ on that point for a moment, that if this doctrine of the soul were really true it could only be confirmed by a long series of scientific investigations.

There is a series of events in the field of the natural sciences, by which we can show, for how long certain problems are in suspense, before it is possible to find their true solution. If this solution is found at last, and found in a direction of which there was a presentiment perhaps centuries ago, it does not follow that during those times which were occupied only by speculation or presentiment the problem might have been taught as a scientific fact.

Prof. Klebs spoke of *contagium animatum* the other day, *i.e.* the idea that in diseases the transmission takes place by means of living organisms, and that these organisms are the causes of contagious diseases. The doctrine of *contagium animatum* loses itself in the obscurity of the middle ages. We have had this name handed down to us by our forefathers, and it is very prominent in the sixteenth century. Certain works of that period exist, which put down *contagium animatum* as a scientific dogma with the same confidence, with the same kind of justication, as nowadays the plastidule soul is set up. Nevertheless the living causes of diseases could not be found for a long time. The sixteenth century could not find them, nor could the seventeenth and the eighteenth. In the nineteenth century we have begun to find some *contagia animata* bit by bit. Zoology and botany have both contributed to them: we have found animals and plants which represent contagia, and a special part of the knowledge of contagia has been absorbed into zoology and botany, quite in the sense of the theories of the sixteenth century. But you will already have seen from the address of Prof. Klebs that the end of proofs has not yet ended. However much we may be disposed to admit the general validity of the old doctrine, now that a series of new living contagia have been found, now that we know cattle disease and diphtheria to be diseases which are caused by special organisms, still we may not yet say that now *all* contagia or even all infectious diseases are caused by living organisms. After it has appeared that a doctrine, which was formulated already in the sixteenth century, and which has since obstinately emerged again and again in the ideas of men, has at last, since the second decade of the present century, obtained more and more positive proofs for its correctness, we might really think that now it was our duty to infer, in the sense of an inductive extension of our knowledge, that all contagia and miasmata are living organisms. Indeed, gentlemen, I will admit that this conception is an extremely probable one. Even those investigators, who have not yet gone so far as to regard the contagia and miasmata as living beings have yet always said that they resemble living beings very closely, that they have properties which we otherwise know in living beings only, that they propagate their kind, that they increase

[1] Address delivered at the Munich meeting of the German Association, by Prof. Rudolf Virchow, of Berlin. Continued from p. 74.

and are regenerated under special circumstances, that, indeed, they appear like real organic bodies,—these men, nevertheless, have waited, and rightly, until the proof of their being living organisms was furnished. And thus caution commands reserve even now.

We must not forget that the history of science presents a number of facts which teach us that very similar phenomena may happen in a very different manner. When fermentation was reduced to the presence of certain fungi, when it was known that its beginning was closely connected with the development of certain species of fungi, then it was really very obvious to imagine that all processes related to fermentation happen in the same way; I mean all those processes which are comprised under the name of "catalytic," and which occur so frequently in the human and animal body as well as in plants. There were, indeed, some scientific men who imagined that digestion, which is one of the processes which closely resemble the fermentative ones, was brought about by certain fungi which occur frequently (in the special case of cattle the question has been practically discussed), and which were supposed to cause digestion in the stomach in the same way as the fermentation fungi cause fermentation elsewhere. We now know that the digestive juices have absolutely nothing to do with fungi. Much as they may possess catalytic properties, we are yet certain that their active substances are chemical bodies which we can extract from them, which we can isolate from their other component parts, and which we can cause to act in the isolated state free from any admixture of living organisms. If the human saliva has the property of being able to change starch and dextrine into sugar in the shortest time, and if every time we eat bread this new formation of "sweet" bread takes place in our mouth, then no fungus takes part in this nor any fermentation organism, but there are chemical substances which, much in the same way as it happens in the interior of the fungus, bring about chemical change in matter. We see, therefore, that two processes which are extremely similar, the one in the interior of the fermentation fungus and the other in the process of human digestion, are brought about in different ways; the same process in the one instance is connected with a certain vegetable organism, while in the other it takes place without any such organism and simply through a liquid.

I should consider it a great misfortune if we were not to continue in the same way as I have done now, to examine in each single case whether the *supposition* which we make, the *idea* which we have formed and which may be highly probable, is really true, whether it is justified *by facts*. With regard to this I would remind you that there are cases also amongst the infectious diseases where most undoubtedly a similar contrast exists. My friend, Prof. Klebs, will no doubt pardon me if I, even now, in spite of the recent progress which the doctrine of infecting fungi has made, still remain in my reserve, and that I only admit that fungus which has been proved by demonstration, while I deny all the other fungi as long as I do not hear of facts which attest them. Amongst infectious diseases there is a certain group which are caused by organic poisons—I will only mention one of them, which, according to my opinion, is very instructive—I mean the poisoning by a snake-bite, a very celebrated and most remarkable form. If this kind of poisoning is compared with those kinds of poisoning which are generally called infectious diseases (infection does not signify much else than poisoning), then we must admit that in the courses both cases generally take the greatest analogies exist. With regard to the course of the illness nothing would oppose the supposition that the total sum of phenomena which occur in a human body after a snake-bite, were caused by fungi which entered the body and which produced certain changes in different organs. Indeed we know certain processes, septical ones, for instance, where phenomena of a completely similar nature occur, and it cannot be denied that certain forms of poisoning by snake-bite resemble certain forms of septical infection as much as one egg resembles another. And yet we have not the least cause to suspect an importation of fungi into the body in the case of snake bite, while in the case of septic processes we, on the contrary, acknowledge and recognise this importation.

The history of our natural science has numerous examples, which ought always to cause us more and more to confine the validity of our doctrines in the most stringent manner to that domain only in which we can actually prove them, and that we do not by way of induction, proceed so far as to extend doctrines immeasurably which have only been proved for one or several cases. Nowhere the necessity of such a restriction has become more apparent than on the field of the theory of evolution. The question of the first origin of organic beings, this question which also forms the basis of progressive Darwinism, is an extremely old one. It is not known at all who first tried to find the different solutions for it. But if we remember the old popular doctrine, according to which all possible beings alive, animals and plants, could originate from a clod of clay—from a little clod under circumstances—then we ought to remember at the same time that the celebrated doctrine of *generatio æquivoca*, of epigenesis, is closely connected with it, and that it has been a common idea for thousands of years. Now with Darwinism the doctrine of spontaneous generation has been taken up again, and I cannot deny that there is something very seductive in the idea of closing the theory of descent in this way, and, after the whole series of living forms has been constructed, from the lowest protozoa upwards to the highest human organism, to connect this long series with the inorganic world as well. This corresponds with that direction to generalise, which is so entirely human, that it has found a place in the speculation of mankind at all times, backwards to the most obscure periods. We have the undeniable desire not to separate the organic world from the universe, as a something which is divided from it, but rather to insure its connection with the universe. In this sense it is pacifying if one can say, the atom-group carbon and company —this is perhaps speaking too collectively, but yet it is correct, since carbon is to be the essential element—therefore, this association, carbon and company, has at some special time separated itself from the ordinary carbon and founded the first plastidule under special circumstances, and continues to found it in the present. But in the face of this we must mention that all real scientific knowledge of the phenomena of life has proceeded in an opposite direction. We date the beginning of our real knowledge of the development of higher organisms from the day when Harvey pronounced the celebrated phrase, "Omne vivum ex ovo," every living being comes from an egg. This phrase as we now know, is incorrect in its generality. To-day we can no longer recognise it as a fully justified one; we know that, on the contrary, a whole number of generations and propagations exist without ova. From Harvey down to our celebrated friend Prof. von Siebold, who obtained the general recognition of parthenogenesis, there lies a whole series of increasing restrictions, all of which prove that the phrase, "Omne vivum ex ovo" was incorrect speaking in a general sense. Nevertheless, it would be the highest ingratitude if we were not to acknowledge that in the opposition, which Harvey assumed against the old *generatio æquivoca*, the greatest progress was made which has been made by science in this domain. Later on a great number of new forms were known, in which the propagation of the different kinds of living beings is going on, in which new individuals originate—direct separation, gemmation, metagenesis. All these forms, parthenogenesis included, are data which have caused us to give up every single (*einheitliche*) system for the generation of organic individuals. In place of a single scheme we now have a variety of data; we have no uniform system left by which we could explain once for all how a new animal being begins.

Generatio æquivoca, which has been disputed and refuted as many times, nevertheless faces us again and again. It is true that not a single *positive fact* is known which proves that *generatio æquivoca* has ever occurred, that spontaneous generation has ever taken place in such a way that inorganic masses, let us say the association carbon and company, have ever spontaneously developed into an organic substance. Nevertheless, I admit that if we indeed *want* to form an idea how the first organic being *could* have originated by itself, nothing remains but to go back to spontaneous generation. This is clear. If I do not want to suppose a creation-theory, if I do not want to believe that a special creator existed, who took the clod of clay and blew his living breath into it, if I want to form some conception in my own way, then I must form it in the sense of *generatio æquivoca*. *Tertium non datur*. Nothing else remains if once we say "I do not admit creation, but I do want an explanation." If this is the first thesis, then we must proceed to the second and say "Ergo, I admit *generatio æquivoca*." But we have no actual proof for it. Nobody has ever seen *generatio æquivoca* occurring in reality, and everyone who maintained that he had seen it, has been refuted, not by theologians indeed, but by naturalists. I mention this, gentlemen, in order to let our impartiality appear in the right light, and this is very necessary at times. We always have our weapons in ourselves and about us, to fight against that which is not justified.

I therefore say that I must admit the theoretical justification

of such a formula. Whoever will have a formula, whoever says "I absolutely want a formula, I wish to be perfectly at one with myself, I must have a coherent conception of the universe," must either admit *generatio æquivoca* or creation; there is no other alternative. If we want to be outspoken we may indeed own that naturalists may have a slight predilection for *generatio æquivoca*. It would be very beautiful if it could be proved.

But we must admit that it is not yet proved. Proofs are still wanting. If any kind of proof were to be successfully given we would acquiesce. But even then it would have to be determined first, to what extent we could admit *generatio æquivoca*. We should quietly have to continue our investigations, because nobody will think that spontaneous generation is valid for the totality of organic beings. Possibly it would only apply to a single series of beings. But I believe we have time to wait for the proof. Whoever remembers in what a regrettable manner, quite recently, all attempts to find a certain basis for *generatio æquivoca* in the lowest forms of the transition from the inorganic to the organic world, have failed, should c nsider it doubly dangerous to demand that this ill-reputed doctrine should be adopted as a basis for all human conceptions of life. I may, doubtless, suppose that the story of the *Bathybius* has become known to nearly all educated persons. With this *Bathybius* the hope has again vanished that *generatio æquivoca* can be proved.

I think, therefore, that with regard to this first point, the point of the connection between the organic and the inorganic, we must simply own that in reality we know nothing about it. We may not set down our supposition as a certainty, our problem as a dogma; that cannot be permitted. Just as in the progress of the doctrines of evolution it has been far more certain, more fertile, and more in accordance with the progress of accredited natural science, to analyse the original single doctrine part by part, we shall also have first to keep apart the organic and inorganic things in the old well-known analysing way, and not to throw them together prematurely.

Nothing, gentlemen, has been more dangerous to natural science, nothing has done more harm to its progress and to its position in the opinion of nations than premature syntheses. While laying stress upon this, I would point out specially how our Father Oken was damaged in the opinion not only of his contemporaries, but also in that of the following generation, because he was one of those who admitted syntheses into their conceptions to a far greater extent than a stricter method would have allowed. Do not let us lose the example of the natural philosophers; do not let us forget that every time that a doctrine which has assumed the air of a certain, well-founded, and reliable one, of one which claims general validity, turns out to be faulty in its outlines, or is found to be an arbitrary and despotic one in essential and great points, then a great number of men lose their faith in science entirely. Then the reproaches begin—"You are not sure even yourselves; your doctrine, which is called truth to-day, is a falsehood to-morrow; how can you demand that your doctrine shall become the object of instruction and of the general consciousness?" From such experiences I take the warning that if we wish to continue to claim the attention of all we must resist the temptation of pushing our suppositions, our merely theoretical and speculative structures into prominence to such a degree that from them we would construct the conception of the whole remaining universe.

(*To be continued.*)

THE METEOR

A METEOR of unusual brilliancy was seen on the evening of Friday, the 23rd inst., from various parts of the kingdom. Mr. F. A. Buxton writing to us from Hertford states that he saw it two miles north of that town at 8.26 P.M. He says:—"I was attracted by its glare notwithstanding the moonlight, and saw it moving vertically downwards. I could not accurately observe its path, but it passed, nearly or exactly, over a small star, just visible in the moonlight, which I think is π Herculis, and disappeared suddenly before it reached the horizon, in about N.P.D. 60 and R.A. 16·40. By comparing notes with another observer (half a mile north of Hertford) it appears to have been visible much nearer the zenith than I had seen it; probably I saw the last 15° of its path. From the apparent slowness of its motion and complete absence of sound I gather that it was far off. My guess at the moment was fifty miles. In consequence of its brightness its apparent diameter was probably illusory. It attained two *maxima* of splendour, one about over the star named, the other at its disappearance. Scarcely any 'trail' was left; what there was almost immediately vanished."

Mr. T. Mellard Reade writes that he saw it from Blundellsands, Liverpool, at 8.20 P.M. Looking up he saw a splendid broad streak of blue light terminating in a ball of red fire rushing across the sky in a north-westerly direction. The first flash seemed directly overhead; if so, Mr. Reade states, the meteor must have travelled through at least 45°. Shortly afterwards the moon being intensely bright and a shower coming on from the west, across the sea a most splendid "moon" rainbow made its appearance, finishing as a perfect arch of vivid colours with a second and a perfect bow above it.

Mr. W. B. Ferguson writes from Edinburgh that while walking down Princes Street about 8·25 P.M. he saw a most brilliant meteor which appeared to fall almost vertically and burst with great brilliance apparently just behind the castle. Its direction from where he observed it was 10° west of south.

Mr. C. H. Dance, writing from Manor House, Ardwick, Manchester, gives the time as 8h. 25m. P.M. Greenwich mean time. The meteor, he states, appeared to come from the constellation Cassiopeia, and after travelling in a direction a little to the west of north, finally burst behind a cloud about thirty degrees above the horizon. The apparent size of the meteor was considerably greater than that of Mars during the late opposition, and the light which it emitted was intensely bright and of a bluish-green colour, leaving a decidedly red impression on the retina. The period of visibility would be about five seconds, and the sparks in the train were also visible for some seconds.

Mr. Plant, the Curator of the Salford Museum, observed the meteor at the same time, visible to the north of Manchester.

Dr. S. Drew, of Sheffield, saw it at about 8.30 P.M. He gives the apparent diameter as two minutes; path, from the square of Pegasus to near Altair; motion, slow; shape, at first globular, afterwards elongated, with tail. It then appeared to break up. Colour, at first blue-green, afterwards ruddy; light, brilliant. He heard no sound accompanying the meteor, and from the absence of sound and slow apparent motion, he infers the real distance and size of the bolide to have been great. Dr. Drew was, at the time of observation, a little to the west of the town of Rotherham.

Several correspondents write to the *Times* describing what they saw of this remarkable meteor, for it is evidently the same body which has been seen by the various observers. The Liverpool correspondent of the *Times* saw it about 8·30. "A large ball of fire shot from the sky, exploding and throwing off innumerable variegated sparks as it descended in a northerly direction. The track of sparks gave the meteor the appearance of a brilliant comet with a long tail. Some spectators state that they heard the hissing noise made in its course, and others allege that it descended into the water near the bar of the Mersey with a great noise, sending up a column of steam and spray."

Mr. Donald Mackay saw it from Victoria Street, London, shortly before 8·30 P.M. "It travelled with great rapidity for about 20° from the zenith to the horizon, bursting in a white ball as large as twelve of the planet Mars in one, lighting up all the houses surrounding Victoria Street, the point of observation, and leaving a large tail behind of the shape of a spear-head, with all the colours of the rainbow in it."

The Rev. J. Hoskyns-Abrahall writes from Combe Vicarage, near Woodstock, that about 8.20 the northern sky was suddenly lighted up with a glow that outshone that spread over the south-eastern sky by a moon nearly full. "Looking northwards I saw a globular meteor of a pale orange colour descending perpendicularly. Its apparent size was scarcely less than that of the moon. Just above the slope on which I was, and seemingly not half a mile off, it burst into huge fragments, which flared forth with a fierce, lightning-like, reddish glare, and scattered sparks of surpassing splendour."

Mr. D. Aldred writes from Milford, Derby, to the same effect. He saw the meteor about six miles north of Derby, about 8.25. "It was almost due north, and travelling from the zenith to the horizon, the point of dispersion being about 45° above the north point of the horizon. In shape it was conical, the greatest breadth about one and a half times the diameter of the moon. It left a trail of considerable length, and the colours detached were of most remarkable brilliancy."

"R. M. C." writes from Cathedine, Brecknockshire, giving the report of two reliable witnesses who were walking in an easterly direction at 8.25 P.M. Looking back, the moon being at the time obscured by a cloud, they saw a ball of the most intense white light, "about the size of a cannon-ball," travers-

ing a space between two clouds, leaving behind it a fiery track of red.

A Worcester correspondent gives the time as 8.20. He describes the colour as brilliant blue and orange, and behind was a streaming trail of brilliant sparks, which remained visible for a few seconds after the brighter light had disappeared.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—At a Congregation on November 22, the University seal was ordered to be affixed to a letter of thanks to his Grace the Chancellor of the University for his munificent gift of a complete apparatus of scientific instruments for the Cavendish Laboratory.

A meeting of the members of the University to consider the propriety of securing a personal memorial of Dr. Darwin, was held on Monday in the combination room of Christ's College, the Rev. Dr. Cartmell, Master of the College, presiding. It was proposed by Prof. Humphry and seconded by Prof. Fawcett, "That it is desirable that the University should possess a personal memorial of Mr. Charles Darwin, LL.D." Proposed by Prof. Newton and seconded by Mr. Piele, of Christ's, "That the members of the University now present form themselves into a committee, with power to add to their number, for the purpose of collecting subscriptions from members of the University to carry out the foregoing resolution." Proposed by Prof. Liveing, seconded by Mr. J. W. Clark, "That Mr. A. G. Dew-Smith, of Trinity College, be treasurer and secretary to the committee, and be authorised to receive subscriptions." It was understood that the memorial should assume the form of a portrait, and about 75*l*. was subscribed in the room.

EDINBURGH.—The subscriptions to the Edinburgh University Extension Fund now amount to 82,000*l*., and Government has now promised to add 80,000*l*. to the amount on condition that 25,000*l*. is raised by public subscription, of which the sum of 10,000*l*. must be subscribed by December 31st next. The University Professors at Edinburgh have already contributed among themselves 5,360*l*. towards the additional 25,000*l*. required.

ST. ANDREWS.—Lord Selborne has been elected Lord Rector of this University. The students had much difficulty in getting any eminent man to allow himself to be nominated, and it was only on the day previous to the election that it was resolved to pit Lord Selborne against the Right Hon. Gathorne Hardy.

Prof. Alleyne Nicholson has been appointed Swiney Lecturer on Geology by the Trustees of the British Museum.

LEIPZIG.—Prof. Leuckhart, the newly-elected Rector of the University, was installed into the duties of the office on October 31, and delivered on the occasion an able address "On the Development of Zoology up to the Present Time, and its Importance." The students already number nearly 3,200, an attendance, as usual, far above that of any other German university.

AMSTERDAM.—The new University of Amsterdam has lately made a most flattering offer to Prof. Gegenbaur, of Heidelberg, which has, however, been declined.

BERGEN.—It is intended to establish a new university in the Norwegian town of Bergen. Eighty thousand crowns have already been subscribed towards this object.

SOCIETIES AND ACADEMIES

LONDON

Mathematical Society, November 8.—Lord Rayleigh, F.R.S., president, in the chair.—The following were elected to form the Council during the session:—President: Lord Rayleigh, F.R.S. Vice-Presidents: Prof. J. Clerk Maxwell, F.R.S., Mr. C. W. Merrifield, F.R.S., Prof. H. J. S. Smith, F.R.S. Treasurer, Mr. S. Roberts. Hon. Secretaries: Messrs. M. Jenkins and R. Tucker. Other members, Prof. Cayley, F.R.S., Mr. T. Cotterill, Mr. J. W. L. Glaisher, F.R.S., Mr. H. Hart, Dr. Henrici, F.R.S., Dr. Hirst, F.R.S., Mr. Kempe, Dr. Spottiswoode, F.R.S., Mr. J. J. Walker.—Prof. Cayley made two communications, on the function $\phi(x) = \dfrac{ax+b}{cx+d}$ (a singularly neat expression was got for $\phi^n(x)$, the late Mr. Babbage had considered the matter in 1813), and on the theta functions.—Mr. Tucker read a portion of a paper by Mr. Hugh MacColl (communicated by Prof. Crofton, F.R.S.) entitled the calculus of equivalent statements. A short account of this analytical method has been given in the July and November numbers (1877) of the *Educational Times*, under the name of Symbolical Language. The chief use at present made of it is to determine the new limits of integration when we change the order of integration or the variables in a multiple integral, and also to determine the limits of integration in questions relating to probability. This object, the writer asserts, it will accomplish with perfect certainty, and by a process almost as simple and mechanical as the ordinary operations of elementary algebra.—The president read a paper on progressive waves. It has often been remarked that when a group of waves advance into still water the velocity of the group is less than that of the individual waves of which it is composed; the waves appear to advance through the group, dying away as they approach its anterior limit. This phenomenon seems to have been first explained by Prof. Stokes, who regarded the group as formed by the superposition of two infinite trains of waves of equal amplitudes and of nearly equal wave-lengths advancing in the same direction. The writer's attention was called to the subject about two years since by Mr. Froude, and the same explanation then occurred to him independently. In his work on "The Theory of Sound" (§ 191), he has considered the question more generally. In a paper read at the Plymouth meeting of the British Association (afterwards printed in NATURE), Prof. Osborne Reynolds gave a dynamical explanation of the fact that a group of deep-water waves advances with only half the rapidity of the individual waves. Another phenomenon (also mentioned to the author by Mr. Froude) was also discussed as admitting of a similar explanation to that given in the present paper. A steam launch moving quickly through the water is accompanied by a peculiar system of diverging waves, of which the most striking feature is the obliquity of the line containing the greatest elevation of successive waves to the wave-fronts. This wave-pattern may be explained by the superposition of two (or more) infinite trains of waves, of slightly differing wave-lengths, whose direction and velocity of propagation are so related in each case that there is no change of position relatively to the boat. The mode of composition will be best understood by drawing on paper two sets of parallel and equidistant lines, subject to the above conditions, to represent the crests of the component trains. In the case of two trains of slightly different wave-lengths, it may be proved that the tangent of the angle between the line of maxima and the wave-fronts is half the tangent of the angle between the wave-fronts and the boat's course.—Prof. Clifford, F.R.S., communicated three notes. (1) On the triple generation of three-bar curves. *If one of the three-bar systems is a crossed rhomboid, the other two are kites.* This follows from the known fact that the path of the moving point in both these cases is the inverse of a conic. But it is also intuitively obvious as soon as the figure is drawn, and thus supplies an elementary proof that the path *is* the inverse of a conic in the case of a kite, which is not otherwise easy to get. (2) On the mass-centre of an octahedron. The construction was suggested by Dr. Sylvester's construction for the mass centre of a tetrahedral frustum. (3) On vortex-motion. The problem solved by Stokes as a general question of analysis, and subsequently by Helmholtz for the special case of fluid motion may be stated as follows: given the expansion and the rotation at every point of a moving substance, it is required to find the velocity at every point. The solution was exhibited in a very simple form.

Zoological Society, November 6.—Mr. A. Grote, vice-president, in the chair.—A letter was read from Mr. R. Trimen, containing remarks on the African species of *Sarcidiornis*.—A letter was read from Mr. A. O. Hume, containing some remarks on Mr. Howard Saunders' recent paper on the Sterninæ.—The secretary exhibited, on the part of Mr. Geo. Dawson Rowley, an egg of *Pauxis galeata*, laid by a black female.—Prof. W. H. Flower, F.R.S., read a paper entitled "A Further Contribution to the Knowledge of the existing Ziphioid Whales of the Genus *Mesoplodon*, containing a Description of a Skeleton and several Skulls of Cetaceans of that Genus from the Seas of New Zealand."—A communication was read from Lieut.-Col. R. H. Beddome, containing the descriptions of three new species of reptiles from the Madras Presidency. These were proposed to be called *Oligodon travancoricum*, *Gymnodactylus jeyporensis*, and *Bufo travancoricus*.—A communication was read from the Marquis of Tweeddale, F.R.S., containing an account of a collection of

birds made by Mr. A. H. Everett in the Island of Luzon, Philippines. Three new species were named *Megalurus ruficeps*, *Dicæum xanthopygium*, and *Oxycerca everetti*.—Mr. D. G. Elliott read some remarks on *Felis tigrina*, Erx., and its synonomy, showing that *F. mitis*, F. Cuv., and *F. macrura*, Pr. Max., are identical with that species.—Prof. Garrod, F.R.S., read a paper on some points in the visceral anatomy of the rhinoceros of the Sunderbunds (*Rh. sondiacus*).—A second communication from Prof. Garrod contained a note on an anatomical peculiarity in certain storks.—Mr. Edgar A. Smith read a paper in which he described some shells from Lake Nyassa, and a few marine species from the mouth of the Macusi River, near Quillimane, on the East Coast of Africa.—A communication from Dr. O. Finsch contained the description of a new species of petrel from the Feejee Islands, which it was proposed to name *Procellaria albigularis*.—A second communication from Dr. Finsch contained a report on the collections of birds made during the voyage of H.M.S. *Challenger* at Tongatabu, the Fiji Islands, Api, New Hebrides, and Tahiti.—Mr. Edward R. Alston read a supplementary note on rodents and marsupials from Duke of York Island and New Ireland. *Macropus lugens*, Alst., was shown to be a synonym of *Helmaturus brownii*, Ramsay, while Mr. Ramsay's *Mus. echimyoides* and *M. musavora* were respectively identical with *Mus. brownii* and *Uromys rufescens* of Alston.—A communication from Mr. L. Taczanowski contained a supplementary list of birds collected in North-Western Peru by Messrs. Jelski and Stolzmann. Two species were new, and proposed to be called *Rallus cypereti* and *Penelope albipennis*.

Cambridge

Philosophical Society, October 22.—A communication was read by Mr. Balfour, on the development of the vertebrate ovum. The points dealt with in this paper were (1) the nature of the stroma of the ovary, and (2) the relation of the permanent ova to the large cells of the germinal epithelium, named primitive ova by Waldeyer.

October 29.—Mr. Bonney read a paper on the rocks of the Lizard District (Cornwall). The author brought forward evidence to prove that the serpentine of this district was clearly intrusive among the hornblende schists.

November 5.—Prof. Clerk Maxwell communicated to the society an account of the unpublished papers of the Hon. Henry Cavendish, which contain his experiments in electricity.

Manchester

Literary and Philosophical Society, October 2.—Rev. William Gaskell, M.A., in the chair.—A case of flowering of *Chæmerops fortunei* (Hook) at Alderley, by Arthur W. Waters, F.G.S. The fact of *Chæmerops fortunei* (Hook) flowering so far north as near Manchester seemed to the author to be of sufficient interest to be worth mentioning to the Society.—Table of effect of movement of the surface of the globe on the shifting of the axis of the earth, by Arthur W. Waters, F.G.S.

Paris

Academy of Sciences, November 19.—M. Peligot in the chair:—The following papers were read:—Meridian observations of small planets at the Greenwich and Paris Observatories during the third quarter of 1877, communicated by M. Villarceau.—New remarks on the quantities of heat liberated by mixture of water with sulphuric acid, by M. Berthelot. He affirms that sulphuric acid always liberates the same quantities of heat whether it have been recently heated or kept a considerable time.—*Résumé* of a history of matter (fifth article), by M. Chevreul.—On the theory and the various manœuvres of the economising apparatus constructed at the dam of Aubois, by M. de Caligny.—On the use of refined neutral oils for lubrication of pistons in engines with surface condensers, by M. Allaire. Lime causes decomposition of neutral fatty matters and unites with their acids, the result being a greater deposit than if lime had not been used. Doubtless the deposit is oleate of lime instead of oleate of iron, and the boiler is preserved from attack; but the inconveniences in condensing engines are aggravated, for the condenser ceases to act as the tubes get covered. M. Allaire commends the use of refined neutral fatty matters which are undecomposable under the ordinary pressure of boilers.—Various observations on phylloxera, by M. Boiteau. The winter egg is deposited exclusively on the exterior of the stock.—Discovery of a small planet at Ann Arbor, by Mr. Watson.—General map of the proper motions of stars, by M. Flammarion. One result of this comparison is contradictory of some common views as to the distance of stars relatively to their order of brightness; for the greatest proper motions do not belong to the most brilliant stars, but indifferently to all sizes. Again, the author cannot support Bessel's and Struve's view that double stars are carried through space more rapidly than simple stars.—On the equation with partial derivatives of the fourth order, expressing that the problem of geodesic lines, considered as a problem of mechanics, supposes an algebraic integral of the fourth degree, by M. Levy.—New applications of a mode of plane representation of classes of ruled surfaces, by M. Mannheim.—On the laws which rule the order (or class) of plane algebraic curves, of which each point (or each tangent) depends at once on a variable point and tangent in a given curve, by M. Fouret.—Extract from a letter (mathematical) to M. Hermite, by M. Fuchs.—On the decomposition into first factors of the numbers $2^n \pm 1$, by M. de Longchamps.—Reproduction of orthose, by M. Hautefeuille. Orthose can be obtained by raising to from 900 to 1,000 deg. a mixture of tungstic acid and a very alkaline silico-aluminate of potash containing one equivalent of alumina to six of silica. The tungstic acid forms tungstate of potash, and the silico-aluminate is thus brought to the composition of orthose.—On the composition and industrial use of gases from metallurgical furnaces, by M. Cailletet. These gases, if suddenly cooled, are found to contain an important quantity of combustible principles which can easily be lit again and burnt by passing, *e.g.*, through a grate with burning fuel, and having their velocity diminished.—Formation of iodous acid by the action of ozone on iodine, by M. Ogier.—On the solubility of sugar in water, by M. Courtonne. A saturated solution of sugar at 12·5° contains 66·5 gr. per cent. of sugar; one at 45° contains 71 gr. per cent.—On the products of oxidation of camphor, by M. Montgolfier.—Note on the accessory discs of the thin discs in striated muscles, by M. Renaut. Muscular striation is formed of a succession of thick discs alone contractile, and of clear bands traversed each by a thin disc and two accessory discs similar to each other as regards form, and probably having similar functions.—Analgesia obtained by the combined action of morphine and chloroform, by M. Guibert. A subcutaneous injection of chlorhydrate of morphine is made at least fifteen minutes before inhalation of chloroform.—On the causes of violet colour in oysters of the basin of Arcachon, by M. Descoust. The colour is found to be due to the presence of a small algal of the family of Rhodospermeæ and Florideæ. This becomes more abundant in time of drought, and probably acts by absorbing moisture.—On the migrations and metamorphoses of the taenia of shrew mice, by M. Villot.—On certain monstrosities of Asterocanthion rubens, by M. Giard.—On the embryogeny of the cestoides, by M. Moniez.—On the bismuth ores of Bolivia, Peru, and Chili, by M. Domeyko.

CONTENTS

THURSDAY, DECEMBER 6, 1877

TECHNICAL EDUCATION

PROF. HUXLEY has seized the occasion afforded him by his promise to aid the Working Man's Club and Institute Union by contributing to their present series of fortnightly lectures, to state his opinion on a question which, as we have already informed our readers, has lately been exercising the minds of some of the most influential members of various city companies.

For some time past a joint committee, representing the most important among these bodies, has been endeavouring to obtain information as to the best means of applying certain of their surplus funds to the assistance of what is called technical education, and there is little doubt that a proposal for a huge technical university, made some time ago, and the discussion which took place in connection with that proposal, has had somewhat to do in leading to the present condition of affairs.

Prof. Huxley and some four or five other gentlemen have been appealed to by this joint committee to send in reports on what they consider the best way to set about the work, and it is from this point of view that Prof. Huxley's lecture is so important. It was not merely fresh and brilliant and full of good things, as all his lectures are, but is doubtless an embodiment of his report to the joint committee.

We are rejoiced, therefore, to see that Prof. Huxley is at one with the views which we have all along expressed in NATURE, namely, that, after all, the mind is the most important instrument which the handicraftsman, whether he be a tinker or a physicist, will ever be called upon to use, and that therefore a technical education which teaches him to use a lathe, or a tool, or a loom, before he has learned how to use his mind, is no education at all.

Prof. Huxley not only defined technical education as the best training to qualify the pupil for learning technicalities for himself, but he stated what he considered such an education might be, and how the city funds can be best spent in helping it on.

Besides being able to read, write, and cipher, the student should have had such training as should have awakened his understanding and given him a real interest in his pursuit. The next requirement referred to was some acquaintance with the elements of physical science—a knowledge rudimentary, it might be, but good and sound, so far as it went, of the properties and character of natural objects. The professor is also of opinion that it is eminently desirable that he should be able, more or less, to draw. The faculty of drawing, in the highest artistic sense, was, it was conceded, like the gift of poetry, inborn and not acquired; but as everybody almost could write in some fashion or other, so, for the present purpose, as writing was but a kind of drawing, everybody could more or less be supposed to draw. A further *desideratum* was some ability to read one or two languages besides the student's own, that he might know what neighbouring nations, and those with which we were most mixed up, were doing, and have access to valuable sources of information which would otherwise be sealed to him. But above all—and this the speaker thought was the most essential condition—the pupil should have kept in all its bloom the freshness and youthfulness of his mind, all the vigour and elasticity proper to that age. Prof. Huxley then went on to explain that this freshness and vigour should not have been washed out of the student by the incessant labour and intellectual debauchery often involved in grinding for examinations.

We gather from this part of the address—we shall refer to the others by and by—that so far as Prof. Huxley's advice goes we are not likely to see any great expenditure of the money of the ancient city corporations either in the erection of a huge "practical" university or in the creation of still another "Examining Board." How then does he propose to spend it?

Here we come to a substantial proposal, which Prof. Huxley may consider to be the most important part of his address. What is wanted, he considers, is some machinery for utilising in the public interest special talent and genius brought to light in our schools. "If any Government could find a Watt, a Davy, or a Faraday in the market, the bargain would be dirt cheap at 100,000*l*." Referring to his saying when he was a member of the London School Board that he should like to see a ladder by which a child could climb from the gutter to the highest position in the State, he dwelt upon the importance of some system by which any boy of special aptitude should be encouraged to prolong his studies, to join art and science classes, and be apprenticed, with a premium if necessary. In the case of those who showed great fitness for intellectual pursuits they might be trained as pupil-teachers, brought to London, and placed in some collegiate institution or training school. In this way the money of the guilds would be spent in aiding existing teaching systems, in which, on the whole, an enormous progress was acknowledged.

It is true the architects of London would not have the opportunity of immortalising themselves by erecting an imposing edifice, but, on the other hand, the influence of the Guilds might be felt whenever there was a handicraft to foster, or a potential Watt to be sought out.

We do not imagine that it is Prof. Huxley's idea that there shall be no local representation of the city's new activity and influence; the reference to the training of teachers, we fancy, and other remarks here and there, seem to point to some such institution as the École Normale of Paris, where the best and most practical scientific teaching could be carried on. Every one knows how much room there is for such an institution as this, but on this little money need be spent, so far as bricks and mortar are concerned, as little money is needed to equip such laboratories as are really meant for work.

There is an advantage in such lectures as these by no means limited to the expression of opinion on the part of the speaker. The slow and sure way in which science is taking a hold upon our national progress is well evidenced by the fact that the daily press can now no longer ignore such outcomes as these, and hence it is that they do good beyond the mere boundary of the question under discussion. They show the importance of, and foster interest in, the general question of intellectual and scientific progress.

The *Times* agrees in the main with the kind of educa-

tion to be given, and holds that "What is needed is to give a man the intelligence, the knowledge of general principles, combined with the habits of correct observation and quick perception, which will enable him afterwards to master the technicalities of his art, instead of becoming a slave to them. No objection can be taken to the advice that, for this purpose, a lad, after learning to read, write, and cipher, should acquire some facility in drawing, and should be familiarised with the elements of physical science. The importance of the latter study for this particular purpose is, indeed, unquestionable, and even paramount, for a handicraftsman is dealing exclusively with physical objects in his work, and his skill in applying the processes of his craft will vary in great measure with his knowledge of the scientific principles on which they depend."

But we fancy that the *Times* writer does not look upon this scientific part of education quite as the lecturer does, for he proceeds to add :—"There can be little doubt, for instance, that many of the perils of mining might be averted if the miners were alive to the scientific reasons of the precautions they are urged to adopt. Many an improvement, probably, which now escapes the eye of a man who adheres slavishly to the rules of his craft would occur to him if he were applying them with conscious intelligence."

The *Times*, however, considers that the school-time is too short for the languages, and curiously enough drives its point home by saying a harder thing about the Greek and Latin of our public schools than Prof. Huxley has ever done; while, on the other hand, the *Daily News* points out that Prof. Huxley this time may have raised a hornet's nest about his ears by the unduly reasonable tone of his demands.

The *Daily News* then adds :—"A man of science who does not demand that from the earliest age an hour a day shall be devoted to each of the ologies may be regarded as a traitor to his cause." For our part we know of no man of science who has ever made such a demand; and a careful examination of what men of science have said on this point for the last ten years will show that these extreme views to which reference is here made are not those of men of science at all.

It will be well also if the strong language used in connection with the multiple examinations of the present day brings that question well before the bar of public opinion. The *Times* is "sorry to see another flout thus inflicted, in passing, on that system of examinations which, like most good institutions, may do harm to the few, but is indispensable as a motive for work to the great majority." Prof. Huxley has expressed the views of most of the leading teachers in this country with regard to the effect of these examinations upon the students, and he might have referred to their reflex action on the examiner. Go into a company of scientific men, and observe the most dogmatic, the most unfruitful, and the least modest among them, you will find that this man is, as we may say, an examiner by profession. Speak to him of research or other kindred topics, he will smile at you—his time is far too precious to be wasted in discussing such trivialities; like his examinees, he finds they do not pay. The example set by Germany in this respect, both as regards students and professors, cannot be too often referred to, and there is little doubt that the love of science for its own sake which has made Germany what she now is intellectually, has sprung to a large extent from the fact that each young student sees those around him spurred from within and not from without. *Noblesse oblige.*

In point of fact so far as our future scientific progress is concerned the examination question is as important as that connected with the kind of education to be subsidised by the city guilds, and it is important, seeing that our legislators will, in the coming time, have to give their opinion on these subjects as well as on beer, vivisection, and contagious diseases, that in Prof. Huxley's language "by the process called *distillatio per ascensum*—distillation upwards—there should in time be no member of Parliament who does not know as much of science as a scholar in one of our elementary schools."

NORTH AMERICAN STARFISHES

Memoirs of the Museum of Comparative Zoology at Harvard College. Vol. v. No. 1. North American Starfishes. By Alexander Agassiz. With Twenty Plates. (Cambridge, U.S., 1877.)

THIS memoir consists of two parts. The first contains a history of the Embryology of the Starfish, which is substantially the same as that published in 1864 as Part I., Vol. v., of Prof. Agassiz's "Natural History of the United States." The author has, however, added notes on the points where additions have been made by subsequent investigations. The second part treats of the solid parts of some North American starfishes.

The plates accompanying the second part were intended to form part of one of Prof. L. Agassiz's volumes of "Contributions to the Natural History of the United States," and have been drawn for more than twelve years. The late Prof. Agassiz intended to add them as illustrating the anatomy of several of the more common American species.

Under these circumstances the memoir is wanting in the completeness that distinguishes some of the other Memoirs of this series, such as that "On the Ophiuridæ," by Lyman, and that "On the Echini," by Alexander Agassiz; but though the subject of the Starfishes as thus presented is incomplete, it is beyond a doubt that we have here a work of great value that will serve not only as illustrating a number of American species, and showing the systematic value of characters often almost completely overlooked, but as determining the homology of several genera not previously figured, and of which the details of the solid parts are fully given.

The arrangement of the star-fishes into families adopted does not materially differ from that given by Perrier in his revision of the group. No general list, much less a synonymic catalogue, as in the case of Echini, is given; and this because the number of species in the hands of Prof. Perrier, from the *Florida* dredgings, as well as those found by the *Challenger* expedition, have added a number of remarkable forms not yet wholly determined to the American starfish fauna.

The author reminds us that the transformations peculiar to the Echinoderms constitute neither a metamorphosis nor a case of alternate generation. The egg becomes the embryo larva. Nothing essential is lost during the

process. No intermediate form comes into the cycle; the yolk becomes the larva, and this latter becomes the young Echinoderm; and this larva is, according to A. Agassiz, an Acalephian larva, reminding one somewhat of the twin individuals of free Hydroids as Diphyes, though adapted to the mode of development of the Echinoderms. The Echinoderm plutean form, with its mouth-stomach intestine, and with its water system originally forming a part of the digestive cavity, bearing as it would seem, about the same relation to the Ctenophoræ, which the Hydroid Polyps hold to the true Polyps. Therefore Agassiz cannot admit that the views so frequently urged and so generally admitted as to the separation of the Acalephs and Polyps as a distinct type (Cœlenterata) from the Echinoderms have any foundation in nature. He would therefore still retain the Radiate sub-kingdom with its three equivalent classes—Echinoderms, Acalephs, and Polyps.

Agassiz thinks G. O. Sars' idea that Brisinga is the living representative of the palæozoic starfishes rather too far-fetched, and he sees no very radical difference between Brisinga and such ordinary starfishes as Solaster and Crossaster, and he considers that if there has been a single ancestral Echinoderm, his primordial descendants early assumed different lines of development diverging to a great degree, and retaining their characteristics from the earliest-known geological period. E. P. W.

VOGEL'S "SPECTRUM ANALYSIS"

Practische Spectralanalyse irdischer Stoffe. Von Dr. Hermann W. Vogel (Nördlingen: C. H. Beck.)

THE aim of the author in writing this book may best be described in his own words. He says in the introduction:—

"The many excellent popular books on spectrum analysis confine themselves chiefly to descriptions of the great discoveries made by means of it; the chemical books only give short descriptions of flame reactions of alkalies and alkaline earths; they contain seldom a detailed account of the methods of observation, and still less a description of absorption spectra. The present work is intended to fill up this want, and to be a text-book to the student, and a reference book to the initiated."

Prof. Vogel is an authority on the absorption spectra of liquids and solids. Nearly half the book is given up to them, and we must add the better half. Here we find for the first time a connected account of all that has been done on the subject. Such an account is exceedingly valuable, and it brings prominently forward the gaps which have yet to be filled up. Prof. Vogel treats the subject chiefly from the chemical point of view, but those who take greater interest in the theoretical part will also find excellent information. So, for instance, the effect of the solvent on the absorption spectra of solutions is discussed. The spectra of colouring matters are given in detail, and the account of the effect of chemical reagents on them will be found exceedingly interesting. There is no doubt that this part of the book will be of great use to every worker on the subject.

We wish we could say as much of the chapter on emission spectra. As long as the author treats of the spectra of alkalies and alkaline earths, he is on safe ground, but when he comes to discuss the question of double spectra and the spectra of gases, he is confused and unintelligible. Led away apparently by a desire to do justice to every writer, he quotes approvingly the most divergent opinions, as if they could be consistently held at the same time. He is very fond of saying that a body has been proved to have two spectra but that one of them belongs to the oxide or to an impurity, which is the same as saying that he possesses two watches but that one of them belongs to his brother.

The author is throughout the book careless in his expressions, and this comes prominently forward in this chapter. What, for instance, can the student make of the following paragraph (p. 170)?—

"*A strong electric spark passing through air gives the spectrum of oxygen together with that of nitrogen.* Both together form the so-called spectrum of air. Only one spectrum of oxygen is known. *In dry pure air the spark only generates the spectrum of nitrogen.*"

The two statements in italics contradict each other as they stand. One of them is true for higher pressures, the other for lower pressures, but this the author has forgotten to add.

It must be said that the subject is a complicated one, and even those who are practically acquainted with all the experimental details would find it difficult to give a connected and clear account of it.

The first part of the book which treats of the optical principles involved in the spectroscope is apparently well written, and the student will find in it elementary proofs of some important theorems.[1] ARTHUR SCHUSTER

OUR BOOK SHELF

Nyassa; a Journal of Adventures whilst Exploring Lake Nyassa, Central Africa, and Establishing the Settlement of "Livingstonia." By E. D. Young, R.N. Revised by Rev. Horace Waller. With Maps. (London: John Murray, 1877.)

THIS is a thoroughly interesting narrative, brisk, fresh, and instructive. Mr. Young tells the story of the planting of a missionary station under the united auspices of the Presbyterian churches of Scotland, at Cape Maclear, on the south-west corner of Lake Nyassa. Mr. Young for the most part takes us over classic ground, by the Zambesi and Shiré, over ground familiar to readers of Livingstone's earlier and his latest travels. Mr. Young in his hardy little steamer the *Ilala*, surveyed the north end of Lake Nyassa for the first time, discovering on its north-east shore a magnificent range of mountains, rising to from 8,000 to 12,000 feet above the level of the lake, and which he named after his old friend Livingstone. On the opposite shore is a range of less elevation. The lake is marshy at the north end, subject to quite oceanic storms, its shores being marked by varied and most attractive scenery. The steamer caused tremendous consternation among the slave-trading Arabs, who seemed to feel that with the advent of a British steamer on the lake their occupation was gone. The settlement was successfully planted and is likely to be of service both as a centre of civilisation and of more minute exploration.

Britannia: A Collection of the Principal Passages in Latin Authors that Refer to this Island. With Vocabulary and Notes. By Thos. S. Cayzer, Head-Master of Queen Elizabeth's Hospital, Bristol. Illustrated with a Map and twenty-nine Woodcuts. (London: Griffith and Farran, 1878.)

THE title-page sufficiently describes the contents of this

[1] As a personal question I may add that the remark attributed to me on page 198 was made by Mr. Stoney and only quoted by me.—A. S.

little volume. We think the idea of making such a collection a happy one, not only for scholastic purposes, but also for the use of those who wish to be able at any time easily to refer to any of the passages in Latin authors in which our island is referred to. Mr. Cayzer gives also translations of some of the chief references in Greek writers. We should think, if teachers and examiners could be persuaded to break through custom, the introduction of such a book into schools would add interest to the reading of Latin, and furnish, besides, the little fellows with a stock of valuable information. Most of the cuts are appropriate, several being old friends.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

The Colour-Sense of the Greeks

MR. GLADSTONE has shown that the language of Homer is an inadequate vehicle for conveying precise and nicely distinguished ideas of colour. Whether the nation that was content to describe colours so imperfectly was also incapable of subtle perception of tones of colour is clearly another question. Language does not keep pace with perception unless a practical or æsthetic necessity arises for expressing what is perceived in words to other people.

Practical necessity gives names to pigments and bright objects, such as flowers and precious stones, rather than to tones of colour; the æsthetic necessity that lies upon the artist to utter what he has felt will naturally lead to imitative expression sooner than to an expression that is merely symbolical. In other words an early race will learn to use colour with nicety for decorative and pictorial purposes before it develops the distinctions of language requisite for accurate word-painting.

That this was actually the case among the Greeks appears, I think, very clearly in a passage of Ion which is preserved to us in Athenæus Deipnos., Lib. xiii. cap. 81 (p. 603 *seq.*). Ion, who was a contemporary of Sophocles, describes an evening which he spent with the great tragedian in Chios. Sophocles, admiring the blushing face of a little boy who served the wine, quoted, with high approval, a line of Phrynicus:—

"The light of love gleams on the purple cheek."

On this a certain pedantic grammarian breaks in—"In sooth, Sophocles, thou art skilled in poetry; but yet Phrynicus spoke not well when he called the cheeks of a beautiful person purple. For if a portrait-painter were to colour the cheeks of this boy with purple pigment he would no longer appear beautiful. It is not fitting to compare what is beautiful with what is not so." Sophocles laughs at the objection, and replies—"Neither, then, my friend, wilt thou be pleased with that line of Simonides which, to the Greeks, has appeared very well said:—

'The maiden sending forth her voice from her purple mouth;'

nor with the poet, when he says, 'golden-haired Apollo;' for if the painter made the hair of the god golden and not black, his picture would be less excellent. Nor wilt thou be pleased with him [Homer] who said 'rosy-fingered,' for if one were to dip the fingers in rose-colour, one would produce the hands, not of a fair woman, but of a dyer of purple." This retort produced a general laugh, and confounded the pedant not a little.

The Greeks, then, were perfectly aware of the insufficiency of the poetic vocabulary of colour; and accordingly they did not expect descriptive rendering of colour from the poet. This, it is plain, is a circumstance that must constantly be kept in view in any attempt to find in the poetry of the Greeks a measure of the development of their colour-sense.

Aberdeen, December 3 W. ROBERTSON SMITH

The Comparative Richness of Faunas and Floras Tested Numerically

IN his letter in NATURE, vol. xvii. p. 9, Prof. Newton has strongly brought out the absurdity of comparing districts of *very different areas* by the proportionate number of species to area in each. On this principle he shows that to be equally rich with the small island of Rodriguez, Madagascar ought to possess four times as many species of birds as exist throughout the whole world! It does not, however, by any means follow that the method thus exposed may not be of value in comparing regions of approximately equal area, as is the case with several of the primary regions, to determine the comparative richness of which Mr. Sclater first applied it. I have not Mr. Sclater's paper at hand, but it is my impression that he made no attempt to show—"that the proper mode of comparing the wealth or poverty of one fauna with another was to state the proportion which the number of species composing it bears to the area over which they range"—as Prof. Newton implies that he did, but that he merely adopted this method as the only one readily available for the comparison of his regions. Although I took the opportunity of making some corrections in the figures, I never committed myself to the principle; and I very soon afterwards found that it was not to be trusted. As, however, several later writers have made use of it without remark, it will be interesting to consider where the exact point of the fallacy lies, and with what modifications the method can be trusted to give useful and consistent results.

If we compare two islands of almost exactly equal areas, such as Ceylon and Tasmania, and find that the one has twice or three times as many species of mammals or birds as the other, it will be generally admitted that we express the fact correctly when we say that, as regards such a group of animals, the one is twice or thrice as rich as the other; and the same may be said of two countries or two continents of identical areas. For on the supposition that there is a general correspondence between the numbers of rare and common, of local and of wide-spread species in the two areas compared (and this seems probable), then the average number of distinct species to be met with on one spot, or to be seen during a journey of equal length, will be proportionate to the total number of species in the two areas. But now let us divide one of the two continents or islands which we are comparing into two or more parts. We know, as a matter of fact, that one-half the area will always contain much more than half the total number of species, while one-tenth of the area will contain immensely more than one-tenth of the species. To take an example: the county of Sussex is about one-eightieth part the area of the British Isles, yet it actually contains full two-thirds of the total number of flowering plants, both being estimated by the same flora (Babington's "Manual," fifth edition, British Isles 1,536 species, Sussex 1,059 species). If we now compare either Britain or Sussex with an *equal area* on the continent of Europe or North America, we may obtain an instructive estimate of the comparative richness of their respective floras; but if we compare *unequal areas*, and then endeavour to equalise them by getting the proportions of species to area, we shall obtain erroneous results, which will become literally absurd when the areas compared are very unequal.

The problem remains, how to compare unequal areas of which we possess the zoological or botanical statistics. We can only do so by equalising them, and this may not be so difficult as at first sight appears. For example, let us take the Palæarctic and North American regions, in which the species of birds are nearly equal in number, but the areas are as about seven to three. The number of the Palæarctic species have, however, been proportionately increased of late years, and if we take the western half of the Palæarctic region so as to include North Africa and Persia we shall have an area about equal to the Nearctic region, and a number of species perhaps one-sixth or one-eighth less, which will thus represent the comparative richness of these two areas. The eastern half of the region, including Japan and North China, is probably as rich as the western; while the intermediate portion is poorer in species. Combining these three portions, and taking the average, we should perhaps find the Palæarctic region about four-fifths or five-sixths as rich as the Nearctic, instead of less than one-half, as shown by the method of proportionate areas.

Whenever we know how many *peculiar* species any district contains, we can deduct its area from the total area of the region to be compared, and this number of peculiar species, from the fauna of the region; and by this means we may reduce two unequal regions to comparative equality. Again, all detached portions or islands should be omitted in estimating the comparative richness of regions, because they affect these regions very unequally. By adding Britain to Europe you increase the area without adding to the fauna, and thus make the region seem poorer; while by adding Madagascar to Africa, or New Zealand

to Australia, you add to the fauna in a greater proportion than you increase the area, and thus make the region seem richer. For a fair comparison continents should be compared with continents, and islands with islands, and these should in every case be brought to an approximate equality of area by lopping off outlying portions with their peculiar species. We shall then get results which will be instructive, and which will afford us a true estimate of the comparative richness of different countries in the several classes of animals and plants.

ALFRED R. WALLACE

Mr. Crookes and Eva Fay

IN Dr. Carpenter's eagerness to show that his statements about Mr. Crookes and Eva Fay had some basis of fact, he seems entirely to have forgotten the real issue which he has himself raised, and which is of great importance to all engaged in the study of these tabooed subjects. The question simply is, whether *any* investigation of the alleged abnormal powers of individuals, however painstaking and complete it may be, and however decisive its results, is to be branded with opprobrious epithets, without any proof of error or fallacy, but merely on the dicta of newspaper writers and alleged "exposers."

In the case before us Mr. Crookes made certain experiments in his own laboratory, in which the greatest refinements of modern electrical science were employed; and of these he published a detailed account. That is the sum total of his acts and deeds in regard to Eva Fay. Yet because these experiments have been referred to in America as indorsing Eva Fay's remarkable powers, and because some persons charge her with being an impostor, and go through an alleged imitation of her performances, Dr. Carpenter accuses Mr. Crookes of encouraging "disgraceful frauds" and indorsing a "notorious impostor." Now it is clear that, to support this accusation, Dr. Carpenter must *prove* that Eva Fay was an impostor in respect to what happened *in Mr. Crookes's house*, and that, to use Dr. Carpenter's own words, she evaded his "scientific tests" by a "simple dodge." He must prove that Mr. Crookes exhibited culpable carelessness or incapacity in accepting, as conclusive, tests which were really fallacious; for, otherwise, how can Mr. Crookes be held responsible for anything which happened afterwards in America? Dr. Carpenter has promised to do this in the forthcoming new edition of his lectures; but as the accusation against Mr. Crookes has been made in the pages of NATURE, and the question is a purely scientific one—that of the absolute completeness of the test of "electrical resistance"—I call upon Dr. Carpenter to explain fully to the readers of NATURE the exact particulars of that "simple dodge" which is to destroy Mr. Crookes's reputation as a physical experimenter, and to sustain the reputation of his accuser. Unless the explanation is so clear and conclusive as to satisfy *all* the witnesses of the experiments that Eva Fay *did* evade the scientific tests, and that what they saw was simple conjuring, then Dr. Carpenter is bound to find a conjuror who will submit to the same tests as Eva Fay did, and produce the same phenomena before the eyes of the witnesses, so as to show "how it is done." Mr. Maskelyne, who professes to have exposed Eva Fay, will of course be ready to do this for an adequate remuneration, which I feel sure will be forthcoming if Dr. Carpenter is proved to be right and Eva Fay's "simple dodge" is clearly explained.

I have already shown (in this month's *Fraser*) that the supposed *exposure* of Eva Fay in America was no exposure at all, but a clumsy imitation, as will be manifest when it is stated that the exposer, Mr. Bishop, performed all his tricks by *stretching the cord* with which his hands were secured to the iron ring behind his back! There is hardly a greater exhibition of credulity on record than Dr. Carpenter's believing that *such a performer* proved Eva Fay to be an impostor and Mr. Crookes's experiments valueless. But what can we expect when we find a *Daily Telegraph* report quoted as an authority in a matter of scientific inquiry?

I venture to think that, whatever may be their opinions as to the amount of *fact* in the phenomena called "spiritualistic" (by Dr. Carpenter, but never by Mr. Crookes), all men of science will agree with me that Dr. Carpenter is bound to *prove by direct experiment* that Mr. Crookes and his coadjutors were the victims of imposture on the particular occasion referred to; or if he fails to do this, that he should in common fairness publicly withdraw the injurious accusations he has made against Mr. Crookes and all who are engaged in similar investigations. If this is not done it is equivalent to deciding that no *possible* proof of such phenomena is admissible—a position which is not that of Dr. Carpenter, or, as far as I am aware, of the scientific world generally.

I beg to take this opportunity of apologising for my involuntary appearance under false colours in this month's *Fraser*. The letters "F.R.S." were added to my name after the corrected proofs left my hands and wholly without my knowledge. I have desired the editor to make a statement to this effect in his next issue, but in the meantime wish to set myself right with the readers of NATURE.

ALFRED R. WALLACE

Nocturnal Increase of Temperature with Elevation

WITH reference to the article in NATURE, vol. xvi. p. 450, on the above subject, allow me to place on record the following facts. On the night of January 7, 1874, in Lucknow, the temperature fell considerably below the usual. The minimum thermometer on the grass at the observatory registered 5° below freezing point. The destruction of plants in the Horticultural Gardens was great. Plantains, pine apples, sugar-cane, mango trees, casuarinas, pomsettias, colvilleas, bugainvilleas, &c., &c., were all injured; some killed outright. The remarkable fact which I observed on that occasion was, that the destruction of vegetation was only up to a certain height, viz., up to between seven and eight feet from the ground. Above that, not a leaf was touched by the frost. On the mango trees especially, which were planted close to each other, it was very remarkable to see a distinct line of destruction along the trees, of seven or eight feet from the ground. This, I think, distinctly showed that the temperature on that night, above eight feet from the ground, was decidedly warmer, and thus protected all vegetation, while all below it was more or less injured, or killed by frost. Other observations, I made lately, corroborate the result of the direct observations made by Mr. Glaisher. During the commencement of October there were several rainy days, with an easterly wind; the total rainfall was under $2\frac{1}{2}$ inches. When it ceased, and the clouds cleared away, I observed the following:—Before seven o'clock in the morning there were only a few low-lying clouds to be seen. As the sun rose, the wind still in the east and almost a calm, clouds began to form in all directions; about noon, and till about 3 P.M., the sky was thickly studded with cumuli of various sizes. After that hour, wider and wider gaps began to form between the clouds, and the dissolving of the cloud-masses continued as the sun approached setting. About two hours after sunset there was scarcely a cloud to be seen, and the twinkling stars came out in their full brilliancy. This melting of the clouds after a certain hour, and completely so after sunset, would, I think, indicate that the cloud region after sunset became decidedly warmer than it had been during the day.

E. BONAVIA

Lucknow, October 22

Expected High Tides

MR. EDWARD ROBERTS in his letter has, I think, missed the chief object I had in addressing you. I did not complain that the authorities had not taken pains to calculate the heights of the tides, but that while one could take up almost any paper on the coast and find the heights of the tides of the place for the coming week, not one of the London papers, so far as I could find, supplied this information for its readers. What I felt to be a desirable thing was that the Meteorological Office, or some other constituted authority, should send to the daily papers warnings, when necessary, that on such a day a dangerous tide might be expected with a wind from such a quarter and with such a barometer, as the tide would be unusually high under even favourable weather—in fact, give a forecast of the tide.

It is almost useless to ask the public or vestries to put two or three facts together and think out the matter for themselves; they require some authoritative announcement to prepare for danger. And this is the more necessary as an overflow of the Thames at above-average spring tides is, as Mr. Roberts says, now a matter of meteorological circumstances only, and on account of the increased range of the tide in the river.

I was not aware that Captain Saxby had predicted high tides so far back as 1869. If, as Mr. Roberts says, the Astronomer-Royal wrote re-assuring the public that there was nothing extraordinary in the November 3 tide, and as, on the contrary, that tide rose 3 feet 3 inches above Trinity high-water mark, this incident may possibly have had something to do with the establishing of Captain Saxby's reputation with the public as a predictor of tides,

especially if he had previously predicted the great tide of March 1 of the same year, which rose to 3 feet 7 inches above the mark.

With respect to the actions of the planets, I did not refer to the ordinary tide-producing power, for on working that out some years ago for one of the planets I was somewhat surprised to find that the height was, I believe, a fraction of an inch. I referred rather to the action of that storm-producing power which apparently gives rise to the great atmospheric disturbances at certain times (and, indeed, more or less at all times) in the sun, and by sympathy, or even directly, in our atmosphere.

B. G. JENKINS

Diffusion Figures in Liquids

PROF. MARTINI describes his diffusion figures as being "both new and singular." In the *Phil. Mag.* for June and November, 1864, I have described and figured various examples of what I call "the submersion figures of liquids" in continuation of a series of papers commenced in 1861 on "The Cohesion Figures of Liquids," or those assumed by liquid drops when delivered to various surfaces. Some of these figures are identical with those given by Mr. Worthington in the *Proceedings* of the Royal Society for 1876, and recently in your pages. C. TOMLINSON

Highgate, N., December 3

Bees and Flowers

IN last Thursday's impression (p. 62) is a letter from Mr. H. O. Forbes, referring to bees confining their visits to plants of one kind during each excursion, and thus in a measure preventing hybridisation of plants, &c.

This may be the general habit of bees, but it is not invariable; some bees, more especially their females, are to be found at certain plants only, as *Andrena hattorfiana*, at the scabious *Colletes succincta*, at the heath, and many others in like manner.

I have collected bees for several years, and have often taken them with the pollen-grains varying from orange-red to almost white, and this mixture on the same leg. I have inclosed a slide of pollen-grains which I washed from the leg of an *Andrena nigro-ænea*, and mounted in balsam; this shows several very distinct kinds of pollen; this was mounted in 1875, and at the time I gathered such of the wild flowers as were then in bloom, and compared the pollen. I was able to identify several of them, but as I made no notes I cannot say which. I would advise such observers as intend investigating this very interesting subject, to capture the insects and examine the pollen which may be found on them; this will be difficult in the case of the Bombi and Apis, as they knead it into pellets, but with those which collect on the belly or whole leg it will be easy enough.

Norwich JOHN B. BRIDGMAN

Hearing in Insects

MY daughter bred this summer a number of the larvæ of *Sphinx ligustri* and *Metopsilus elepenor*, and I was much struck with the extreme sensitiveness to the sound of the voice—especially of the former. The child's treble I observed did not affect them so sharply; but at the first word I uttered they invariably started, and remained some time motionless, with head drawn back, after their manner. I was disposed to attribute it to the vibration set up in objects around by sounds toward the deeper end of the scale, as I have felt a form tremble under me at the deep bass notes of a strong singer; but it had all the appearance and effects of hearing. HENRY CECIL

Bregner, Bournemouth, December 1

A ZOOLOGICAL STATION FOR THE CHANNEL ISLANDS

SOME definite prospect at length presents itself of the establishment within British waters of an institution long recognised as a leading desideratum among our Biologists, Museum-Conservators, and Natural History Students, namely, a building with the necessary appurtenances suitably situated, and founded on a somewhat similar basis to that of Dr. Anton Dohrn's noted Zoological Station at Naples, or the Anderson School of Natural History at Penikese Island, Buzzard's Bay, U.S. This long-felt need will be met by the proposed "Channel Islands' Zoological Station and Museum, and Institute of Pisciculture" described at some length in the advertising columns of this journal, and the establishment of which, or a similar institution, has been the guiding star and main object of the writer's ambition during the several years' "apprenticeship" spent by him as Naturalist and Curator to the various leading public aquaria of England.

Successfully carried out, the more prominent features of this undertaking will comprise, as at Naples, in addition to an attractive public exhibition of the living inhabitants of the surrounding waters, laboratories fitted with tanks, tables, and all the necessary instruments and apparatus requisite for the satisfactory prosecution of marine biological research, supplemented by a library replete with the standard scientific works and serials mostly in demand by those occupied in such investigation. Under the same roof it is likewise intended to establish a natural history museum accessible to the public, and more essentially illustrative of the notably rich marine fauna and flora of the Channel Islands. In connection with the library and museum departments popular lectures upon natural history subjects will from time to time be given. Following the system productive of such gratifying results at the Penikese Island Station, it is further proposed for the full development of the scientific advantages of this institution to institute summer classes for the attendance of students, and to hold out sufficient inducements for the most eminent authorities on various biological subjects to deliver lectures and a course of instruction to these classes upon that branch of natural history with which their reputation is more especially associated.

An entirely novel feature to be incorporated with the Channel Islands' Zoological Station will be a department relegated to the conduct of experiments associated with the—in this country—little developed science of economic pisciculture, and in which department it is proposed to award a prominent place to the artificial rearing of lobsters. Experiments made in this direction by the writer some years since at the Manchester Aquarium have decisively shown that the artificial culture of these crustacea on an extended and systematic scale might be developed into a highly important and remunerative industry. In the experiments here referred to it was found that the little lobsters occupied from six to eight weeks in passing through those singular free swimming larval conditions, known respectively as the "Zoea" and "Megalops" stages, antecedent to their assumption of the adult and ambulatory form, and during which short interval they exuviated or cast their shells many times. These initial metamorphoses safely past, their further development to a marketable size, is a comparatively easy task. The scientific culture of the oyster and other edible species will likewise receive attention in association with this undertaking.

The appropriateness of Jersey as a site for this intended Museum of Pisciculture and Zoological Station is at once apparent, the variety and exuberance of the marine fauna of the Channel Islands being such as to assimilate it more closely to that of the Mediterranean than any other one within British waters. The occurrence on the Channel Islands' coast of the Sea Horse (*Hippocampus*), Urchin-fish (*Diodon*), Remora (*Echeneis*), Electric Ray (*Torpedo*), and Lancelet (*Amphioxus*), among the vertebrate group; and of the *Haliotis*, *Scyllarus*, *Comatula*, *Physalia*, *Velella*, *Lucernaria*, and many others among the invertebrate section, are a few from among many that might be named in demonstration of this fact. The sponge-tribe and the division of the tunicate might be likewise specially singled out as attaining upon the shores of these islands a development in both numbers and variety rarely if anywhere else excelled. Unprecedented facilities for the collection of all such marine productions are also afforded by the extraordinary

low limit to which the water recedes during the monthly spring-tides. In no case less than thirty, and not unfrequently more than forty feet represents the vertical height of the rise and fall of the tide on these occasions, the waves on their retreat exposing to view and rendering accessible an extent of rocks and life-teeming pools that constitute a veritable elysium to the marine zoologist or botanist. The situation of Jersey, again, is such as to render it not only readily accessible to English naturalists and students, accompanied with just that amount of sea-passage requisite to satisfy the marine predilections of our countrymen, but it is also most conveniently reached from France, Belgium, Holland, and other Northern European countries, and which will thus invest the institution with international utility. Paris, indeed, already supplies a considerable number of the numerous summer visitors to the island, and from these no doubt might be enticed a strong contingent of students for the laboratories.

As will be found in the advertisement already referred to, a special appeal is addressed to the scientific section of the community rather than to the general public for the funds required for the successful establishment of this institution, and it is certainly most desirable that an enterprise calculated hereafter to confer so great advantages upon this more limited class should receive a fair quota of support through its ranks. The sum total required, in fact—5,000*l*.—for the founding of this zoological station, and all accessory departments, is so comparatively small as to place it not quite beyond the pale of hope that sufficient enthusiasm to effect the purpose may be yet forthcoming from among the more wealthy devotees to the shrine of science, and in emulation of the praiseworthy example set on the other side of the Atlantic by Mr. John Anderson, the munificent founder and endower of the Penikese Island Station. At all events, it is scarcely to be anticipated that so desirable an undertaking, replete with such promise of future advantage to the scientific world, will long lack the essential "sinews of war," considering that a contribution by each member of one only of our leading metropolitan scientific societies of less than one-half of his annual subscription to that society, would more than suffice to defray the whole expenditure contemplated. Through the kind liberality of a few, moreover, and the financial confidence of others, a small but substantial nucleus has been already formed, and it is confidently hoped that the full sum needed may yet be raised in time for naturalists and the public generally to participate in the advantages the Channel Islands' Zoological Station and Museum of Pisciculture will place at their disposal, so early as the summer of the year 1878.

In conclusion it is perhaps desirable to note that in drawing up the legal foundation of this Channel Islands' institution the strictest care has been taken to permanently exclude all possible chance of the society's premises being used for any of those attractions of an entirely irrelevant and unscientific nature more usually associated with exhibitions of the living inhabitants of the ocean, and the existence of which must ever constitute an insuperable barrier to that good service to science which these last-named establishments might otherwise contribute. It is only under such restrictions as are above set forth that patronage and support are solicited. In recognition of the purely scientific status of this enterprise, the members of the Executive Committee, or Directors of the Society, have also unanimously resolved to accord their services as such members gratuitously; and it is further proposed, so as to divest the undertaking of any merely speculative aspect, that all profits arising from the business of the Society, beyond what would yield to the shareholders a return of five per cent., shall be devoted to the further development of the institution, or otherwise towards the aid and promotion of scientific research.

St. Helier's, Jersey W. SAVILLE KENT

GERMAN UNIVERSITIES

THERE have been comparisons made recently both in this and in other journals between the Universities of Germany and those of this country, and as the university question is at present giving rise to much discussion, it may be useful to give some statistics with reference to the former. Such statistics are much more easily attainable for Germany than for England, as there are two German publications in which all the important information concerning the various universities of the empire is systematically arranged, viz., the *Deutsche Universitäts Kalendar* and the *Deutsche akademisches Jahrbuch*. To obtain similar information concerning the universities of the United Kingdom it would be necessary to obtain a copy of the calendar of each university. Our statistics are obtained from the *Jahrbuch*, which contains information not only relating to the universities, but also to the technical and high schools, learned societies, and libraries of the country. Some such publication is wanted here, and might be made to include not only our various universities and colleges, but also our principal public schools. The *Jahrbuch* includes, moreover, the Russo-German University of Dorpat, the Universities of Vienna, Graz, Innsbruck, Prague, Czernowitz, Basel, but these we shall not take into account.

Germany has in all twenty-one universities, each complete in all departments. The number of students matriculated and non-matriculated attending each, mostly in the 1876-77 semester was as follows:—

	Matriculated Students.				Non-matriculated.	Total.
	Theology.	Law.	Medicine.	Philosophy.[1]		
Berlin	139	1003	281	1067	2107	4597
Bonn	163	200	118	312	36	829
Breslau	107	377	165	458	15	1122
Erlangen	136	37	102	147	—	422
Freiburg	41	64	128	60	36	329
Giessen[2]	—	—	—	—	10	331
Göttingen	71	324	122	474	—	991
Greifswald	32	89	235	142	9	507
Halle	190	150	103	439	16	898
Heidelberg	9	410	101	215	60	795
Jena	66	101	71	201	20	459
Kiel	47	14	73	78	11	223
Königsberg	44	186	127	264	10	631
Leipzig	328	1102	364	1182	113	3089
Marburg	49	65	104	164	4	386
Munich	75	357	440[3]	408[4]	—	1280
Münster	208	—	—	223	—	431
Rostock	24	35	31	54	—	144
Strassburg	49	211	178	236[5]	26	700
Tübingen	295	251	138	335[6]	6	1025
Würzburg	150	93	547	328	22	1040
	2223	5069	3428	6787	2501	20229

Thus, then, there are about 18,000 matriculated students attending the twenty-one universities of Germany, under a teaching staff of about 1,300 paid professors, besides about 450 privat-docenten. Of the students, about one-third belong to the philosophical faculty, the faculty in which the sciences are included. Unfortu-

[1] In "Philosophy" are included the physical and natural sciences.
[2] The Giessen students are divided into Hessian and non-Hessian, not according to faculties.
[3] Including 100 students of pharmacy.
[4] Including 9 students of forestry.
[5] Including 97 mathematical and natural science students, these being a separate faculty at Strassburg. The figures are for 1875-6.
[6] Including 53 students in political economy and 141 in natural science these subjects forming separate faculties at Tübingen.

nately, in very few cases is the number of students attending the scientific as distinct from the literary classes given, and only in one or two universities has science as yet been erected into a separate faculty. If we may take the two universities, Strassburg and Tübingen, in which natural science forms a separate faculty as a criterion from which to judge of the number of students of science in the other universities, the proportion must be very large. In Strassburg, of the 236 students whom we have placed in the philosophical faculty, ninety-seven are students of science, and in Tübingen 100, or something like one-third of the whole philosophical faculty. Or again, if the number of science students is at all in proportion to the number of science-teachers, the position held by science in German universities is in striking contrast to its position in our universities and colleges. Of the professors, among whom we do not count the privat-docenten, about one-half belong to the philosophical faculty, and of these again, nearly one-half are teachers of science, that is, in the philosophical faculty of the German universities there is one teacher on an average to every ten students, and in science the proportion is considerably greater. In these estimates we do not take account of the medical faculty, in which, in most of the universities, there are several chairs which might well be classed as belonging to science generally.

For example, the well-known anthropologist, Dr. Virchow, the conclusion of whose address at the German Association we give this week, is Professor of Pathology at Berlin, and has been able to bring the results of his special medical line of investigation to bear, in an important way, upon his anthropological researches. Both in Berlin and elsewhere, other names of eminent medical professors might be mentioned who have not only themselves made important contributions to science, but under whom students are encouraged to do so likewise.

Of the nature and extent of the scientific teaching in German universities some idea may be formed from the subjects represented by the teaching staff at Berlin, which may fairly be taken as a type of the whole. In Berlin then we find that there are (excluding the privat-docenten) five professors of mathematics, two of astronomy, seven of chemistry, five of physics, three of geology, four of botany, two of zoology, one of meteorology, two of geography, one of anthropology, and one of agriculture—physiology and comparative anatomy being well represented in the medical faculty, and we might well have included among teachers of science those who devote themselves to the scientific investigation of languages. But a mere statement of the number of teachers gives no adequate idea of the means at the command of a German University for training its students in science. The number of teachers in each subject secures that its various departments will be thoroughly worked out, and gives a student a chance of following out any specialty he may take up; this is made still further possible by the number and variety of institutions, museums, laboratories, collections, &c., attached to each university, not to speak of its large and comprehensive library. In connection with Berlin alone there are twenty-three scientific "Anstalten," as they are called, for practical investigation in connection with the various faculties. Had we taken the numerous Realschule and the high and polytechnic schools into account, where an education can be obtained quite equal to that obtainable at most of our universities and colleges, it would have been seen that higher education in Germany leaves little to be desired.

And in reference to the subject of our leader this week, we would point to these Realschulen as embodying the German idea of what *practical* training should be. The carefully drawn-up time-tables of these schools are an instructive study, showing, as they do, that general mental culture is regarded as of the first importance in training a youth for the work of the world.

The *Jahrbuch* gives a statement of income and expenditure in connection with only one or two of the universities. Some interesting details, however, on the contributions of the State to the universities, as well as on other points, were given in a recent number of the *Academy* by Prof. Ray Lankester:—

"The sum expended by the North German States on the twenty universities belonging to them is annually more than 500,000*l.* The Imperial Government has expended upon the new University of Strassburg alone 70,000*l.* in one year. The University of Leipzig alone receives annually from the Saxon Government over 50,000*l.* There are eight universities in North Germany which are little, if at all, less costly, and there are eleven of smaller size which receive each from 8,000*l.* to 20,000*l.* annually.

"In North Germany there is one university to every two million inhabitants; in Austria there is one to every five millions; in Switzerland one for each million; in England one to every seven millions. In the twenty North German universities there are 1,250 professors.[1] In the British Islands we ought to have sixteen universities and 1,000 professorships in order to come up to the same level in this respect as North Germany. The stipend (apart from fees) of a professor in a German university ranges from 100*l.* to 600*l.* a year. As a rule, at the age of five-and-thirty, a man in this career may (in Germany) count on an assured income of 400*l.* a year (with retiring pension). The expenditure on attendants, libraries, laboratories, and officials may be calculated as being (in a well-conducted university) more than equal in amount to the total of the professors' stipends. Taking the *average* German professorial stipend at only 200*l.* a year, we find that 250,000*l.* must be spent annually on this item alone in the North German States.

"In order to equip and carry on sixteen universities in this country which should bear comparison with the German universities, we require not less than an immediate expenditure of 1,000,000*l.* sterling in building and apparatus, and an annual expenditure of from 500,000*l.* to 800,000*l.*"

When we add to the Government subsidy the income of the universities from other sources, the sum is enormously increased. The half-million, moreover, does not include the occasional grants of the Government for special purposes. Some idea of the magnificence of these was shown in our recent "University Intelligence," where it was stated that in the budget submitted to the present Prussian House of Deputies are the following items:—Erection of the German Industrial Museum, 998,000 mk.; erection of a Polytechnic in Berlin, 8,393,370 mk.; erection of an Ethnological Museum in Berlin, 1,800,000 mk.; and for the Berlin University, erection of an Herbarium, 422,000 mk.; of a Clinic, 1,955,000 mk.; of a new building for a second Chemical Laboratory, as well as of a Technical and Pharmaceutical Institute, 967,000 mk.

OUR ASTRONOMICAL COLUMN

THE METEORITE OF JULY 20, 1860.—The occurrence of the splendid meteor of November 23, which has probably been observed with sufficient completeness to allow of the determination of its path, while it remained visible, recalls a similar object which passed over the northern parts of the United States and adjacent parts of Canada, on the evening of July 20, 1860, which was made the subject of investigation by the late Prof. J. H. Coffin, of Lafayette College, N.Y. Probably no one of these remarkable bodies has been more extensively observed, and we do not remember any case where the calculations have been more laboriously conducted, and with greater hope of reliable results.

[1] *i.e.* we presume professors strictly so-called, exclusive of "privat docenten."

The "meteoric fire-ball," as Prof. Coffin calls it, was first seen moving in an easterly direction from a point nearly over the western shore of Lake Michigan, though it may have become luminous somewhat further to the west as the sky was clouded over that region. From thence it was watched until it disappeared out at sea in a south-easterly direction from the island of Nantucket. Its course was therefore about 1,300 miles, and it was seen for several hundred miles on either side of this track. Upwards of 230 descriptions of the meteor were collected, and upon the best of these Prof. Coffin undertook the determination of the orbit, by an elaborate process detailed in his memoir, which formed No. 221 of the "Smithsonian Contributions to Knowledge," entitled "On the Orbit and Phenomena of a Meteoric Fire-ball, seen July 20, 1860." The various accounts of the meteor are printed in the memoir, and reveal some peculiar points of interest in its path. There were two "remarkable ruptures of the main body of the meteor," particularly near the meridian of 77° west of Greenwich, when it separated into two parts nearly equal in size which disappeared below the horizon, as one observer describes it, like a chain-shot.

Considering that whatever might have been the orbit of the meteor before it became visible, its course while it was under observation, from being so near the earth, must have been controlled almost wholly by her attraction. Prof. Coffin mentions that the orbit he has investigated is not the path of the meteor in space, but the orbit relative to the earth, having the centre of our globe in one of its foci. Approximate elements having been obtained, azimuths and altitudes deduced from them were compared with those given by the various observations to ascertain what modifications of the elements were required in order to satisfy them. It was found that with certain corrections thus indicated the first orbit represented tolerably well most of the reliable observations to the west of 76° or 77°, near which the most easterly of the two points from which it was determined, was situated; but further to the east the discrepancies between calculation and observation were "so great that they could be reconciled only by introducing changes in the elements of the orbit, one on the meridian of 77° and another near the meridian of 74°, and as Prof. Coffin remarks, it is worthy of note that it was in the vicinity of these points that observers report the violent ruptures of the body of the meteor, which seems to afford a rational explanation of the changes of elements found to be required. It was apparent that while the meteor descended rapidly towards the earth till it reached the meridian of about 74°, it afterwards rose, and the change was too great to be accounted for on the supposition that the meteor at that point attained the perigee of its hyperbolic orbit. After the introduction of other considerations, it resulted that the path divided itself into three sections, "the first and third of indefinite length, over only a small portion of which the meteor was visible, and the second an intermediate one, 160 miles long, where it was most brilliant." The elements for the three sections, as finally adopted, are:—

	Sec. I.	Sec. II.	Sec. III.
	° ′	° ′	° ′
Long. of perigee	294 57	275 37	261 2
,, descending node...	332 56	325 11	329 24
Inclination to ecliptic	66 12	67 10	66 26
Eccentricity	2·9984	2·9817	2·9921
Major semi-axis	2005·3	2005·3	2005·3
Perigeal distance	4007	3974	3995

The major semi-axis and the perigeal distances are expressed in miles. According to these elements, Prof. Coffin concludes that the meteor entered the sphere of the earth's attraction from the direction of the constellation Sextans, in about R.A. 148°, N.P.D. 87°, and left it toward a point in R.A. 355°, N.P.D. 121°.

The Planet Mars and B.A.C. 8129.—The near approach of Mars to the seventh-magnitude-star, B.A.C. 8129, appears to have been observed pretty generally. Taking the mean place of the star from the Washington Catalogue of 1860, its apparent position on the evening of November 12 is found to be R.A. 23h. 14m. 24·37s., N.P.D. 96° 34′ 22″·5. By Leverrier's tables the place of Mars at 6h. Greenwich time and the hourly motions were:—

R.A. ... ... 23h. 14m. 24s·91 + 3s·473t.
N.P.D.... ... 96° 34′ 25″·1 − 30″·49t.

Taking account of parallax, the star at 6h. would be on an angle of 319°·4, distant from planet's centre, 17″·8, by calculation, as seen at Greenwich. Probably the actual approach was not quite so close.

The Binary-star Castor.—Dr. Doberck, of Col. Cooper's Observatory, Markree, whose investigations relating to the orbits of the revolving double-stars have been on several occasions referred to in this column, has corrected the elements of the fine binary α Geminorum, given by Thiele in 1859, by measures to 1877 inclusive. Thiele's period of revolution was 997 years, Dr. Doberck's calculation gives 1,001 years, and the comparison with observations, from those of Bradley and Pound in 1719 to the present year, exhibits no larger differences than are to be attributed to unavoidable errors, or in one or two cases, bias on the part of the observer. The new elements are as follow:—

Passage of the peri-astre	1749·75
Node	27 46 (meridian of 1850).
Node to peri-astre on orbit ...	297 13
Inclination...	44 33
Eccentricity	0·3292
Semi-axis major...	7″·43
Revolution	1001·21 years.

This orbit gives, for 1878·0, position 234°·9, distance 5″·76.

Transits of the Shadow of Titan across the Disc of Saturn.—Mr. Marth has drawn attention to the following dates of transit of the great satellite's shadow, as the only opportunities for observation until the year 1891:—December 9, about 6½h. Greenwich time, December 25, about 5¾h., and January 10, about 5h.

The "Nautical Almanac," 1881.—As usual the *Nautical Almanac* was published in November, the last volume being for the year 1881, which does not appear to be one distinguished by any particular astronomical phenomena. The two solar eclipses on May 27 and November 21, the first partial, the second annular, are both invisible in this country, and the line of annularity in the November eclipse runs at great south latitude. The total eclipse of the moon on June 11 will also be invisible here, while in the partial eclipse on December 3 (magnitude 0·97) the moon will rise at Greenwich about twenty minutes after first contact with the shadow. A transit of Mercury on November 7, will be wholly invisible in this country, the first external contact (geocentric) taking place at 10h. 16m. 13s., and the last at 15h. 37m. 41s. mean times at Greenwich. The list of visible occultations does not contain any planet, nor any star over the third magnitude. The list of standard stars is on the same scale as for the year 1880, and numbers close upon 200. The *Nautical Almanac* circulates to the extent of more than 20,000 copies, inclusive of the number appropriated for the use of the Royal Navy.

OLE RÖMER

When Newton's "Principia" raised the theory of astronomy to a height not previously dreamt of, practical astronomy was still where Tycho Brahe left it almost a century before. Such was the respect paid to

the memory of that great man that Hevel in Danzig carried out Tycho's ideas about his observatory, and rejected all the improvements that had since originated, amongst which was the application of the telescope to astronomical observations. The obstinacy with which Hevel refused to adopt this invention appears strange to us now, but we must remember the great accuracy which was then obtained by pinnules alone. Tycho had reduced the probable error of astronomical observations from ten minutes to one, and some of Hevel's observations have been found to be affected by errors of less than half a minute of arc, results which show that the old astronomers were in possession of a skill in handling their apparatus which has since been lost. It should also be taken into account that the telescopes of Hevel's day were generally of Dutch construction, and Kepler's tube, with wires in the field to mark the centre, was first brought into general use by Auzout and Picard about the end of the century. Of hardly less importance was the application of the pendulum to clocks, which from that time have been used as astronomical instruments. They had in Tycho's observatories been used merely to show what o'clock it was when observations were made, but never to determine differences of right ascension.

With sufficiently good clocks it was possible to determine the positions of the stars by observations in the meridian alone, and it was no doubt Picard who first became aware of the immense advantage of this. Consequently he solicited Government for a large mural quadrant, but Cassini was then called in from Italy, and no notice was taken of the request made by Picard, who, unfortunately for the practical astronomy of France, was not thought much of by the court of Louis XIV., his important, but modestly-conducted researches being eclipsed by Cassini's brilliant discoveries. Had Picard got the direction of the Royal Observatory in Paris he would have been able to make further improvements in the construction of instruments; but with no sufficient means at hand, he ascribed the partial failure of his attempts to the small size of his instruments. A mural quadrant like Tycho Brahe's, but furnished with a telescope, was first fixed at the observatory when Picard died.[1] Flamsteed and Sharp adopted the methods just as Picard left them and with all their drawbacks. They used the quadrant both for right ascensions and declinations. Their observations may perhaps be said to be twice as accurate as Hevel's naked-eye observations.

This was the state of practical astronomy when Römer raised it to a height which was not surpassed before Bessel. Ole Römer was born in Aarhus on September 25, 1644. Thence he came, 1662, to Copenhagen, where he studied mathematics and astronomy under Erasmus Bartholin, whom he subsequently assisted. As has already been pointed out, Tycho's observations continued to be consulted by astronomers, and in 1671 Picard went to Denmark to determine the difference between the longitude of Uraniburg and Paris.[1] There he found Römer occupied in revising Tycho's manuscripts, and he secured his assistance in the observations on Hveen, and when Picard returned to France he procured Römer a place as assistant at the observatory of Paris. There his talents did not fail to be appreciated, and he was soon elected a member of the Academy. It was in Paris that Römer discovered the gradual propagation of light from observations of the eclipses of the first satellite of Jupiter. His results, which were not very exact, were however, contested by Cassini and most other authorities for a long time after. Indeed, the theory of the motion of the satellites was at that time so little elaborated, that similar conclusions might be questioned all the more as they had been deduced from observations of the first satellite alone. Römer shortly afterwards discovered the epicycloid, and published a paper on the proper form of toothed wheels, and descriptions of a Jovilabium and Saturnilabium; he afterwards invented different kinds of

QVADRANS MVRALIS

SIVE TICHONICUS.

FIG. 1.

[1] This interesting instrument is represented in Fig. 1. It was cast wholly in brass and fixed with strong iron screws as exactly as possible in the meridian in the south-western room of the ground-floor of Uraniburg. Its radius was about six feet, and it could by means of transversals be read off to ten seconds of arc. It had in a hole in the south-western wall in the centre a gilt cylinder and two pinnules movable along the edge, which were so constructed that the slit could be opened or closed more or less according to the faintness or brightness of the objects to be observed. Tycho Brahe, who, in contradistinction to Ole Römer, was not only anxious about the quality but also about the appearance of his instruments, had ornamented the large empty space of the quadrant with the splendid picture shown in the plate. He is here depicted in his usual attire. At his feet is lying one of his favourite hunting-dogs, more as a symbol of ingenuity than as a symbol of nobility. Behind him are small portraits of King Frederick and Queen Sofie. This was painted by John of Antwerp and is more like him than any other image, but the space contained also an architectonic picture by Steenvinkel, somewhat reduced as if at a distance. In the upper story are represented some of his most celebrated instruments, in the middle story the library inside with the large celestial globe and his pupils occupied with their studies, and in the cellar the chemical laboratory. On the plate is seen a young man observing through one of the pinnules, another is watching the clocks, while a third is noting down their remarks at a table. Tycho Brahe's image seems as if admonishing and instructing them in their work.

[1] On this occasion fire-signals were for the first time made use of for the determination of longitude. A fire was lit on the top of the astronomical tower in Copenhagen. There Picard eclipsed it at regular intervals, and the moment the light disappeared was noted by the observers on Hveen.

planispheres. He was, in 1679, sent by the Academy to London, to examine the English determination of the length of the second pendulum. He took part in the levellings necessary for conducting water to Versailles,

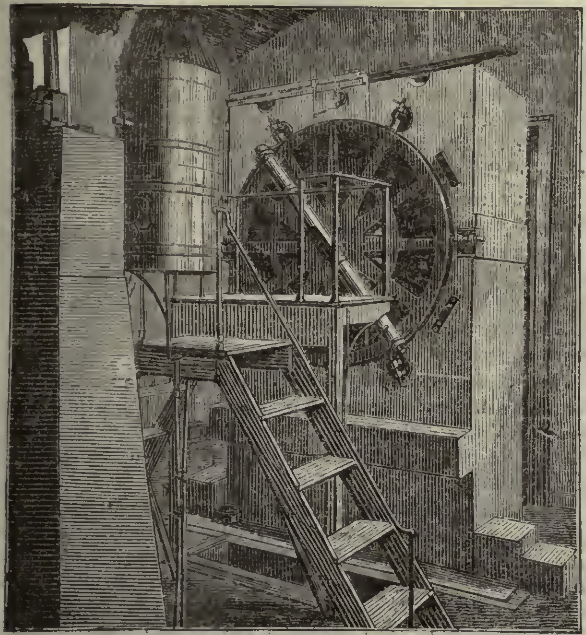


Fig. 2.—Mural Circle, U.S. Naval Observatory.

which gave him occasion to write several interesting papers. He made many observations in Paris. Already in 1671 he had taken part in the observations of the altitude of Mars, which was simultaneously observed by Richer in Cayenne, for the determination of the parallax of the sun. His fame increased so much that he was made tutor to the Dauphin, and in 1681 Christian V. called him to Denmark as Astronomer-Royal. His great technical knowledge made him useful to that country in many ways, and we see him in succession as Professor of Mathematics, Mayor of Copenhagen, Master of the Mint, Prefect of the Police, Privy-Councillor, and one of the Judges of the Supreme Court, in all of which capacities he left behind a lasting fame. He was one of the first who recommended Protestants to adopt the calendar as reformed by Gregory XIII. He had also to make a journey in 1687 to acquaint himself with the latest progress in naval architecture and pyrotechnics. We cannot fail to respect his perseverance when we hear that, notwithstanding so many different occupations, he left behind about as many observations as Tycho Brahe himself. But these were mostly all lost by the great fire which devastated the town in 1728.

Römer found in Copenhagen the old observatory of Longomontanus on "the round tower" almost devoid of instruments, and it was first in 1690 that two were placed there. One of these was not unlike a modern equatorial, and intended for extra-meridian observations; but it was generally clamped in the meridian, and used as a transit circle. The other was a vertical circle for taking corresponding altitudes, a method much used by Picard. The position of these instruments on the top of the tower (over 100 feet high), where the observations had to be made under the open air, rendered their use, however, so inconvenient to the observer, that Römer about the same time arranged an observatory in one of the windows of his dwelling house. Here was placed the transit instrument which Römer invented, but it was greatly inferior to the instrument he afterwards constructed. The telescope was not fixed in the middle of the horizontal axis as in modern instruments, but near one end. The axis, which rested on iron supports in the wall, was a long and thin iron bar, furnished with a counterpoise acting in the middle, to prevent flexure. The tube itself was cone-shaped for the same reason. In the focus were drawn a horizontal and a number of vertical wires. The interval between the three he generally used was thirty-four seconds in the equator, and the time was noted to half seconds. The field was illuminated by means of a polished ring placed outside of the object-glass. The circle was not movable with the telescope but fixed to the wall, and the telescope carried with it a microscope fixed upon an arm for reading the declinations. The arc was divided to ten minutes and in the microscope were eleven wires, each one minute distant from its neighbour. The minutes were read thus and could be subdivided by estimates to about four seconds. The instrument being placed in a window Römer could only observe the stars of between twenty-eight south and forty degrees north declination, and the arc was therefore not a whole circle but merely about seventy degrees. The error of collimation was corrected by reversion. The azimuth was ascertained by comparing the observed error of the clock with that determined by corresponding altitudes. It was at this observatory that Römer tried from observations of the right ascensions of two bright stars on opposite sides of the sky, to determine the sum of their parallaxes.

But these arrangements did not long satisfy Römer, and in 1704 he built, at his own cost, the "Observatorium Tusculaneum," seventeen feet long and broad, near the village Vridlösemagle, between Copenhagen and Roeskilde. The principal instrument of this observatory was a meridian circle, and the stars were observed through a very narrow opening in the ceiling and the walls running

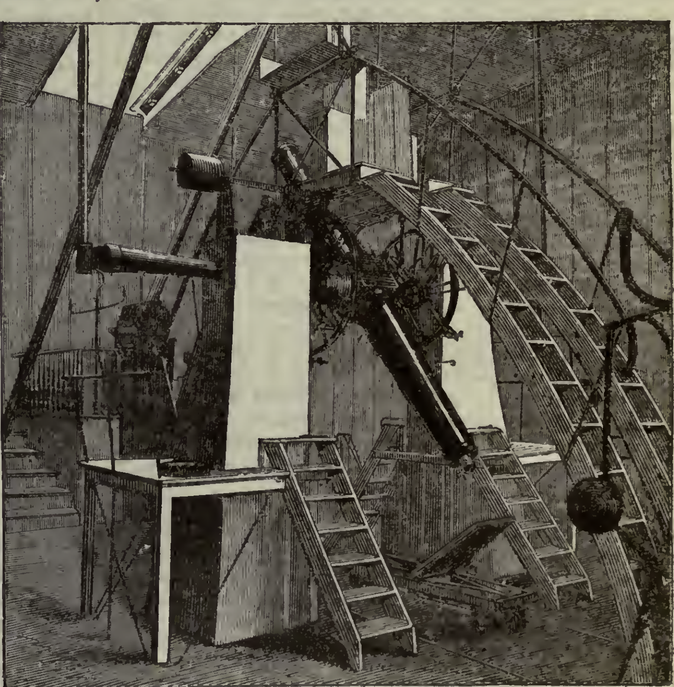


Fig. 3.—Meridian Circle, U.S. Naval Observatory.

from north to south, which was closed with shutters when not in use. The axis was made of conical iron plates lighter and more inflexible than in the old transit. The pivots were conical and fitted into brass holes on the sides of the pillars, into which the axis was tightly pressed with screws. The axis could be shifted a little both in altitude and azimuth. Römer had afterwards occasion to regret that the instrument was supported on wooden pillars and not on stone. The tube was not fixed immediately to the axis but to the circle. It was five feet long, and allowed stars of the second magnitude to be observed during the day. It had three horizontal wires in the focus and seven vertical; the intervals between these were twenty-four seconds in the equator, and the time was noted to a fraction of a second. There were three good clocks in the observatory. The circle which was fastened to one end of the axis was about five feet in diameter. It is not unlikely that Römer afterwards considered a smaller size preferable. He disapproved altogether of the use of the quadrant and sextant, and said that a circle of four feet was superior to a quadrant of ten. This circle had been divided to ten minutes with Römer's own hands, and in the microscopes three seconds were easily discerned. It was read by two microscopes fixed side by side to one of the supports of the axis. Before the observations were made the circle was ascertained, by means of a plumb-line, to be vertical. The errors of collimation and azimuth were determined in the same way as with the old instrument, and Römer had fixed two meridian-marks, which were besides used to discover whether the microscopes had changed their position. Römer was the first who determined the azimuth from culminations of circumpolar stars above and below the pole.

Besides this instrument Römer had also a transit instrument placed in the first vertical, but that was not used much because it had been so badly made by the workmen that it disturbed the meridian circle with which it had one of the supports in common. Römer intended to observe declinations of fixed stars with it and compare them with those observed in the meridian, and thereby determine the refractions. He would also have observed the sun with it.

After Römer's death, on September 19, 1710, his observatory was neglected and the instruments were spoiled, when at last they were sent to Copenhagen. Römer was to have published a description of the observatory and his methods, but was prevented by the illness which terminated his active career, and the descriptions were afterwards given from memory by his little gifted pupil and successor, Horrebow, who did not fully understand all the precepts of his great master. All his observations and instruments were ultimately destroyed by the conflagration of the observatory in 1728, except three days' observations, which Römer had intended to use for his description of the instruments. Thus it came to pass that this great genius passed away without any adequate influence upon the progress of the science. These three days' observations have been carefully reduced by Dr. Galle; their accuracy is shown to be almost equal to that attained at the present day.

In England the methods adopted by Flamsteed were followed until Bradley permanently introduced Römer's transit at Greenwich. He continued, however, to use the quadrant for declinations, and in most other observatories of this country the right ascensions and declinations continued to be observed with different instruments. We may also trace to this circumstance the immovable heavy mural instruments so common in this country.[1] The French astronomers adhered to Picard's methods until lately, and used quadrants even for the right ascensions; the transit in the first vertical was not used before it was rediscovered by Bessel. On the whole we may say tha no observatory fully expressed Römer's ideas before Bessel's and Struve's practical talents had altogether changed the face of the science. W. DOBERCK

[1] On the accompanying plates are represented one of the formerly more common mural circles (Fig. 2), and also a meridian circle (Fig. 3); both instruments of the U.S. Naval Observatory, Washington.

NOTES

PROF. KIRCHHOF has been created a Knight of the Order of Maximilian for Science and Art, by King Louis of Bavaria.

M. BRUNET, the late French Minister of Public Instruction, nominated M. Gramme, the inventor of the well-known machine for generating electric light, a Chevalier of the Order of the Legion of Honour.

NEARLY 200*l.* have already been promised for the Darwin Memorial Fund at Cambridge.

A MONUMENT was inaugurated on November 23 at Rouen in honour of M. Pouchet, the celebrated naturalist, who organised the Rouen Museum in 1828, and died director in 1872. M. Pouchet was a correspondent of the French Institute. He was a supporter of the theory of spontaneous generation.

THE Rhine Provincial Museum in Bonn has succeeded in purchasing the famous collection of prehistoric remains from the Neander Valley, hitherto in the possession of the late Prof. Fuhlrott, of Elberfeld, although a high price had been offered from England.

PREPARATIONS are being made at the Champ de Mars, Paris, for executing Foucault's pendulum experiments on an enlarged scale. His apparatus was suspended in 1851 under the dome of the Pantheon. It was in operation for a long while and removed only when the building was transformed into a church after the *coup d'état* in 1852. The weight of the pendulum will be 300 kilogrammes, and it will oscillate at the end of an iron wire from 65 to 70 metres long. Thus a special construction will be required for its suspension. The pendulum will be suspended above a grooved pipe which will move freely on an axis in its centre. The pendulum in oscillating will displace this pipe, which will remain, like the pendulum itself, fixed in space, in reference to the constellations. Underneath the pendulum will be arranged a large terrestrial globe, from 25 to 30 metres in diameter. This globe, resting on the ground, will necessarily follow with the spectators the movement of the earth. The pipe, on the contrary, supported by a pivot at the extremity of the axis, will carry large indexes, which will appear to be displaced with it. The globe, which will represent the earth, having a considerable volume, the movement of these indexes will be visible; it will render tangible in some degree to the least attentive, the rotation of the planet on its axis.

IN the *Times* of Monday is a pleasant leader on the Royal Society *à propos* of the anniversary last Friday. The article contains nothing striking, the drift of it being that the Royal Society has done much to foster science, but that science never was altogether, and is now not at all, dependent on the Royal Society for its progress—which is probably true. The article concludes with a strongly-expressed desire to see literature, "the old learning," recognised by the Royal Society, that, in fact, it should be turned into a sort of academy, after the pattern of that of Paris. But practically the French Academy is a collection of societies, one of which, like the Royal Society, devotes itself wholly to science.

AN article in Tuesday's *Times* describes some experiments which are being made at the Fulham gas-works in the lighting of lamps by electricity. The patent is that of Mr. St. George Lane Fox, the distinctive feature being an electromagnetic apparatus attached to each lamp, and connected with a central station, at which an electric current is generated. If the experiments prove successful and the apparatus is adopted, a great saving is likely to be effected. All practical difficulties seem, however, to have been solved in America Electricity

has been tried for the purpose of lighting and extinguishing 220 street lamps in Providence, R. I., scattered over a district nine miles long. One man attends to the whole business and does it in fifteen seconds. The method has now been on trial for some months, and a saving of ten dollars per lamp per year is reported.

As might have been expected, Mr. Stanley has been received with unbounded enthusiasm at the Cape, and his followers petted and loaded with presents to such an extent that they must feel amply rewarded for all their labours. Mr. Stanley in his lecture at Cape Town, reported in the *Times* and *Telegraph*, went over all his journey again, and defended himself stoutly against the criticisms which have been made on his conduct. He is expected in England about Christmas. The United States House of Representatives are to pass a vote of thanks to Mr. Stanley, and the King of Italy is to present him with a gold medal. Would it not be just to recognise, in some public manner, the great service rendered to geography by the organisers of the expedition, the proprietors of the *Telegraph* and *Herald*?

COL. W. H. REYNOLDS has concluded a contract with the English Government by which the Post Office Department has adopted the Bell telephone as a part of its telegraphic system. In a recent telephonic experiment in connection with the cable 21¾ miles long, between Dover and Calais, there was not the slightest failure during a period of two hours. Though three other wires were busy at the same time, every word was heard through the telephone, and individual voices were distinguished. This important experiment was conducted by Mr. J. Bourdeaux, of the Submarine Telegraph Company. Some very successful experiments were made with the telephone on Saturday night between Aberdeen and Inverness, a distance of 108 miles. Songs and choruses were distinctly transmitted, and conversation was carried on at times with marvellous distinctness, notwithstanding the weather was unfavourable. The experiments were made with Prof. Bell's instruments. The Berlin correspondent of the *Daily News* states that a Berlin house is making a number of telephones for experimental use in the Russian army. The result is awaited with great curiosity in military circles. The *Cologne Gazette* denies that any telephone is in existence between Varzin and Bismarck's office at Berlin. Our contemporary says that the distance, 363 kilometres, is too large for using a telephone with any advantage.

ON Dec. 1 the council of the Paris Observatory held its second meeting for deliberating upon the improvements to be suggested to the Government. The existing regulations had been printed and distributed among members, who discussed them article by article, in order to better understand their bearing. M. Faye, the present Minister of Public Instruction and one of the councillors, did not resign his office. He merely intimated to his colleagues that he should not take part in the discussions so long as he should be obliged to remain a minister for the welfare of the commonwealth. Consequently it may be considered as certain that the Assembly will come to no conclusion so long as the political crisis does not permit the learned astronomer to resume his usual labours. M. Faye, whose voice will have great weight, is a strong supporter of the existing connection between astronomy and meteorology.

THE Society of Apothecaries have decided to offer two prizes for competition by young women under twenty years of age, in the science of botany. The prizes will consist of a gold and a silver medal and books, to be awarded to the first and second candidates respectively in order of merit. The Rev. M. J. Berkeley (the examiner for the prizes given by the Society to medical students) will conduct the examinations. The date of the examination and the conditions of competition will be published shortly.

A PRIZE of 1,000 Italian lire has been offered by the Committee of the Italian Alpine Club for the best description of any Italian mountain group.

THE Horseshoe at Niagara, the *New York Tribune* states, is now a right-angle rather than a curve. The rocks in the centre have been eaten away from year to year, and now the side walls are crumbling. On November 17 a large section of rock toward the Canada shore fell with a tremendous crash, and during the night a still larger area went down. The falls now wear a new face, and visitors will undoubtedly be charged twenty-five cents extra next season.

THE Russian Government has issued an ukase according to which Novaya Zemlya is to be colonised. The Norwegian journal *Tromsoeposten* now reports that on August 28 last six Russian sailing vessels arrived at Tromsoe, carrying the necessary building materials such as timber, bricks, and lime for the construction of six houses upon Novaya Zemlya. These houses were to be constructed during the course of the present autumn and are to be inhabited by six Samojede families, who will form the first colonial residents upon the island. The Russian Government hopes by the colonisation of Novaya Zemlya to be able to establish successfully a permanent commercial communication with the mouths of the Yenisei and Obi Rivers, while at the same time the new colony may form a convenient place of exile for political criminals.

THE deepest artesian well in the world is being bored at Pesth, and has reached already a depth of 951 metres. The well at Paris, which measures 547 metres has hitherto been the first. The work is undertaken by the brothers Zsigmondy, partially at the expense of the city, which has granted 40,000*l*. for the purpose, with the intention of obtaining an unlimited supply of warm water for the municipal establishments and public baths. A temperature of 161° F. is shown by the water at present issuing from the well, and the work will be prosecuted until water of 178° is obtained. About 175,000 gallons of warm water stream out daily, rising to a height of 35 feet. This amount will not only supply all the wants of the city, but convert the surrounding region into a tropical garden. Since last June the boring has penetrated through 200 feet of dolomite. The preceding strata have supplied a number of interesting facts to the geologist, which have been recorded from time to time in the Hungarian Academy of Sciences. Among some of the ingenious engineering devices invented during the course of the boring are especially noteworthy the arrangements for driving in nails at the enormous depth mentioned above, for pulling them out (with magnets), for cutting off and pulling up broken tubes, and above all, a valuable mechanical apparatus by means of which the water rising from the well is used as a motive power, driving the drills at a rate of speed double that previously imparted from the mouth of the well.

THE preliminary works for boring the British Channel Tunnel are being prosecuted with very great activity at Sangate. A shaft has been sunk to a depth of 100 metres, and the experimental gallery has been commenced. It is to be continued for a kilometre under the sea. If no obstacle is met with the work will be continued without any further delay. Two powerful pumps have been established for elevating the water which, of course, filters in in large quantity.

IN the French estimates for 1878 a supplementary credit of 5,000*l*. is asked for the learned societies in connection with the exhibition of 1878.

AN international exhibition is to be held at Milan in 1879.

AN excellent measure was decided on by M. Brunet, the late French Minister of Public Instruction. Special maps on the

scale of $\frac{1}{10000}$ are to be designed of the country around each college, so that pupils when out walking, may be enabled to practice topography. These maps will extend to a radius of thirty kilometres from the college, and will be placed in the hands of masters.

THE Geographical Society of Paris will hold its anniversary meeting on December 19; a banquet will take place at the Grand Hotel on the 22nd.

THE administration of the Eastern Railway of France has intimated to the Geographical Society of Paris that orders will be given for inscribing on the wall of each station the altitude above the sea, the distance from Paris, the name of the chief town of the district, the name of the department, &c., &c. Thus railway travellers will learn the geography of France *nolens volens*.

IN last week's NATURE Mr. G. J. Hinde gave some details concerning the earthquake of November 4 in Canada. The *New York Tribune* gives some interesting details concerning the same earthquake in the States as well as subsequent earthquake phenomena. The shocks were felt in the east, in the west, and in the south. Commenting upon them the *Chicago Evening Journal* makes an interesting statement about the recent active condition of a little-known volcano in Nebraska. The latest earthquake shocks, it states, which especially affected Western Iowa, and were still sharper in North-eastern Nebraska and South-western Dacota, bring to mind the fact that the "Ionia Volcano," known to a few scientific investigators of the west as existing in the high bluffs near the little village of Ionia, in North-eastern Nebraska, is directly in the centre of the area traversed by the earthquake vibrations. Being in a retired spot, miles away from any line of travel, on the west bank of the Missouri River, in a bluffy region, the little volcano has attracted the attention of only a few of those who make such subjects a study, and hence is not mentioned, as we believe, in any of the works on geography or geology. The occurrence of the earthquake, with its key or centre at the Ionia volcano, makes worthy of remark the fact that for a few months past this little American Vesuvius has been unusually active. Its vapours have arisen almost constantly, and, for the first time since white men have viewed its action, these vapours have been easily distinguishable for a dozen or more miles away. The first of these disturbances of the earth's surface was perceived on November 4 by the inhabitants of Northern New Hampshire, Vermont, Western Massachusetts, Northern and Central New York, and Canada. The course of the shocks was from west to east. They were especially violent in the Adirondack Mountains region. On November 15 an earthquake shock was felt in the States of Kansas, Nebraska, and Iowa, and in Dakota Territory. The shock was a very severe one, and its effects were perceptible in most of the cities of the States mentioned. In Sioux City, Iowa, there were two earthquake waves, the second being the most powerful and immediately following the first. There was a continuous vibration lasting forty-five seconds. In Kansas the shock was noticed at Topeka and Atchison. At Topeka, in the Santa Fé depot, the employés felt the building rocking gently from north to south. On November 16, the day following the earthquake in the west, a violent earthquake shock was felt at Knoxville, Tenn. The shock was apparently only perceived at this place in the south, as there are no reports from any other southern city of such an occurrence.

NOTHING is as yet known about the Marquis Antinori and his expedition. The news of his death, which did not emanate, we believe, from the Italian Geographical Society, may therefore be considered as premature. Matteucci, who takes a lively interest in the fate of the Antinori expedition, will probably be able to gather more precise and definite information at Khartoum.

PROF. STOPPANI, the eminent Italian geologist, has been called to occupy the chair of geology at the Instituto Superiore of Florence. He delivered his opening discourse on Saturday, November 17, and will give exclusively public lectures during the whole following scholastic year.

THE "Science Primers" by Hooker, Balfour Stewart, and Geikie, have been translated into Italian by Profs. Pedicino, Cantoni, and Stoppani, and published in nicely-bound small volumes by the editor, U. Hoepli, of Milan.

THE enormous whale captured in the Gulf of Taranto in February last, has now been studied by Prof. Capellini, who found it to be a new species, to which he gives the name of *Balæna tarentina*.

WE are informed that Dr. Forsyth Major, of Florence, intends to publish a periodical for the "Zoology and Palæontology of Vertebrata," which will contain original articles in four languages. We cannot but wish the best success to Mr. Major's interprise, which is the first of the kind in Italy or anywhere else, we believe.

A NEW and perfectly mounted meteorological observatory, under the direction of Prof. Nardi, was inaugurated on Sunday, November 25, in the Seminary of Fiesole, near Florence. The funds for the same were subscribed by the Bishop of Fiesole and the Italian Alpine Club. Another observatory will shortly be opened under the care of the latter society, at Castel Piano, on Mount Amiato, near Siena. The number of meteorological stations in Italy thus amounts to about eighty, the greater part of which have been founded on the initiative, and by the support, of the Club Alpino, who deserve every praise for their continual and strenuous efforts to further and foster the study of meteorology in Italy.

A MOST elaborate monograph has been published by a distinguished Italian geologist, Prof. Baretti, on the geology of the large Alpine group known under the name of Gran Paradiso in the Graiian Alps.

IN the *Annali di Storia naturale del Museo Civico di Genova*, the illustrious traveller and botanist, Prof. O. Beccari, describes the wonderful gallery or bower-constructions of the *Amblyornis inornata*, observed by himself in the Arfak Mountains. The huts and gardens, as built and laid out by this bird, which is called "the gardener," seem to surpass any production of intelligence and taste for the beautiful hitherto described and observed in birds of the Paradise family.

ON the very rich collections made in, and sent over from, New Guinea by those intrepid and persevering champions of science, Messrs. O. Beccari and D'Albertis, Prof. Mantegazza has completed a series of anthropological and ethnographical studies, the first part of which are now being published in the *Archivio per l'Antropologia e la Etnologia*. It may be mentioned that the museum, founded by Prof. Mantegazza in Florence contains the largest known collection of Papuan skulls, the number of which exceeds two hundred.

THE additions to the Zoological Society's Gardens during the past week include a Vervet Monkey (*Cercopithecus lalandii*) from South Africa, presented by Mr. F. H. Taylor; a Green Monkey (*Cercopithecus callitrichus*) from West Africa, presented by Mr. J. R. Phillpotts; a Spotted Ichneumon (*Herpestes auropunctatus*) from Persia, presented by Mrs. Fleuss; a Common Ocelot (*Felis pardalis*), a Scarlet Ibis (*Ibis rubra*), a Fulvus Tree Duck (*Dendrocygna fulva*) from South America, presented by Mr. George Ransom; a long-eared Owl (*Asso otus*), European, presented by Mr. W. H. Millington; three Weeper Capuchins (*Cebus capucinus*), a Squirrel Monkey (*Saimaris sciurea*) from South America, two Cheer Pheasants (*Phasianus reevesii*) from North India, purchased; a Black-footed Fox (*Canis jubata*) and an Azara's Fox (*Canis azaræ*) from South America, deposited.

THE LIBERTY OF SCIENCE IN THE MODERN STATE[1]

III.

If what I have said before is true—that half-knowledge is more or less the characteristic of all naturalists, that in many, perhaps in most, of the lateral branches of their own science, even the naturalists themselves are only half-knowers; if later on I said that the true naturalist was distinguished by his being perfectly aware of the limit between his knowledge and his ignorance, then you understand, gentlemen, that also with regard to the public at large we must confine our claims to demanding that merely what every single investigator in his own direction, in his sphere, can designate as reliable truth which is common to all—that only this shall be admitted into the general plan of education.

In thus marking the confines of our knowledge we must remember before all things that what is generally termed natural science is, like all other knowledge in this world, composed of three totally different parts. Generally a difference is only made between *objective* and *subjective* knowledge, but there is a certain intermediate part—I mean *belief*—which also exists in science, with this difference only, that here it is applied to other things than in the case of religious belief. It is somewhat unfortunate, in my opinion, that the expression belief has been so completely monopolised by the church, that one can hardly apply it to any secular object without being misunderstood. In reality there is a certain domain of belief even in science, upon which the single worker no longer undertakes to prove what is transmitted to him as true, but where he instructs himself merely by means of tradition, just what we have in the church. I would like to remark on the contrary—and my conception has not been contradicted by the church—that it is not belief alone which is taught in the church, but that even ecclesiastical dogmas have their objective and their subjective sides. No church can avoid developing in the three directions I have pointed out: in the middle the path of belief, which is certainly very broad, but on the one side of which there is a certain quantity of objective historical truth, and on the other a variable series of subjective and often very fantastic ideas. In this the ecclesiastical and the scientific doctrines are alike. The cause of this is that the human mind is a simple one, and that it carries the method which it follows in one domain finally into all the others as well. But we must be aware at all times how far each of the directions mentioned extends in the different domains. Thus, for instance, in the ecclesiastical domain—it is easier to show it in this one—we have the real dogma, the so-called positive belief; about this I need not speak. But each creed has its peculiar historical side. It says: this has happened, this has occurred, these events have taken place. This historical truth is not simply handed down, but in the garb of an objective truth it appears with certain proofs. This is the case with the Christian religion just as much as with the Mohammedan, with Judaism just as much as with Buddhism. On the other side we find the left wing as it were, where subjectivity reigns; there the single individual dreams, there visions come and hallucinations. One religion promotes them by special drugs, another by abstinence, &c. Thus subjective individual currents are developed, which occasionally assume the shape of perfectly independent phenomena existing by the side of and apart from the previous ecclesiastical domain, which at other times are rejected as heresies, but which often enough lead into the large current of the recognised church. All this we find again in natural science. There too we have the current of the dogma, there too we have the currents of the objective and subjective doctrines. Consequently our task is a compound one. First of all we always try to reduce the dogmatic current. The principal aim of science has for centuries been to strengthen more and more the right, the conservative side. This side, which collects the *ascertained facts* with the *full consciousness of proof*, this side, which adheres to *experiment as the highest means of proof*, this side, which is in possession of the real scientific treasury, has always grown larger and broader, and this principally at the expense of the dogmatic stream. Really, if we only consider the number of natural sciences which since the end of last century have grown and now flourish, we must admit that an almost incredible revolution has taken place.

There is no science in which this is so eminently evident as in medicine, because it is the only science, which has a continuous history of nearly 3,000 years. We are, so to speak, the patriarchs of science, inasmuch as we have the dogmatic current at its longest. This current was so strong, that in the early part of the middle ages even the catholic church embraced it, and the heathen Galen appeared like a father of the church in the ideas of men; indeed, if we read the poems of that period, he often presents himself exactly in the position of a church dignitary. The medical dogma went on until the time of the Reformation. As contemporaries of Luther, Vesal and Paracelsus came and made the first grand attempts at reduction, they drove piles into the dogmatic stream, constructed dykes by its sides, and left only a narrow fair-way to it. Beginning from the sixteenth century it has grown narrower and narrower every century, so that finally only a very small channel has remained for the therapeutists. Thus vanishes the lordliness of the world.

Only thirty years ago the Hippocratic method was spoken of as something so sublime and important that nothing more sacred could be imagined. Nowadays we must own that this method is annihilated nearly down to its root. At least, a good deal of imagination is necessary if we say that any physician of the present day acts as Hippocrates did. Indeed, if we compare the medicine of to-day with the medicine of the year 1800—accidentally the year 1800 marks a great turning-point in medicine—then we find that our science has undergone a complete reformation during the last seventy years. At that time the great Paris school was formed, immediately under the influence of the French Revolution, and we must admire the genius of our neighbours that enabled them to find all at once the fundamental basis of an entire new discipline. If now we see medicine continue its development in the greater breadth of objective knowledge, we will never forget that the French were the precursors, as in the middle ages the Germans were.

By our own example I only wished to show you shortly what changes both the methods and the storehouse of knowledge undergo. I am convinced that in medicine, at the end of the present century, only a sort of clay-pipe system will have remained, through which the last weak waters of the dogmatic stream may move—a sort of drainage. For the rest the objective current will probably have entirely consumed the dogmatic one.

Perhaps the subjective one will remain as well. Perhaps even then many an individual will dream his beautiful dreams. The field of objective facts in medicine, great as it has become, has yet left such a number of lateral fields, that for anybody who *wants* to speculate, plenty of opportunities offer daily. And these opportunities are honestly made use of. A multitude of books would remain unwritten if only objective things were to be communicated. But the subjective wants are still so great, that I believe I am justified in maintaining that of our present medical literature about one half might safely remain unpublished, without doing any damage worth mentioning to the objective side.

Now when we *teach*, in my opinion, we ought not to look upon this subjective side as an essential object in the doctrine. I believe I now belong to the oldest professors of medicine; I have taught my science now for over thirty years, and I may say that during these thirty years I have honestly striven by myself to free my mind more and more from all subjective tendency, and to get more and more into the objective current. Nevertheless I openly confess that I find it impossible to give up subjectivity altogether. Every year I see again and again that even in points, where I had believed myself to be entirely objective, I still retained a large number of subjective ideas. I do not go so far as to make the inhuman demand that everybody is to express himself entirely without any subjective vein, but I do say that we must set ourselves the task to transmit to the students the real knowledge of facts in the first place, and if we go further, we must tell them each time: "but this is not proved, but this is *my* opinion, *my* idea, *my* theory, *my* speculation."

This, however, we can only do with those who are already educated and developed. We cannot carry the same method into the elementary schools, we cannot say to each peasant boy, "This is a fact, this we know, and that we only suppose." On the contrary, that which is known, and that which is only supposed, as a rule get so thoroughly mixed up that that which is supposed becomes the main thing, and that which is really known appears only of secondary importance. Therefore we who support science, we who live in science, are all the more called upon to abstain from carrying into the heads of men, and most of all into the heads of teachers, that which we only suppose. Certainly, we cannot give facts only as raw material, that is impossible. They must be arranged in a certain systematic

[1] Address delivered at the Munich meeting of the German Association, by Prof. Rudolf Virchow, of Berlin. Continued from p. 94.

order. But we must not extend this arrangement beyond what is absolutely necessary.

This is a reproach which I cannot help making against Prof. Nägeli as well. Prof. Nägeli has discussed, certainly in the most measured way and—you will notice this if you read his address—in a thoroughly philosophical manner, the difficult questions which he has chosen as subjects for his address. Nevertheless he has taken a step which I consider extremely dangerous. He has indeed done in another direction what is in one way done by *generatio æquivoca*. He asks that the mental domain shall be extended not only from animals to plants, but that finally we shall actually pass from the organic world into the inorganic with our conceptions of the nature of mental phenomena. This method of thinking, which is represented by great philosophers, is natural in itself. If anyone wants by any means to connect mental phenomena with those of the rest of the universe, then he will necessarily come to transfer the mental processes, as they occur in man and the animals of highest organisation, to the lower and lowest animals; afterwards a soul is even ascribed to plants; further on the cell thinks and feels, and finally he finds a passage down to chemical atoms, which hate or love one another, seek one another, or flee from one another. All this is very fine and excellent, and may after all be quite true. It *may* be. But then, do we really want, is there some positive scientific necessity, to extend the domain of mental phenomena beyond the circle of those bodies, in which and by which we see them really happening? I have no objection if carbon atoms have a mind as well, or that they obtain a mind in their union with the plastidule association, but *I do not know in what I am to recognise this*. It is simply playing with words. If I declare attraction and repulsion to be mental occurrences, to be mental phenomena, then I simply throw the mind (*die Psyche*) out of the window; then the mind ceases to be mind. The phenomena of the human mind may eventually be explained in a chemical way, but for the present, I think, it is not our task to mix up these domains. On the contrary, it is our duty to keep them strictly where we understand them to be. And as I have always laid stress upon this, that we should not in the first line try to find the *transition* from the inorganic into the organic, but that we should first of all determine the *contrast* between the inorganic and the organic, and carry on our investigations among those contrasts in the same way, I now maintain that the only way to progress—and I hold the firmest conviction that we shall not advance at all otherwise—is to limit the domain of mental phenomena where we really perceive mental phenomena, and not to *suppose* mental phenomena, where perhaps they *may* be, but where we do not notice any visible, audible, sensible, in one word, *perceptible* phenomena, which we might call mental ones. There is no doubt that for us the whole sum of mental phenomena is attached to certain animals, not to the totality of all organic beings, not even to all animals generally, and I maintain this without hesitation. We have no reason yet to say that the lowest animals possess mental characteristics; we find them only with the higher animals, and with perfect certainty only with the highest.

Now I will admit with pleasure that certain gradations, certain gradual transitions, certain points can be found, where from mental phenomena one gets to phenomena of simply material or physical nature. I certainly do not declare that it will never be possible to bring psychical phenomena into immediate connection with physical ones. All I say is, that *at present* we are not justified in setting down this *possible* connection as a scientific *doctrine*, and I must distinctly oppose the attempts to enlarge our doctrines prematurely in this manner, and to bring again and again into the foreground as a positive statement what we so often proved a useless problem. We must distinguish strictly between what we want to teach and what we want to investigate. What we investigate are problems. We need not keep them to ourselves; we may communicate them to the whole world and say, There is the problem, this is what we are trying to find; like Columbus, who, when he started to discover India, made no absolute secret of it, but who eventually did not find India, but America. And the same happens to us not rarely. We start to prove certain problems which we suppose to be perfectly correct, and in the end we find something quite different, which we never expected. The investigation of such problems, in which the whole nation may be interested, must be open to everybody. That is the *liberty of research*. But the problem is not at once to be the object of instruction. When we teach we must confine ourselves to those smaller domains which are already so large, and which we have actually mastered.

Gentlemen, I am convinced that only with a resignation of this kind, which we impose on ourselves, which we exercise towards the rest of the world, shall we be enabled to conduct the fight against our enemies with a victorious result. All attempts to transform our problems into doctrines, to introduce our theories as the basis of a plan of education, particularly the attempt simply to depose the church, and to replace its dogma by a religion of descent without further trouble, these attempts, I say, must fail, and their failure would at the same time bring the greatest dangers upon the position of science generally.

Therefore let us be moderate, let us exercise resignation, so that we give even the most treasured problems which we put forth, always as problems only, and that we say it a hundred and again a hundred times: "Do not take this for confirmed truth, be prepared that this may perhaps be changed; only for the moment we are of opinion *that it may be true.*"

By way of illustration I will add another example. At this moment there are probably few naturalists who are not of opinion that man is allied to the rest of the animal world, and that a connection will possibly be found, if indeed not with apes, then perhaps in some other direction, as is now the opinion of Prof. Vogt.

I acknowledge openly that this is a desideratum of science. I am quite prepared for it, and I would not for a moment wonder nor be alarmed if the proof were found that the ancestors of man were vertebrate animals. You know that just at present I work by preference in the field of anthropology, but yet I must declare that every step of positive progress which we have made in the domain of prehistoric anthropology, has really moved us further away from the proof of this connection. At this moment anthropology studies the question of fossil man. From man in the present "period of creation" we have descended to the quaternary period, to that period when, as Cuvier maintained with the greatest confidence, man never existed at all. Nowadays quaternary man is a generally accepted fact. Quaternary man is no longer a problem, but a real doctrine. But tertiary man is a problem—of course a problem which is already in a stage of material discussion. There are objects already about which discussions are going on as to whether they may be admitted as proofs for the existence of man during the tertiary period. We do not merely speculate on the subject, but we discuss certain objects, whether they may be recognised as witnesses for the activity of man during the tertiary period. The question raised is answered differently according to whether these objective material elements of proof are considered sufficient or not. Even men who, like Abbé Bourgeois, are decided ecclesiastics, are convinced that man has lived during the tertiary period; for them tertiary man is already a doctrine. For us, who are of a more critical nature, tertiary man is still a problem, but, as we must acknowledge, a problem worthy of discussion. Let us therefore for the present remain at quaternary man, whom we really find. If we study this quaternary, fossil man, who ought after all to stand nearer to our ancestors in the series of descent, or rather of ascent, we find a man just the same as we are ourselves.

Only ten years ago, when a skull was found, perhaps in peat or in lake dwellings, or in some old cave, it was believed that wonderful marks of a wild and quite undeveloped state were seen in it. Indeed we were then scenting monkey air. But this has died out more and more. The old troglodytes, lake inhabitants, and peat people turn out to be quite a respectable society. They have heads of such a size that many a person living would feel happy to possess one like them. Our French neighbours have certainly warned us not to conclude too much from these big heads; it may be possible that they were not filled only with nerve-substance, but that the old brains had more intermediary tissues than is the case now-a-days, and that their nerve-substance in spite of the size of the brain, remained at a low state of development. However this is only a friendly conversation which to some extent is held as a support of weak minds. On the whole we must really acknowledge that all fossil type of a lower human development is absolutely wanting. Indeed if we take the total of all fossil men that have been found hitherto and compare them with what the present offers, then we can maintain with certainty that amongst the present generation there is a much larger number of relatively low-type individuals than amongst the fossils hitherto known. That only the highest geniuses of the quaternary period enjoyed the good fortune of being preserved for us I do not dare to suppose. As a rule we draw conclusions from the condition of a single fossil object with respect to the majority of others which have not been found. But I will not do this. I will not maintain that the whole race was as

good as the few skulls which were found. But I must say that one fossil monkey-skull or man-ape skull which really belonged to a human proprietor has never been found. Every addition which we have obtained in the material inventory of objects for discussion has moved us further away from the problem to be solved. Now of course we cannot avoid the consideration that perhaps it was on some quite special spot of the earth that tertiary man lived. This is quite possible, since during the last few years the remarkable discovery has been made in North America that the fossil ancestors of our horses occur in countries from which the horse had entirely disappeared for a long time. When America was discovered there were no horses there at all; in the very place where the ancestors of our horses had lived no living horse had remained. Thus it may also be that tertiary man has existed in Greenland or Lemuria, and will again be brought to light from under the ground somewhere or other. But as a fact we must positively acknowledge that there is always a sharp limit between man and the ape. *We cannot teach, we cannot designate it as a revelation of science, that man descends from the ape or from any other animal.* We can but designate this as a problem, may it seem ever so probable and may it lie ever so near.

We ought to be sufficiently warned by the experiences of the past, at a time when we are not justified in drawing conclusions, not unnecessarily to burden ourselves with the obligation, or yield to the temptation of drawing them all the same. Look you, gentlemen, it is in this that the difficulty lies for every naturalist who speaks to the world at large. Whoever speaks or writes for the public, ought, in my opinion, doubly to examine just now, how much of that which he knows and says is objective truth. He ought to try as much as possible to have all inductive extensions which he makes, all progressing conclusions by the laws of analogy, however probable they may seem, printed in small type underneath the general text, and to put into the latter only that which really is objective truth. In that case we might perhaps succeed in gaining an always increasing circle of followers, in obtaining an always increasing number of fellow-workers, and in causing the educated public to continue to take part in that fertile manner in which it has already taken part in many domains. Otherwise, gentlemen, I fear that we overrate our power. Certainly old Bacon said with perfect justice, *scientia est potentia*, knowledge is power. But he has also defined knowledge, and the knowledge which he meant was not speculative knowledge, not the knowledge of problems, but it was the objective knowledge of facts. I think that we should abuse our power, we should endanger our power, if in our teaching we do not fall back upon this perfectly justified, perfectly safe, and impregnable domain. From this domain we may as investigators make our excursions in the direction of problems, and I am convinced that every attempt of this kind will then find the necessary safety and support.

AMERICAN SCIENCE

THE principal paper in the *American Journal of Science and Arts* for November, is Prof. Marsh's able address at the recent meeting of the American Association, on the Introduction and Succession of Vertebrate Life in America, which we have given at length.—Discussing the question, Is the existence of growth rings in the early exogenous plants proof of alternating seasons? Dr. Warring concludes from observations, that some exogens form rings at intervals much less than a year; others require intervals of several years, and some form no rings. The presence or absence of rings in exogens occurs in all climates. Large and well-defined rings are found where there is absolutely no appreciable variation of temperature or moisture throughout the year. An exogen naturally forming rings will continue to form them, although the climate become uniform throughout the year. Thus the existence of these markings in ancient flora gives no information as to the existence at that time of seasons, and so far as they are concerned we are left free to adopt any conclusion as to inclination of the earth's axis, which may appear most reasonable.—Some years ago Prof. Newcomb showed that the improvements introduced into the theory of the moon's mean motion by Hanssen's lunar tables did not extend to the inequalities of long period in that motion. While Hanssen, by an empirical term had secured a very good agreement with observations from 1750 to 1860, this agreement was found to have been obtained by sacrificing the agreement before 1750, and the moon had then begun to deviate from the tables at such a rate that they could not continue satisfactorily to represent the observations. Prof. Newcomb has since attempted a complete discussion of all recorded observations of any astronomical value before the year 1750, and his suspicion has been entirely confirmed. The results of this examination are communicated. Comparing a theory of the moon's mean motion founded on gravity alone, with the observations, he is led to suppose that the deviations may be due to the action of some of the bodies of the solar system. He corrects Hanssen's term by an empirical addition.—Prof. Dana contributes to the number a note on the Helderberg formation of Bernardston, Massachusetts, and Vernon, Vermont, and Mr. Mallet describes "Serpylite," a new niobate, from Amherst County, Virginia.

The *New York Tribune* states that the Johns Hopkins Scientific Association has recently been organised in Baltimore. Prof. Sylvester is president, Prof. Remsen, vice-president, Dr. Story, secretary. A great feature in the programme is that the essays presented are to be short and concise, and to contain the particulars of original research exclusively. There is also to be a discussion of new scientific publications, both foreign and domestic, at the meetings, of which the first has been held, with a score of members present.

Under date November 20, the *Tribune* has the following telegram from Washington:—Messrs. S. H. Scudder of Cambridge, and F. C. Bowditch, of Boston, have just returned from a two months' tour in Colorado, Wyoming, and Utah, where, under the direction of Dr. Hayden, they have been exploring for fossil insects and collecting specimens especially in the high regions. They report having secured many specimens of fossil insects at different points along the railways from Pueblo to Cheyenne, and from Cheyenne to Salt Lake, as well as at Lakin, Kansas, and Garland, and Georgetown, Col., and in various parts of the South Park and surrounding region. Their time was so limited that they were unable to visit White River and explore the beds of fossil insects] known to exist there. Ten days were spent at Green River, and in that vicinity, in exploring the tertiary strata for fossil insects, but with very unsatisfactory results. Near Florisante the tertiary basin was found to be exceedingly rich in insects and plants. Mr. Scudder spent several days in the careful survey of this basin, and estimates that the extent of the insect-bearing shales there is at least fifty times as great as that of those in Southern Bavaria. Six or seven thousand specimens of insects, and 2,000 or 3,000 of plants have already been received from Florisante, and as many more are expected before the close of the year. Arrangements were also made with persons who have found a new and rich deposit of fossils in the tertiary strata in Wyoming to forward all the specimens obtained there. Mr. Scudder believes that the tertiary strata of the Rocky Mountain region are richer in the remains of fossil insects than any others in the world, and that within the next few months the amount of material at hand for the study of the subject will be greater than was ever before possessed by any single naturalist. Prof. Joseph Leidy, the comparative anatomist and microscopist, has also recently returned from his second visit to the west, under the direction of Dr. Hayden. His field of operations during the past season was the country about Fort Bridger, Unitah Mountains and the Salt Lake Basin. The specimens he has collected comprise the lowest and simplest forms of animal life, the most minute requiring high microscopic power to distinguish their structure.

THE METEOR

WE have received some further communications concerning this remarkable phenomenon, and some interesting details concerning a similar body will be found in our "Astronomical Column." Mr. A. O. Walker writes from Chester:—

In reading the notice of the meteor of November 23 in NATURE, vol. xvii. p. 94, I am surprised to see no mention of any report from it. As I only heard it without seeing it I send you the notice of it from my diary, written immediately after the occurrence:—

"About 8.30 P.M. heard a loud report like that of a cannon (say 32 lbs.), fired about 200 yards off, which shook the house, and the servants saw a bright flash. The sky overhead was quite clear and only cloudy on the horizon south and east. Thought it was the explosion of an aerolite."

Next day I made inquiries and added the following:—

"Parry and Field said the flash was blue, and five minutes

elapsed between the flash and bang. Parry's girl was outside, and came in crying; said she had seen 'a very funny kind of lightning.' Parry remarked it shook his door."

The two men named above are in my employ, and live about 300 yards from my house. Some friends of ours living about two miles from us also saw the flash and heard the report, but the latter not so loud as we did. They described it as sounding as if a bird had flown against the window."

I give the above extracts *verbatim*, as first impressions, uninfluenced by what one hears or reads subsequently, are much the most valuable.

Dr. S. Drew, of Chapeltown, Sheffield, writes as follows:—

I send you the following calculations as to the meteor of November 23. They may interest some of your readers. The estimates are only intended as approximate, as the observations at different points of view were too vague for much accuracy, and indeed, in two instances, obviously quite unreliable.

The visible course of the meteor appears to have been from a point about 150 miles above the town of Worksop to the Irish Channel, north-west of Liverpool, probably nearly half-way between Liverpool and the Isle of Man—a direction from east by south to west by north, the horizontal distance traversed being rather over 100 miles and the perpendicular 150 miles. The size of the fire-ball before breaking up was about 150 yards in diameter. By this is meant the size of the luminous sphere, not that of the actual bolide, which would be much less.

The rate of motion was near twenty miles per second in horizontal, and thirty miles in perpendicular; as this in horizontal is little more than would be caused in appearance by the orbital and diurnal motion of the earth, it is evident that the proper motion of the meteor was nearly perpendicular to the earth's surface; and, if belonging to the solar system, it must have moved in a very eccentric orbit, stretching far beyond that of the earth. The meteor broke at an elevation of about fifty miles, and then appeared much larger. The fragments must have dropped into the sea.

Was it seen from Ireland or the Isle of Man?

S. A. K. writing to the *Manchester Courier* from Blackpool states that about 8.30 P.M. on the 23rd he beheld a ball of a pale blue colour shoot across the sky from east to west, followed by a train of rainbow lines, brilliant beyond description. "It was over in a moment; but as I and several others tood discussing the phenomenon we had just witnessed, two muffled booms as of far-distant cannon were distinctly heard in the west, after an interval of two or three minutes." Capt. Tupman writes from the Royal Observatory, Greenwich, to the *Times*: "There is reason to suppose that the great meteor which appeared at 8.20 P.M. on Friday last (November 23) fell into the sea near the mouth of the river Dee. From its splendour it was probably seen by many persons near the shores of North Wales, Cheshire, and Lancashire, whose observations would be of the greatest value; and I venture to solicit the publicity of your columns in order that such observations may be forwarded here. On Tuesday night (Nov. 27), at 10.26, G.M.T., I observed another pass slowly from a point about 6° over Castor to 5° left of Sirius. It remained in sight fifteen or sixteen seconds, determined by counting. Towards the end it became faint, of a dull red colour, and moved with extreme slowness. I have no doubt it must have appeared very large to observers near Dover and in Normandy, and it is to be hoped its path has been recorded elsewhere."

A meteor was observed at Strassburg on November 23, the very day when the meteor was observed in England, but the time was a little after six o'clock (local time), and the direction from north to south. A violent detonation was heard, but without any resemblance to that of thunder. The light was as vivid as ordinary lightning at Strassburg. A witness states that he saw the meteor falling at a small distance from him (three or four metres) in a wood belonging to the Chevaudier de Valdrome on the new road leading from Lorquin to the French frontier. All the trees were illuminated as if by daylight. It is not reported by the *Strassburg Gazette* whether any stone was found on the spot.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—The Brackenbury Scholarship in physical science has been awarded to Mr. Cunningham, Balliol College.

LONDON.—The Council of University College have awarded the Sharpey Physiological Scholarship to Mr. Patrick Geddes and the Joseph Hume Scholarship in Political Economy of 20*l.* per annum for three years to Mr. J. G. Schurman.

EDINBURGH.—A public meeting, under the presidency of the Right Hon. the Lord Provost, was held on the 29th ult. at Edinburgh to advocate the claims of the Edinburgh University Buildings Extension Scheme. The cost of the new medical school, &c., will be about 187,000*l.*, and of that sum 82,000*l.* has been subscribed by the public and 80,000*l.* has been promised by Government on condition that the remaining 25,500*l.* be subscribed before the end of next year. It was announced that about 10,000*l.* of this has been promised, leaving upwards of 14,000*l.* still to be raised. In support of the appeal it was mentioned that in some class-rooms there is not sitting room for the students. The number of students is increasing every year, there being at present enrolled 212 more than at the same time last year, so that before the summer session is over there will probably be close on 2,500 students matriculated.

The first meeting of the fourth session of the Chemical Society of the University was held in the University on November 28, the president, Prof. A. Crum Brown, in the chair. The president gave an introductory lecture on the "Life and Works of Dr. Joseph Black." The following office-bearers were elected for the ensuing season:—President—Prof. A. Crum Brown; Vice-Presidents—J. Gibson, Ph.D., F.R.S.E., W. Inglis Clark, B.Sc.; Secretary—J. Adams; Treasurer—C. Maxwell, R.N. The society numbers fifty-two members, and ten new members were proposed.

MANCHESTER.—A Chemical Society has been commenced at the Owens College. The society is intended to include all students of science at the College—Dalton Scholars, Associates, and a few others connected now, or in the past, with the Science Classes of Owens College. The society was opened on Wednesday evening by an address from Prof. Thorpe, F.R.S. on "Robert Boyle and the Sceptical Chemist." The Syllabus of the society for the session is as follows:—"Are the Elements Elementary?" by Mr. Pattison Muir; "Graham," by Mr. P. P. Bedson, B.Sc.; "Berzelius," by Mr. J. K. Crow, B.Sc.; "Alkali Manufacture," by Mr. Bevan: "Crystallisation," by Mr. Baker; "Liebig," by Mr. C. F. Cross; "Valensy," by Mr. O'Shea; "Chemical Industry of Japan," by Mr. Siguira; and a paper, subject not settled, by Prof. Gamgee. It is hoped and believed that the society will tend to increase the interest in scientific pursuits already manifested by members of the College.

FRANCE.—A number of important measures have been taken by the French Minister of Public Instruction for fostering the zeal of students and professors in the several French faculties. By a decree issued on November 5 a number of scholarships have been created in each academy at the expense of the public exchequer. In future years scholars are to be appointed after having passed special examinations similar to those for exhibitions in the English universities. Exceptions are created in favour of students who have been particularly successful in taking their preliminary degrees and have published approved papers in the Academical *Transactions*, or have rendered special services in tuition. For the present year the different scholarships are to be granted by a special commission. Three of these commissions have been established—one for letters, another for science, and the third for medicine. These scholarships are to be continued only for a limited time, varying from two to four years, but are to be stopped at once if the scholar does not give satisfaction to the professors or lecturers. A part of these scholarships is to be granted to candidates for the mastership of arts (Licencié-ès-Lettres and ès-Sciences), and another part to the masters in several faculties wishing to take the highest honours in their respective faculties. By another decree, published on the same day, M. Brunet has created a number of lectureships styled "conferences." A number of the lecturers are to act as public tutors, helping public professors in their duties. Other lectureships are to be granted to professors teaching supplementary sciences which, up to the present time, have not come within the limits of the official programme. The salary of all of them is 120*l.*, and they are to be appointed yearly from among doctors or members of the academies. In some peculiar cases Masters of Arts are eligible to these lectureships. The new organisation is expected to work during the present classical year.

SOCIETIES AND ACADEMIES

LONDON

Geological Society, November 7.—Prof. P. Martin Duncan, F.R.S., president, in the chair.—Stephenson Clarke, William Hunter, and the Rev. W. Roberts, were elected Fellows of the Society. The following communications were read: A letter dated September 14 was read, from Lord Derby, stating that his lordship had received a despatch from her Majesty's Minister at Tehran, reporting that a mining engineer had arrived there from Berlin, who, at the request of the Persian government, had been selected by Messrs. Siemens to ascertain what foundation there was for the reported existence of a rich vein of gold in the vicinity of Zengan; that he had visited the locality and reported that auriferous quartz does exist, but that he had not yet succeeded in finding any vein or deposit of the metal.—Notes on fossil plants discovered in Grinnell Land by Capt. H. W. Feilden, Naturalist, to the English North Polar Expedition, by Prof. Oswald Heer, F.M.G.S. Near Discovery Harbour, where H.M.S. *Discovery* wintered in 1875-6, in about 81° 45′ N. lat., and 64° 45′ W. long., a bed of lignite, from twenty-five to thirty feet thick, was found, resting unconformably upon the azoic schists of which Grinnell Land chiefly consists. The lignite was overlain by black shales and sandstones, the former containing many remains of plants; and above these there were, here and there, beds of fine mud and glacial drift, containing shells of marine mollusca of species now living in the adjacent sea. This glacial marine deposit occurs up to levels of 1,000 feet, indicating a depression and subsequent elevation of the region to at least this extent. Remains of twenty-five species of plants were collected by Capt. Feilden, and eighteen of these are known from miocene deposits of the Arctic zone. The deposit is therefore no doubt miocene. It has seventeen species in common with Spitzbergen (78° 79′ N. lat.), and eight species in common with Greenland (70° 71′ N. lat.). With the miocene flora of Europe it has six species in common; with that of America (Alaska and Canada) four; with that of Asia (Sachalin) four also. The species found include two species of *Equisetum*, ten Coniferæ, *Phragmites œningensis*, *Carex noursoakensis*, and eight dicotyledons, namely, *Populus arctica*, *Betula prisca*, and *Brongniarti*, *Corylus macquarrii* and *insignis*, *Ulmus borealis*, *Viburnum nordenskiöldi*, and *Nymphæa arctica*. Of the Conifers, *Torellia rigida*, previously known only by a few fragments from Spitzbergen, is very abundant, and its remains show it to have been allied to the Jurassic genera *Phœnicopsis* and *Bæiera*, the former in its turn related to the carboniferous *Cordaites*, and among recent conifers, to *Podocarpus*. Other conifers are, *Thuites ehrenswärdi* (?), *Taxodium distichum miocenum* (with male flowers), *Pinus feildeniana* (a new species allied to *P. strolbus*), *Pinus polaris*, *P. abies* (twigs covered with leaves), a species of *Tsuga Pinus dicksoniana*, Heer.), and a white spruce of the group of *Pinus grandis* and *cariocarpa*. *Pinus abies*, which occurs here and in Spitzbergen, did not exist in Europe in miocene times, but had its original home in the extreme north, and thence extended southwards; it is met with in the Norfolk forest-bed, and in the interglacial lignites of Switzerland. Its present northern limit is 69½° N., and it spreads over 25° of latitude. *Taxodium distichum*, on the contrary, spread in miocene times from Central Italy to 82° N. latitude, whilst at present it is confined to a small area. *Betula brongniarti*, Ett., is the only European species from Grinnell Land not previously known from the arctic zone. The thick lignite bed of Grinnell Land indicates a large peat-moss, probably containing a lake in which the water-lilies grew; on its muddy shores stood the large reeds and sedges, the birches, poplars, *Taxodia*, and *Torelliæ*. The drier spots and neighbouring chains of hills were probably occupied by the pines and firs, associated with elms and hazel bushes. A single elytron of a beetle (*Carabites feildenianus*) is at present the sole evidence of the existence of animals in this forest region. The nature of the flora revealed by Capt. Feilden's discoveries seems to confirm and extend earlier results. It approaches much more closely to that of Spitzbergen than to that of Greenland, as might be expected from the relative positions of the localities; and the difference is the same in kind as that already indicated by Prof. Heer between Spitzbergen and Greenland, and would indicate the same kind of climatic difference. Nevertheless, the presence of *Taxodium distichum* excludes arctic conditions, and that of the water-lily indicates the existence of fresh-water, which must have remained open a great part of the year. Representatives of plants now living exclusively in the arctic zone are wanting in the Grinnell Land deposits; but, on the other hand, most of the genera still extend into that zone, although they range in Grinnell Land from 12° to 15° further north than at present.—On our present knowledge of the invertebrate fauna of the lower carboniferous or calciferous sandstone series of the Edinburgh neighbourhood, especially of that division known as the Wardie Shales, and on the first appearance of certain species in the beds, by Mr. R. Etheridge, jun., F.G.S.

Zoological Society, November 20.—Prof. Flower, F.R.S., vice-president, in the chair.—Mr. Howard Saunders exhibited a specimen of the rare Aleutian Tern (*Sterna aleutica*) from Alaska, and made remarks upon its intermediate position between typical *Sterna* and the group of the Sooty Terns (*Onychoprion*).—A communication was read from the Marquis of Tweeddale, F.R.S., containing an account of a collection of birds made by Mr. A. H. Everett in the Island of Zebu, Philippines. Six new species were found in this collection, and were named *Oriolus assimilis*, *Phyllornis flavipennis*, *Zosterops everetti*, *Prionochilus quadricolor*, *Turnix nigrescens*, and *Megapodius pusillus*.—Three communications were read from Dr. O. Finsch, C.M.Z.S. The first contained a report on a collection of birds made at Eua, Friendly Islands, by Mr. F. Hübner, which had increased our knowledge of the avifauna of Eua from four to twenty-four species. The second contained a description of a collection of birds made on the Island of Ponapé, Eastern Carolinas, by Mr. J. Kubary. The total number of species known at present from Ponapé was stated to be twenty-nine, of which seven were peculiar to the island. The third contained a list of the birds obtained at Ninafou Island in the Pacific, by Mr. F. Hübner. This collection raised the number of the known birds of this island from one to twenty.—Prof. Garrod, F.R.S., read notes on the *Taenia* of the rhinoceros of the Sunderbunds *Plagiotaenia gigantea*), on the anatomy of the Chinese water-deer (*Hydropates inermis*), on the possible cause of death in a young seal, and on the occurrence of a gall-bladder in certain species of parrots.—Mr. Howard Saunders, F.Z.S., read a paper on the *Laridæ* collected during the voyage of H.M.S. *Challenger*, which comprised nine species of *Sternæ*, five of *Larinæ*, and three of *Stercorarinæ*, altogether seventeen species represented by forty-given specimens; several of these were very rare in museums, although none of them were absolutely new to science.—A communication was read from Dr. A. B. Meyer, containing some additional proofs of the fact that the Red *Eclecti* are the females of the green species of that genus.—A paper was read by Mr. G. French Angas, C.M.Z.S., containing notes on *Helix sepulcharalis* of Ferrusac, and its allies, with descriptions of two new species.

Physical Society, November 17.—Dr. Stone, vice-president, in the chair.—The president, Prof. G. C. Foster, described and exhibited a very simple form of absolute electrometer, which acts on the same principle as Sir W. Thomson's trapdoor form of apparatus, but can be constructed at a very moderate cost. To one arm of a balance is suspended by silk fibres a zinc disc, which hangs horizontally in the plane of a sheet of the same metal forming a guard-plate; and at a distance of about one inch below is a flat sheet of zinc, also horizontal. An electrical connection is formed between the guard-plate and suspended disc by a bridge of very fine wire. The method of using the apparatus to determine the potential required for a spark to pass from a Holtz machine through varying thicknesses of air was explained. When the balance has been accurately counterpoised, an excess weight, say one gramme, is introduced into the scale pan, and the guard-plate and the lower attracting-plate, as well as the two knobs of a spark-measurer, are connected with the conductors of the machine. If this be now set in action, and the knobs of the spark-measurer be gradually separated, a point will be reached at which the attraction upon the suspended disc just overcomes the excess weight in the balance pan. The length of spark for which this occurs can now be read off. The difference of potential causing the spark is given by the formula $\frac{e}{a}\sqrt{8F}$, where a is the radius of the attracted disc, e its distance from the attracting-plate, and F the force of attraction in dynes. In the apparatus exhibited, a had the value 5·195 cm., and e the value 2·4 cm., whence, if w be the excess weight in grammes—so that $F = 981\ w$—the difference of potential becomes $39\sqrt{w}$. The proper action of the apparatus depends essentially upon the attracted disc being accurately in the same plane with the guard-plate. To facilitate this adjustment, each of the silk fibres by which the disc is suspended is attached to a

screw, by which it can be separately raised or lowered; and by means of another screw the small brass plate holding the suspending screws can be raised or lowered as a whole. A few numerical results were given to illustrate the action of the apparatus. These were taken from a set of experiments in which the difference of potential needed to produce sparks in air between two equal brass spheres of 2·61 cm. radius was measured. The following are the results for a few of the shortest and longest sparks measured:—

Length of Spark.	Difference of Potential.	Mean Electrical Force.
cm.		
0·1325	17·4	131
0·1825	20·4	117
0·237	24·6	104
0·68	62·9	93
0·71	65·2	92
0·74	68·7	93

VIENNA

Imperial Academy of Sciences, October 11.—Preliminary note on the position of the optical axes of elasticity in gypsum for various colours, by M. Lang. The angle of the optic axes shows a maximum for the Fraunhofer line D. The dispersion of the axes of elasticity in the plane of symmetry is abnormal. These observations agree on the one hand with Poggendorff's exact description of the axial forms of gypsum, and on the other side with Descloigeaux's observation that at the higher temperatures, where the plane of axis is at right-angles to the place of symmetry, no horizontal dispersion is observable.—Annual periods of the insect fauna of Austro-Hungary, by M. Fritsch.—On the relation between the second principal proposition of the mechanical theory of heat and the calculation of probability respecting the propositions on heat-equilibrium, by M. Boltzmann.—The cylindroid and its specialties, by M. Kozak.—Simple calculation of elliptic arches, by G. Seewald.—On eruptive sands, and on the Flysch and the *Argille scagliose*, by M. Fuchs.—On equal figures in curves, cones, and surfaces of the second order and of certain of higher orders, by M. Puchta.—Calculation of cylindrical vessels with complicated relations, by M. Streicher.—On development of the resinous passages in some coniferæ, by M. Weiss.—Continued studies on the mode of ending of nerves of smell, by M. Exnor.

PARIS

Academy of Sciences, November 26.—M. Peligot in the chair:—The following papers were read:—Geographical positions of the principal points of the coast of Tunis and Tripoli, by M. Mouchez. This relates to observations during the hydrographic voyage of the *Castor* in 1876, of some fifty points equally distributed along about 300 leagues of coast.—On some applications of elliptic functions (continued), by M. Hermite.—The Echidna of New Guinea, by M. Gervais. He notes several points in which the head differs from that of the Australian animal.—On invariants, by Prof. Sylvester.—On the waves of various kinds which result from the working of the sluice of Aubois, by M. Caligny.—On the solution of the equation of the fifth degree, by M. Brioschi.—Nature of the hydrocarbons produced by action of acids on manganesiferous spiegeleisen, by M. Cloez. Several of these products seem identical with those which exist in the ground and are extracted on a large scale under the name of petroleum. This production of complex carbonised compounds, without any intervention of life, supports the views of certain geologists on the origin of petroleum. The reproduction of a large number of organic species might be realised by commencing with ethylenic or formenic hydrocarbons, furnished by cast iron.—Discovery and observation of the planet 175 by Mr. Watson.—On the distances of stars, by M. Flammarion. He cites several facts which seem not to allow of basing on differences of brightness an estimate of distances.—On the intermediary integral of the third order of the equation with partial derivatives of the fourth order expressing that the problem of geodesic lines supposes an algebraic integral of the fourth degree by M. Levy.—Graphic tables and anamorphic geometry; reclamation of priority, by M. Lalanne.—Second note on the magnetisation of steel tubes, by M. Gaugain. The variations of magnetism produced by heat in a solid bar of steel are not different from those in a system composed of a tube and a core. Both seem to depend on the *inverse* magnetism developed by the mutual reaction of concentric layers, whether of the bar or of the system.—Liquefaction of bioxide of nitrogen, by M. Cailletet. This he effected by compressing to 104 atmospheres at −11°. At +8° the bioxide is still gaseous under 270 atmospheres. He hopes, also, to be able to liquefy formene. M. Berthelot remarked on the importance of this achievement, and thought it probable that most of the gases not yet liquefied, such as oxygen, which already diverges from Mariotte's law under great pressures, and oxide of carbon, would yield to M. Cailletet's new processes.—On nitrification by organic ferments, by MM. Schloesing and Muntz. Whenever, in these experiments, a nitrifiable medium has remained in the presence of chloroform, or has been heated to 100°, then guarded from dust, the nitrification has been suspended, but it has been possible to renew it, by introducing into the heated medium a minimum quantity of a substance like mould in process of nitrification.—On the termination of the nerves in tactile corpuscles, by M. Ranvier. He studied these organs in the tongue and bill of the domestic duck (where they are found in great simplicity). The tactile disc, the true sensitive nervous organ, is protected against mechanical excitations from without by the special cells surrounding it. It can only be impressed in an indirect way.—An experiment in *stasimetry* or measurement of the consistence of organs, by M. Bitot. The instrument is a kind of balance having at the end of one arm a perforating or sounding needle, at the end of the other a small controlling plate, and at the centre a pendulum with successive weights and a long indicator needle connected to it above, moving over a graduated scale.—On a modification of Bell's telephone, with multiple membranes, by M. Trouvé. A cubical chamber is substituted for the single membrane; each face of it is a membrane which, in vibrating, influences a fixed magnet with electric circuit. Associating all the currents generated, an intensity is obtained proportional to the number of magnets affected.—On the telephone, by M. Pollard. This describes some experiments at Cherbourg. M. Du Moncel called attention to the ideas expressed by M. Ch. B—— more than twenty years ago, and which contains the telephone in germ.—On a new sounding apparatus for works of coast hydrography, by M. Pinheiro.

CONTENTS

THURSDAY, DECEMBER 13, 1877

HYDROPHOBIA

POPULAR alarm has of late been aroused by the publication of an unusual number of cases of death from this most terrible disease, and interest and hope have been excited by the statement that, at last, a drug has been found—curare—which does exert such an influence that at least one case is said to have been rescued from otherwise certain death. We propose to discuss briefly in this article the chief points in the natural history of hydrophobia, to examine what light, if any, science has thrown upon its nature, and to inquire what reasons there are for believing in the alleged efficacy of drugs in its treatment.

Hydrophobia is a disease which never occurs spontaneously in man, being invariably communicated to him by the bite of some animal affected with it—commonly by the dog, more rarely the cat, more rarely still the fox and wolf. The bite induces the disease by permitting the absorption of the saliva of the diseased animal, the peculiar poison or "*materies morbi*" of the disease being contained in the saliva. Inasmuch, then, as man only becomes affected with hydrophobia through the intermediation of the lower animals, it will be necessary to consider it, first of all, as it makes itself manifest in them.

It has been, and still is, a subject of dispute amongst veterinarians whether hydrophobia, or "rabies," was originated spontaneously in the dog. Avowedly the immense majority of cases of the disease can be proved to have been due to the bites of rabid animals; some cases do occur, however, in which it is stated that there was no possibility of contact with a diseased animal, and these are held to prove the occasional spontaneous origin of the disease. Now, whilst we are not prepared absolutely to contradict such a surmise, and to allege that at no time, and under no circumstances, hydrophobia originated spontaneously, we do hold that there is no better evidence of such a new origin *now* than there is of the spontaneous generation of the poisons which induce small-pox, scarlet fever, or measles. In the case of these diseases, as in that of hydrophobia, it does sometimes happen that some of the links in the chain of evidence are lost which are required to prove the connection between one case of disease and its precursor, but the exceptional cases do not outweigh the immense mass of evidence which proves that each of the diseases previously mentioned is as certainly the offspring of a previous case as is each animal or plant at present living the offspring of a pre-existing parent organism. We shall then probably be quite right in assuming that not only is it true of hydrophobia as it affects man, but of the disease as it is manifest in all animals, that it is always due to the inoculation of poison from a diseased into a healthy organism.

In commencing a description of hydrophobia we must point out that whilst the disease is always more or less prevalent, periods when it becomes much more frequent occur from time to time. Within the present century, especially between 1800 and 1830, several such outbreaks occurred; in this respect hydrophobia resembles other diseases of the zymotic class, which, though always more or less prevalent, only occasionally prevail with epidemic intensity. We must assume that at this period the circumstances which are required for the spread of the particular disease are specially favourable, though it is only rarely that we can do more than surmise what these special circumstances really are.

In the dog, as indeed in all animals, there is a period of latency, or as it is technically termed, of "incubation," which intervenes between the inoculation of the poison of hydrophobia and the development of any symptoms; this period varies remarkably: it may be as short as a week, or as long as three months; the greater number of cases occurring, however, between the twentieth and fiftieth days after the poisonous wound has been inflicted. It must not be supposed that the bite of a rabid dog always induces the disease in other dogs which it bites; a certain number of such bites prove abortive. Thus, out of 131 dogs which had been bitten by, or inoculated with, the virulent saliva of certainly rabid dogs, only sixty-three fell victims. The failures in these cases are to be explained in several ways. In some cases it is probable that the saliva was not active, just as sometimes the liquid from the vaccine vesicle, when fairly tested, is found to be incapable of reproducing vaccinia; in other cases the poisonous saliva has doubtless been prevented from penetrating the wound, having been retained by the hair and cuticle of the bitten animal; finally, in a third class of cases, it must be assumed that the bitten animal did not offer conditions required for the development of the disease. A case is, indeed, recorded, on the best authority, in which a pointer dog was caused to be bitten on seventeen separate occasions by dogs affected with rabies, without the disease being induced.

The period of incubation having passed, the first symptoms of rabies usually consist in a change in the temper of the dog, which becomes sullen and snappish, and which often bites those around it, even without any provocation. This prominence of the cerebral symptoms in the early stages of hydrophobia in the dog is very remarkable, and contrasts, as will be seen in the sequel, with the phenomena of the disease in man. It is evidenced not merely by the tendency to bite, but by the whole changed aspect of the animal, which is now observed to be obviously ailing. The appetite becomes capricious, food often being refused, and all kinds of rubbish swallowed, and often, though by no means invariably, the dog utters dismal howls. It is in this stage that the dog often wanders from home, and apparently under the influence of maniacal excitement, rushes on, biting all dogs which it meets, and often all human beings who happen to come in its way. It is to be noted that the dog does not exhibit any of the dread of water which is so painfully evident in the disease as it affects man; this depends upon the fact that in the dog there appears to be little, if any, tendency to spasm of the muscles of deglutition. As the disease advances palsy of the posterior extremities often occurs; in other cases a peculiar paralysis of the muscles connected with the lower jaw sets in, so that the suffering animal is unable to utter any sound, and is said to be suffering from "dumb-madness." Throughout the disease there is usually an increased secretion of viscid saliva. The

whole course of hydrophobia in the dog is run in from four to eight days, the majority of cases proving fatal about the fourth or fifth day. This short description of rabies or hydrophobia, as it affects the dog, is almost exactly applicable to the disease as it occurs in other domestic animals; a maniacal excitement and a tendency to injure men and animals with which they come in contact being as characteristic of herbivorous animals as it is of dogs, cats, foxes, and wolves.

Having, then, before us an outline of hydrophobia as it affects the lower animals, let us compare with it the disease as it is observed in man.

In the first place as to the frequency with which the bite of a mad dog is followed by hydrophobia. No general satement can be made on this matter, as the results vary very greatly according to the part bitten, according to the treatment to which the bitten part is subjected, &c. For instance, bites inflicted upon parts protected by clothing are followed by hydrophobia much less frequently than those in which the hand or face is injured, the poison in the former case being absorbed by the intervening clothing.

Next, as to the period of incubation. In man this varies even more than in the case of the dog; the majority of cases of human hydrophobia have, however, a period of incubation which varies between thirty and fifty days, though exceptional cases occur in which many months have elapsed between the infliction of the bite and the supervention of the symptoms; these remarks might be illustrated by reference to cases which have occurred in England, and which have been recorded in the medical journals during the last two years; the shortest period of incubation observed within this period having been eighteen days, and the longest nine months. During the period of incubation there is nothing to distinguish a bite inflicted by a rabid dog from the bite of a healthy dog. The study of some of the recorded cases of the disease would almost lead to the conclusion that in man there is during the period of incubation a tendency to nervous depression and melancholia which is a precursor of the terrible symptoms which are to follow; it is obvious, however, that great caution ought to be exercised in the interpretation of such mental symptoms, which are after all in many cases but the necessary and logical results of an injury of which the possible consequences are but too well known and correspondingly dreaded. If we except these symptoms of depression and melancholy there are no characteristic phenomena which intervene between the infliction of the bite and the onset of the attack of hydrophobia.

In a certain number of cases the advent of the disease is ushered in by pain of a neuralgic character in the bitten part; this appears to be merely an evidence of the general feeling of illness which then supervenes, rather than any evidence of the specific nature of the bite. More commonly the first phenomena are merely vague symptoms of feeling very unwell, accompanied often by an intense feeling of melancholy. A deep sighing character of the inspirations, or even paroxysmal attacks of difficulties of breathing, with some pain in the throat and pain in the præcordial region often follow. Beyond the feeling of impending evil, there is no mental symptom at this stage of the disease at all comparable with those observed in the lower animals. Next in the order of accession is the difficulty which the poor patient experiences in swallowing; this, at first slight, symptom soon acquires a terrible intensity; the patient is troubled by an agonising thirst, and yet dares not drink; any attempt to drink gives rise to a terrible spasm of the muscles engaged in deglutition, and apparently to a simultaneous spasm of the muscles engaged in inspiration so powerful that he dreads suffocation. An analysis of the symptoms at this stage leads one, indeed, to the opinion that swallowing is often dreaded because of, and is indeed impeded by, the spasm of the inspiratory muscles which it induces. Then follows a stage in which often, though by no means invariably, the patient becomes subject to delusions, and often violently maniacal, and this is succeeded by a stage of exhaustion and quiet which ushers in the fatal termination.

If we have sketched with some degree of minuteness the outlines of a very painful picture, we have done so because a knowledge of them was absolutely essential before we could attempt to consider what light science has thrown upon this dread disease, and what reliance is to be placed upon the remedies which have been suggested for its cure.

We shall now, in the first place, consider the results of pathological investigations relating to hydrophobia. Are there not some well marked and constantly present lesions of the great nerve-centres corresponding in some measure to the symptoms which manifest themselves during life? The older observations generally concur in showing that the brain and spinal cord are the seat of congestions which are, however, not sufficiently constant in their localisation to admit of any conclusions being drawn from them. And, since the time when pathological anatomy attained its present development and accuracy, but few persons sufficiently competent to draw accurate conclusions from their observations have had the opportunity of working at the subject. From the observations of Benedict (*Virchow's Archiv*, 1875), it resulted, that in addition to more or less widely spread congestion, there occur granular degeneration of nerve-cells, and of nerve-fibres in various parts of the brain. Subsequently Wassilieff, working under the direction of Prof. Botkin, of St. Petersburg, described (*Centralblatt f. d. med. Wissenschaft*, 1876, p. 625) *a*, some alterations in the nerve-cells of the medulla oblongata, the outlines and nuclei of which are indistinct and the contents cloudy; *b*, a large accumulation of corpuscles of the size of white blood-cells in the interstitial connective tissue of the brain, in the peri-vascular canals and immediately surrounding them; and *c*, the presence of a highly refracting substance in the peri-vascular spaces, especially in the cortical layers of the cerebral hemispheres. Somewhat akin to them are the observations of Dr. Gowers who found in the medulla oblongata after death from hydrophobia, accumulations of cells, resembling white blood-cells, in the vicinity of the blood-vessels, and also in the nervous substance. But what do all these observations indicate? In all probability the accumulations of white cells are caused by the emigration of white blood corpuscles from the blood, so that they are to be held as supporting the older observations which alleged congestions of the brain to be frequently present, and the other pathological changes

noticed by the three observers to whom we have referred, cannot as yet be adequately interpreted.

Pathological anatomy then helps us a little in our attempts to elucidate hydrophobia. Can we obtain better results by reasoning upon the symptoms and course of the disease from the standpoint of physiology? Hardly, but we may make the attempt. Physiology necessarily cannot help us to understand the nature of the peculiarly subtle poison which can lurk so long in the system without betraying its presence by any symptom, but she may help us in explaining the phenomena which it induces. Of this poison we know as little, if not less, than of the other poisons which are capable of inducing zymotic diseases. Each of those diseases appears to depend upon a definite *materies morbi*, upon the presence of which the peculiar phenomena of each depend; but the periods which elapse between the introduction of the poison and the manifestation of the disease varies in each case, no less than the course and duration of the disease, and the organs and tissues of the body which are affected. Thus, in scarlet fever the poison induces changes in the epitheliated surfaces of the body, manifested by the rash, the sore throat, the acute kidney affection; in typhoid fever anatomical changes of the most obvious nature are wrought in the alimentary canal, and lead to the special dangers of the disease; in typhus, again, the poison, whilst producing changes in the general nutrition of the body, and exciting a specially-marked action upon the brain proper (as evidenced by the marked affection of all mental processes), produces no typical anatomical changes. These diseases all illustrate the fact that the poison of each zymotic disease affects certain tissues and organs of the body, and it might be easily shown that it is by the implication of particular functions that each of these poisons *usually* induces death. Is there, in the case of hydrophobia, any evidence that it affects specially any particular organ of the body? Yes; a physiological analysis of the disease reveals the fact that its symptoms depend upon an affection of the nerve-centres, and especially of the medulla oblongata.

These essential symptoms are—the spasmodic difficulty of breathing, which depends upon a spasm of the *inspiratory* mechanism and a spasmodic affection of the group of muscles engaged in deglutition. The nerve-centres which preside over respiration and the co-ordinated movements of deglutition are situated in the medulla oblongata, and it is these centres which appear to be peculiarly affected. The reflex excitability of this portion of the nervous apparatus becomes first of all heightened so that a stimulus applied to the mucous membrane of the gullet, which in health would give rise to a normal contraction of the muscles of deglutition, travelling on to the morbidly irritable medulla, throws the centre presiding over deglutition into a state of tonic spasm so intense as to be acutely painful; not confining its action to this one centre, the stimulus is able to throw the contiguous respiratory centre into a similar state of spasm, and the patient runs the risk of suffocation because the movements of the thoracic box, which are essential causes of the passage of air into and out of the lungs, cease for a time. The mechanism of suffocation in these cases resembles that observed when the upper end of the pneumogastric nerve is stimulated by a succession of strong induction shocks, except that in hydrophobia the abnormal effect is doubtless due not to the intensity of the stimulus, but rather to the heightened excitability of the nerve-centres implicated. Apparently a subtle animal-poison acting upon an intensely vulnerable but limited part of the nervous mechanism induces in it an action similar in kind to that produced by strychnia upon the spinal cord. Under the influence of this well-known poison the excitability of the nerve-centres in the cord is heightened, so that a stimulus reaching it by an afferent nerve which would in the healthy unpoisoned condition lead to the reflex and painless contraction of but a small group of muscles, will be able to throw the nerve-cells of the whole cord into intense activity, and as a result occasion the characteristic and terribly painful convulsions of strychnia poisoning. There are, indeed, other facts besides those previously mentioned which point to a state of irritation and increased nervous excitability of the medulla and contiguous nerve-centres. Thus it has been observed that occasionally the pulse has been abnormally slow, a result almost certainly due in these cases to an excitation of the inhibitory centre in the medulla—of that centre which exerts a moderating or restraining influence upon the heart's action; further, it not unfrequently happens that towards the close of the hydrophobic stage, stimuli which were at first only capable of inducing the spasms of deglutition and inspiration, are able to bring on attacks of general convulsions. Here we have a still further extension of the effects of the irritation due to an extension of the reflex excitability from the medulla to the spinal cord.

Our analysis of the symptoms of hydrophobia reveals that as a rule the spasmodic stage terminates before death, which is not produced, as in strychnia poisoning, by the mechanical result of the convulsions—suffocation—but apparently by a more general, though we confess unknown, action of the poison on the organism generally. We know as little of the mode of death in this case as we do in that of scarlet fever, or diphtheria, or typhus, each one of which *may* produce death without leading to the anatomical results which, at any rate in the case of the two former of these diseases are their usual accompaniments. Zymotic poisons may indeed leave as few traces of their action as the simpler and better known poisons such as prussic acid or morphia, so that whilst we cannot disregard the local manifestations or changes which they induce, and which of themselves are a frequent source of danger, we must admit that they are in many cases—nay in most cases—secondary in importance to the more general phenomena which are the expression of the poisonous influence affecting the organism.

(*To be continued.*)

ANCIENT HISTORY FROM THE MONUMENTS

Ancient History from the Monuments. The History of Babylonia. By the late George Smith; edited by A. H. Sayce. *The Greek Cities and Islands of Asia Minor.* By W. S. W. Vaux. (Society for Promoting Christian Knowledge, 1877.)

THE Society for Promoting Christian Knowledge has been doing a very useful work in acquainting the public with the historical results of recent Oriental research

in a cheap and handy shape. The work has been wisely placed in the hands of those who have themselves been pioneers in the task of discovery, and the reader has thus been secured against the errors and unfounded conclusions almost inseparable from second-hand information. The histories of Egypt, Assyria, and Persia, have now been followed up by those of Babylonia and Asia Minor, and the fact that the history of Babylonia was the last literary work which Mr. George Smith, the indefatigable Assyrian explorer, lived to accomplish, gives a melancholy interest to it over and above that of its subject matter. Indeed, the materials for reconstructing Babylonian history are still but scanty, and must remain so until systematic excavations can be made among the buried cities and libraries of ancient Chaldea. With the exception of a few early bricks and a few dedicatory inscriptions of Nebuchadnezzar and his successors, it is from the clay tablets of Nineveh that almost all our knowledge of the sister kingdom has been derived. Even Babylonian chronology is still in an uncertain and tentative condition, and the fragments of the Babylonian historian, Berosus, help us but little. Whole periods must still be left blank, and though one or two dates, like the conquest of the Elamite king, Cudur-nankhundi, in B C. 2280, can be fixed by the aid of later monuments, the relative position of even whole dynasties has not yet been settled. Our acquaintance with the mythical epoch is quite as great as with the historical epoch ; the Assyrians preferred the legends of the rival monarchy to a record of its glories, and while, therefore, we now have in detail the stories of the creation, of the flood, or of the hero Izdubar, we know comparatively little of the political changes which passed over the Babylonia of history. Compared, however, with what we knew of them a few years back, even this limited knowledge seems large and accurate, and the best evidence of this is the volume which Mr. Smith has written, and which would have been an impossibility but a short time ago. Those who wish to learn what light has been thrown by cuneiform discovery on this important section of ancient history cannot do better than refer to his book. The importance of Babylonia for the history of culture and civilisation is daily becoming more manifest ; the early Accadian population of the country, who spoke an agglutinative language and invented writing, left a rich inheritance of art, science, mythology, and religious ideas to their Semitic successors, and through them to the Jews and Greeks. The latter were influenced partly through the Phœnicians, partly through the nations of Asia Minor. Mr. Vaux's volume on the Greek cities of Asia Minor is therefore a suitable companion to Mr. Smith's "History of Babylonia." His difficulty in compiling it must have been the converse of Mr. Smith's, as here it was not the meagreness but the superabundance of materials which was likely to cause embarrassment. His selection, however, is good and judicious, and the book he has produced is at once instructive and readable. He has not forgotten to invoke the assistance of the latest discoveries ; the first few pages are devoted to an account of Dr. Schliemann's life and discoveries, and the researches of Newton, Wood, and Fellows, have been largely drawn upon. Considering the space at his command, Mr. Vaux must be congratulated upon the amount he has been able to cram into it, and, so far as we can see, no city or fact of importance has been omitted. Both volumes are appropriately illustrated, and the "History of Babylonia" contains a copy of a bronze image of an ancient Chaldean monarch recently brought to the British Museum, and interesting on account of the rarity of such early monuments. Their value is further increased by the addition of indices, and the editor of Mr. Smith's volume has added a chronological table of the Babylonian kings, and an explanatory list of proper names.

FRENCH POPULAR SCIENCE

Musée Entomologique Illustré. Les Papillons : Organisation, Chasse, Classification. 80 Plates and 260 Woodcuts. *Les Coléoptères : Organisation, Mœurs, Chasse, Collections, Classification.* 48 Plates and 335 Woodcuts.

Anatomie et Physiologie de l'Abeille. Par Michael Girdwoyn. 12 Lithographic Plates.

Les Champignons. Par F. S. Cordier. 60 Chromolithographs and 8 Woodcuts.

Les Prairies Artificielles. Par Ed. Viaune. 127 Woodcuts.

Les Ravageurs des Forêts et des Arbres d'Alignement. Par H. De la Blanchère. 162 Woodcuts.

Les Ravageurs des Vergers et des Vignes ; avec une Étude sur le Phylloxera. Par H. De la Blanchère. 160 Woodcuts.

Le Chalumeau. Analyses Qualitatives et Quantitatives. Guide Pratique. Traduction libre du Traité de B. Kerl. Par E. Jannettaz.

Les Aliments. Determination Pratique de leurs Falsifications. Par A. Vogl. Traduction par Ad. Focillon. 160 Woodcuts. (All published by J. Rothschild, Rue des Saints-Pères, Paris.)

WE have received the preceding batch of works from the house of Rothschild of Paris. This is not the first time we have been able to show not only how worthily M. Rothschild is maintaining his position as one of the first publishers of popular science works of the time, but how eagerly such works are read, and how highly they are appreciated in France. It is impossible to speak too highly of the honest work which has been put into each of the volumes, while many of them are written by men whose names are widely known on this side the Channel. As is proper in this style of literature, the text is equalled by the illustrations. Why is it that in the matter of illustrated books such as those before us, the French finished product is so far superior to nine-tenths of those published on this side the water? Nothing can exceed the perfection of many of the hundreds of woodcuts in the above volumes, while we have rarely seen more finished specimens of chromolithography than those to be found in some of the volumes.

We cannot think that the French public is so far beyond our own in its appreciation of science, as to make the publication of similar works in our own country hopeless. We shall therefore give an analysis of each of the above works in a single article, with a view of showing the treatment adopted abroad in popularising the branches of science with which the volumes deal, instead of devoting

one to each of them in turn, which we should have been quite justified in doing, having regard to their value.

Of the two volumes on the Natural History of Insects, which are published by a society of French and foreign entomologists, vol. i. is devoted to the Coleoptera, and comprises their organisation and their different orders, with a short description of each, and woodcuts showing their different stages of development. These are followed by other useful matter, and then, in the second part, we come to "Le Monde des Scarabées." The stag-beetle is here taken as an example of his family to show the anatomy of these insects. The description of their dwellings and instincts is clear, and written in such a style that it may be understood and enjoyed by those not versed in entomology. This part occupies a good portion of the book. In the pages devoted to the hunting, preparing, and keeping of beetles, beginners may find every information they require; pincers, pins, and nets are all shown, as well as the necessary requisites for the knapsack. A list of the principal entomological works is given, after which we have a lengthy classification and iconography of European coleoptera, illustrated with forty-eight plates beautifully coloured by hand.

The arrangement of volume ii., on Butterflies, is very similar to the above, and contains thirty coloured plates illustrating the butterfly, caterpillar, and chrysalis, together with the plants on which these are most frequently to be found.

"The Anatomy and Physiology of the Bee," is taken from volume vi. of the "Memorials of the Polish Society of Exact Sciences in Paris," and translated into French by M. Pillain. This work consists of twelve lithographic plates which obtained medals of merit both at the Universal Exhibition at Vienna and from the Royal and Imperial Society of Agriculture of Cracow. On these plates we have 172 figures of the various parts of a bee, greatly magnified. It is scarcely necessary to add that these are extremely well finished. In the folio we have the memoir itself, a book of forty pages, which first introduces us to the bees of different countries and the bibliography of the subject. In chapter i. the author describes the exterior parts of the bee, and in the second and third the interior and more complicated, such as the muscles, nervous system, circulation of the blood, &c. The work terminates with explanations of the figures. Altogether this is a valuable addition to an entomologist's library, and does great credit to the society from which it has emanated.

In M. Cordier's book on Fungi we have much valuable information. In the first place he treats generally of the organisation of fungi, their physiology, mode of reproduction, and geography, how to distinguish the edible from the poisonous, and he shows us how to extract this poison; he tells how this works on the animal economy and the best means of counteracting it. In the second part all the fungi useful to man are chronicled, with detailed descriptions of each order and drawings from nature.

M. Cordier has adopted Persoon's classification in preference to any other, as he takes it to be more practical; indeed he dedicates the book to his memory as the "Créateur de la Science Mycologique." The drawing and colours of the sixty chromolithographs are well worthy of note. The book also contains a glossary, table of common, and one of the scientific, names of the fungi.

The author has evidently endeavoured to make his subject as interesting and complete as possible. The style of the popular portion of the book is admirable, and *bon vivants* will be glad to be informed that there are eight pages dealing with the proper way of cooking truffles.

The two small books by H. de la Blanchère—one on the enemies of forest trees, with 162 engravings of insects and larvæ, the other on the enemies of orchards and vines similarly illustrated, form part of a large series now well known and highly appreciated in France. We have already noticed some of them, and these are in no way inferior to the former ones.

"Plants used for Food," written by A. Vogl, of Prague, translated into French by Ad. Focillon, is a practical guide for detecting the adulteration of flour, coffee, chocolate, tea, and the like.

"The Blowpipe," by E. Jannettaz, is extremely well arranged, and is a thoroughly practical guide for engineers, mineralogists, &c.; the information is accurate and condensed, and M. Jannettaz's name is a guarantee of its scientific value.

OUR BOOK SHELF

The Fifth Continent, with the Adjacent Islands; being an Account of Australia, Tasmania, and New Guinea, with Statistical Information up to the Latest Date. By C. H. Eden. With Map. (London: Society for Promoting Christian Knowledge; no date.)

THIS volume contains much information on the Australian colonies, but it is somewhat desultory and incomplete. It is not a children's book, and it will not satisfy those who are in quest of full information on the subject. It affords some idea of the history, people, and products of Australia and New Guinea, but it would be better to cut out much of what is said about the history and the people and give more space to well-digested information about the resources of the countries.

Notes by a Field Naturalist in the Western Tropics. By Henry H. Higgins, M.A. (Liverpool: Edward Howell, 1877.)

THIS is a readable record of observations made during a yacht voyage to the West Indies by Mr. Higgins, who is president of the Liverpool Naturalists' Field Club. Mr. Higgins went over well-trodden ground, and therefore we need not look for any novelties in this little volume, although much of it is interesting. The chief purpose of the voyage, undertaken by Mr. Cholmondley, the owner of the yacht, was to observe and collect tropical birds. Mr. Higgins collected, also, many specimens, both zoological and botanical, from sea and land, which are now being arranged. He may possibly, he states, publish an account of the biology of the voyage.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

The Radiometer and its Lessons

PROF. FOSTER'S clear representation of what he conceives to be the effect of rarefaction reduces the question between us to a definite issue.

Having assumed that heat is flowing across an intervening layer of gas from a hotter surface A to a colder surface B, he

says:—"Then, I imagine, the flow of heat through the gas will take place *as though* there were, in contact with each solid surface, a layer of gas whose temperature is throughout the same as that of the contiguous solid, and whose thickness is equal (or at least proportional) to the mean length of path of the molecules." Without these layers of uniform temperature or whatever may produce an equivalent effect it follows directly from Prof. Foster's reasoning that the rate at which heat is communicated is, as I maintain it is, independent of the density, whereas *if there were* any such layers I should at once admit the force of Prof. Foster's reasoning. The whole question turns therefore on the existence of these layers of uniform temperature.

Now what evidence of such layers have we? No experimental evidence certainly; and not only has the kinetic theory not as yet been applied to explain their existence but it is easy to demonstrate that according to this theory no such layers or any equivalent can exist. For in order that the condition of heat may remain unaltered it is necessary that the rate at which heat is transmitted across all surfaces parallel to the solid surfaces which can be drawn through the gas should be the same. And the rate at which heat is transmitted is for small variations of temperature proportional to the degradation of temperature, hence if there were a layer of uniform temperature no heat could be transmitted.

It is surely incumbent on Prof. Foster in assuming the existence of these layers to give some sort of proof in support of his assumption, but not one word does he say!

I cannot allow this to pass without pointing out that the description which Mr. Stoney has given of my view is grossly wrong and is certainly not to be gathered from anything I have written. Mr. Stoney carefully turns my position. He makes out that I have explained the action in question as *arising* from *convection currents*, whereas I have from first to last maintained that it is these currents which oppose and eventually overcome the action. He makes out that my theory takes no account of molecular motion, whereas, in truth, it takes no account of anything but molecular motion, the effect of the expansion of the gas being so obviously trivial that I have never even mentioned it.

Your readers may judge of this by comparing the first of the following quotations, which is from Mr. Stoney's letter, with the others which are from my own papers, and are the only expressions, not mathematical, which I have given of my views as to action in the question:—

Mr. Stoney's.

"Prof. Osborne Reynolds's explanation is based on the fact that when a disc with vertical sides is heated on one side and exposed to a gas, a convection current sets in, which draws a continuous supply of cold gas into contact with the hot surface of the disc. As this cold gas reaches the disc it is expanded, and thus its centre of gravity is thrown further from the disc. Accordingly, the disc, if freely suspended, will move in the opposite direction so as to keep the centre of gravity of the gas and disc in the same vertical line as before, and, if not freely suspended, will suffer a pressure tending to make it move in that direction. If I have understood Prof. Reynolds aright, this is both a correct and full description of his explanation as last presented."

My Own.

"Whenever heat is communicated from a hot surface to gas, the particles which impinge on the surface will rebound with a greater velocity than that with which they appropriate; and consequently the effect of the blow must be greater than it would have been had the surface been of the same temperature as the gas.

"And, in the same way, whenever heat is communicated from a gas to a surface, the force on the surface will be less than it otherwise would be, for the particles will rebound with a less velocity than that at which they approach.[1]

"These forces arise from the communication of heat to or from the surface from or to the gas. These forces will be directly proportional to the rate at which the heat is communicated; and since this rate has been shown by Prof. Maxwell to be independent of the density of the gas, these forces will be independent of the density of the surrounding medium, and their effect will increase as the density and convection-currents diminish."[2]

[1] *Proceedings*, Royal Society, 1874, p. 407.
[2] *Phil. Mag.*, November, 1874, p. 3.

The first of the quotations from my papers is followed by a mathematical expression on which I have depended for completeness, and from this expression, in which neither convection currents nor the expansion of the gas have any place whatsoever, it follows that whenever heat is steadily diffusing *into* or *through* a gas, the momentum transmitted across any surface in the direction in which the heat is diffusing will be greater than that transmitted in the opposite direction by a quantity proportional to the rate at which the heat diffuses, divided by the square root of the absolute temperature of the gas.

As to the value of what follows in Mr. Stoney's letter, I must leave it to your readers to decide. He proceeds to claim that his own theory has the advantage of being based on molecular motions, he says:—

"My explanation, on the other hand, is based on *molecular* motions which go on in the gas without causing any molar motion, and is independent of convection currents."

Then having thus attributed to me an explanation, I never even thought of offering, and having assumed the true base of my theory as alone belonging to his, he proceeds to show wherein I am wrong. And in every subsequent position which he attributes to me, he is as wrong as he is in his first statement.

Under these circumstances it would be useless for me to enter upon questions as to how far "diffusion," according to the kinetic theory may be more "sluggish" than Mr. Stoney's "penetration," or to discuss further the possibility of his "Crookes's layers."

In my last letter I showed that the condition of a gas which Mr. Stoney called a "Crookes's layer" was impossible, and I do not see that Mr. Stoney has improved his position by showing that he had arrived at the possibility of the condition by making the false assumption "*that gas is a perfect non-conductor of heat.*"

Wherein Mr. Stoney's views are at variance with the results of the laborious investigations of Maxwell, Clausius, Thomson, and others, he may best convince himself by referring to the works of these authors. Until he has read my papers and explained the discrepancies between his views and the generally-accepted laws of gases, I do not see that we have any common ground for discussion. OSBORNE REYNOLDS

November 30

Mr. Crookes and Eva Fay

IF Mr. Wallace had read my letter in NATURE of November 29 with a little more attention, he would have seen that I did *not* refer to the *Daily Telegraph* "as an authority in a matter of scientific inquiry," but that the account I gave of Mr. Crookes's "scientific tests" was given *in Mr. C.'s own communication to the 'Spiritualist,'* which would have been reproduced without abridgment if the columns of NATURE could have admitted it.

What I hold myself pledged to show (in NATURE, if it pleases, as well as in the new edition of my Lectures) is that the "tying-down by electricity" described by Mr. Crookes in the *Spiritualist*, is no more effective in preventing the performance of juggling tricks than Eva Fay's *ordinary* tying-down under which her tricks were publicly reproduced two years ago by Messrs. Maskelyne and Cooke. And since Mr. Crookes made no mention of the *extraordinarily* sensitive galvanometer he used, which is described for the first time by Mr. Wallace in the last number of *Fraser*, I only consider myself bound to show the *method* by which, with *ordinary* apparatus, the electric test may be evaded—the trained skill of the acute *trompeuse* being very probably required to meet the more severe test now first specified.

Mr. Wallace seems to me to have been a little hasty on another point. "The supposed *exposure* of Eva Fay in America," he says, "was no exposure at all, but a clumsy imitation." As this is merely Mr. W.'s *dictum* founded upon an imperfect newspaper report, I prefer to trust the judgment of the eye-witnesses who have publicly testified to *the completeness of the exposure*. Among these are not only three of the ablest men in New York (the Rev. Dr. Bellows, Ex-Surgeon-General Mott, and Dr. Hammond), but the reporters of the very newspaper referred to which had previously shown a decided leaning to the claims of spiritualism. And their judgment is confirmed by the fact (which Mr. Wallace probably considers as a newspaper fiction, but of which I have independent testimony) that *Eva Fay was forced by the local authorities to take out a licence as a juggler* as a condition of the continuance of her public performances.

The fundamental difference between Mr. Wallace and myself as to the validity of testimony in regard to the "occult" comes out so strongly in this case that we have really no common

ground for a discussion which I cannot consider it profitable to continue. WILLIAM B. CARPENTER

The Glacial Geology of Orkney and Shetland

OWING to an accident I did not see your number of September 13 containing my letter on the glacial geology of Orkney and Shetland and Prof. Geikie's article (vol. xvi. p. 414), until my return from Scotland a few days ago. Otherwise I should have troubled you sooner with a few observations thereon.

In the first place I wish to thank Prof. Geikie for the very courteous manner in which he has referred to the remarks of an outsider who has ventured to intrude on what the Professor has made, to such an extent, his own peculiar province.

In the next place I am glad to find that upon what was the most important fact in my statement, viz., the absence of raised beaches or other signs of recent elevation of the land in Orkney, Prof. Geikie agrees with me.

I call this the most important because it bears directly on the theory of wide-spread changes in the relative level of sea and land owing to secular causes, such as a change in the axis of the earth's rotation, or in the position of its centre of gravity. If it can be proved that the difference of level, which caused the raised beaches of the south of Scotland, and extended north along the coast of Ross and Sutherland, dies out as we proceed further north, and disappears altogether in Orkney and Shetland, it is truly a crucial experiment which shows that these raised beaches are due to local elevations of the land, and not to a general sinking of the sea.

This is the conclusion to which Prof. Geikie points, though he naturally finds it difficult to understand why the upheaval, so marked in Sutherland, did not affect Caithness and Orkney.

I believe I can add a few facts which may assist in removing these doubts.

At one of the places in Caithness mentioned by Prof. Geikie, where the existence of a raised beach might be possible, viz., in the sheltered Bay, between Freswick and Wick, I believe there is one, though less strongly marked and at a lower elevation than those in similar situations in Sutherland. I allude to a terrace which bounds the links of Keiss Bay, about half a mile inland from the present coast-line. I cannot speak positively, not having seen it for some years; but my recollection is that it is a perfect miniature reproduction of the terraces round Brora and other bays in Sutherland. If so, it is a positive proof that the elevation of the land died out towards the north, and we might reasonably suppose that somewhere about the line of the Pentland Firth was the neutral axis, on one side of which the land rose, while on the other it fell.

Be this as it may, the *fact* is, I think, incontrovertible that Orkney did not share in the southern movement of elevation. This rests not only on the absence of raised beaches, forming terraces, which might possibly have disappeared, but still more on the absence of all traces of marine action, such as pebbles, sand, or shells, on the low plains which must have been submerged.

I would ask Prof. Geikie to consider whether the single instance of the Loch of Stennis is not conclusive. If the sea had ever stood twenty or thirty feet higher relatively to the land than it now does, the whole plain up to the hills must have been a sheltered, shallow, inland fiord.

As the land rose to its present level this must have left not only a terraced beach at the foot of the hills, which might possibly have disappeared (though it is hard to see why it should have done so in such a sheltered situation), but the whole plain must have been a raised sea-bottom, strewed over with pebbles, sand, and shells. These could not have disappeared, and as they are nowhere visible and the plain consists everywhere of the ordinary rock, with a thin mantle of soil resulting from its disintegration by ordinary atmospheric causes, I am, I think, justified in assuming it to be proved that Orkney did not share in the recent movement of elevation which affected the rest of Scotland.

Now one word as to glaciation. I can assure Prof. Geikie that I do not think for a moment of setting my authority against his, and that if he is right in the instances of glaciation he tells us he has observed in Orkney, so far from being disappointed, I shall be pleased, for it will clear up what has long seemed to me a perplexing anomaly.

Of course Orkney must have experienced the full rigour of the glacial period, and it is only natural to expect that it should show the same abundant signs of glaciation as the adjoining counties of Scotland. Prof. Geikie will therefore excuse me if I still retain a little of that healthy scepticism which is so conducive to the establishment of truth, and venture to plead that judgment may be suspended until there is further evidence. I do so mainly because the Professor's own statement is that during his visits to Orkney his attention was devoted mainly to the old red sandstone, and his remarks on glaciation were only incidental. Now there are some proofs of glaciation which are so obvious that there can be no mistake about them, others which may easily be mistaken, and which require close examination by a practised eye directed specially to them, to arrive at a just conclusion.

Boulders of foreign rock, perched blocks, rocks unmistakably rounded and polished by the ice plane, are among the former. But striæ require great practice and careful examination to be sure of them in a district of finely laminated sandstones which weather constantly into parallel lines or grooves. Stony clay again, from disintegrated rock, is often so like boulder clay that it requires close observation to distinguish one from the other. And finally where steep hills have crumbled away and filled up many places in the narrow valleys between them with their *débris*, as at Hoy, the appearances are very like those of glacial moraines.

Now I observe that nearly all the conclusive proofs of glacial action are wanting in Prof. Geikie's enumeration. He has not seen, or heard of anyone who has seen, a single boulder or perched block, or even a single piece of foreign stone in Orkney.

As regards boulder-clay I would join issue on his instances, taking especially that of Kirkwall Bay, because it is typical of the other cases and so easily accessible that the facts can readily be verified.

I believe it to be disintegrated and not boulder clay, for the following reasons:—

1. The clay is not compact like that of genuine boulder-clay, but of looser structure, and often clearly made up of minute splinters of the disintegrated rock.

2. The stones in the clay are never foreign stones, and are not scattered irregularly, as if shot out into a huge rubbish heap, as in true boulder-clay, but arranged for the most part so that the original lines of stratification can be followed.

3. If the section which resembles boulder-clay be followed up, it will be found to merge insensibly in what is unmistakably the common disintegrated surface soil of the district.

There only remains the question of *roches moutonnées*, and here I speak with the greatest diffidence, for certainly Prof. Geikie ought to know a great deal better than I whether a hummock of rock is or is not "admirably ice-worn and striated" like those behind Stromness.

I can only say that I have looked at them often, and they appear to me to be very different from the *roches moutonnées* of which I have seen so many in Scotland, Wales, and Switzerland. They are not rounded, smooth, and polished, as if planed into shape by some gigantic tool, but simply irregular hummocks of rock, sometimes smooth and sometime rough, according to accidents in the bedding and weathering of the strata. So at least they seem to me, and even in the valleys of Hoy, where, if anywhere, there were local glaciers, the sections shown by the small streams and low coast-line, always, I believe, exhibit the same appearance of sandstone strata, coming at an angle to the surface, and with their edges not planed off, but passing gradually into surface soil by disintegration.

Of course I make these statements subject to correction. It may be that I have failed to see things because my eye is not sufficiently educated. But when we couple what is, I believe, absolutely certain, viz., the absence of the more prominent and obvious proofs of glaciation in the form of boulders and foreign rocks, with the equally certain fact that Orkney was an exception to the general rule of recent elevation, I think Prof. Geikie will admit that the interests of science will be promoted by any remarks which may lead to reasonable doubts, and therefore to conclusive investigation, as to the fact whether Orkney does or does not give proof of having been covered by a great polar ice-sheet during the glacial period. S. LAING

36, Wilton Crescent, S.W.

Explosions

I HAVE been waiting to see if Mr. Galloway's paper on "Explosions in Mines" published in NATURE, vol. xvii. p. 21, would lead to any correspondence. Your readers may be interested in an incident reported to me by the late Dr. Böttinger, of Messrs. Allsopp's brewery, Burton-on-Trent.

In their new brewery, near the railway station, the crushed malt is lifted from one floor to another by a series of cups revolving on a leathern band. The casing, which incloses the band, is full of floating malt dust while the revolution is going on, and on opening one of the doors of the casing a puff of malt-dust is sent out into the room. Soon after the brewery was opened, a workman went with an undefended light to make some examination of the working of the leathern band, and on opening the door of the casing an explosion followed; not of a very serious character, but enough, I think, to throw the band out of gear. The cause of the explosion is evident; the rapid combustion of the fine malt dust with which the air puffed out into the room was charged.

Dr Böttinger died a few years since, but it would probably not be difficult to get accurate details of the accident from Messrs. Allsopp and Sons. A. MACKENNAH

Bowdon, December 11

Means of Dispersal

IN his great work, "Insecta Maderensia," Mr. Wollaston remarks upon the great affinity in the coleopterous fauna of Madeira with that of Sicily, and in his "Coleoptera Hesperidum," on the *northern* character of that of the Cape Verde Isles. Mr. Andrew Murray also found that out of 275 Cape Verde species 91 were common to the Canaries and 81 to the Madeiran group. The last author would seem to rely on the efficacy of now submerged continents as a means of transmission between the two areas.

Towards the end of the fifteenth and commencement of the sixteenth century, the Portuguese carried the sugar-cane from Sicily to Madeira and the Canaries. The means of introduction would probably be the same then as now; the young shoots of cane would be conveyed in boxes or baskets of earth from one locality to the other, as the writer once carried young cane plants from Car Nicobar *viâ* Rangoon to Penang, and has seen the same arrive in the last locality from the West Indies. There can be little doubt that many of these plants must have been carried from Sicily to the Atlantic Isles before a successful or sufficient introduction was made, and with the earth in which the plants were conveyed, many geodephagous and other coleoptera would find an enforced means of migration. The sugar-cane is also reported as having been introduced into Cyprus from Asia, and transplanted from there to Madeira, thus adding another link to the localities in which these coleopteral affinities have been detected.

It is not proposed that this was the sole, but only a probable means of the transmission of common forms in the coleopteral faunas of these widely-separated districts. The number of causes which have been factors to the same in the past may be in an inverse ratio to our knowledge of them. W. L. DISTANT

Supplementary Eyebrows

I MET a gentleman a few days ago who has on either side of the forehead a supplementary eyebrow branching off from the superciliary ridge near the supra-orbital notch, and passing obliquely upwards and outwards for about ¾ inch across the forehead. Beneath these brows, which contain large and coarse hairs, are lines of soft down-like hair, one on either side occupying the usual position of the eyebrows. Since my attention was drawn to this subject I have noticed that many persons have a short secondary spur of hairs at the points indicated. Artists, I believe, have noticed this deviation from the normal eyebrow-line, as we occasionally observe it in portraits of Puck and other mischievous sprites.

There is a spot about midway between the orbits in animals that I have examined (namely, horses, dogs, and cats), whence the lines of hair-insertion into the skin radiate in various directions. If we consider the secondary eyebrows of man as a reversion to an ancestral type, we must conclude that our hairy progenitors also possessed such a radiating point of hair insertion upon their foreheads, and that the secondary eyebrows are only remnants of a hairy covering which originally enveloped the whole face. W. AINSLIE HOLLIS

Brighton

Diffusion or Cohesion Figures in Liquids

WITH reference to the above, allow me to relate some experiments made several years ago, and easily repeated.

1. Take a tall precipitate glass, fill it with water, drop into it a piece of lump or refined sugar and four or five grains of common salt. Let the vessel remain quiet, so that when the sugar is dissolved there may be different densities in the fluid from top to bottom. Then lightly touch the surface with a piece of lunar caustic (silver nitrate), and observe the figure which results.

2. The experiment may be repeated with sugar, diluted sulphuric acid, and barium chloride, the figures varying with the proportions of the ingredients used.

3. Take a common tumbler glass filled with water, dissolve in it half a tea-spoonful of common salt. Touch the surface of the solution with the point of a pen filled with ordinary black ink, and the characteristic figures are produced. F.R.S.

Brighton, December 12

Meteor

AT 8h. 13m. (± 2m.) P.M. on December 9, a brilliant meteor passed from 32 Cameleopardalis (± 1°) through μ Lyræ (± 1°), and disappeared about 6° beyond; time of passage, 1·6 (± ·3) sec.; mag.. 8 (± 2) χ Lyræ; colour, emerald green; track, yellow, visible 1 second; seen from 51° 24′ 43″ N., 2′ 13″ E. This may enable a northern observer to fix the position.

Bromley, Kent W. M. F. P.

ON THE CAUSATION OF SLEEP

THE last number of *Pflüger's Archiv* (vol. xv., p. 573) contains the following interesting note by Dr. Strümpell:—

"In the autumn of last year there was received into the medical clinik of Leipzig a youth, aged 16, in whom various phenomena of anæsthesia gradually developed themselves to an extent which has very rarely been observed. The skin of the whole surface of the body was completely insensible, and that in respect to every kind of sensation. The most powerful electric current—a burning taper held to the skin—was not able to produce any pain or even a sensation of touch. Almost all the accessible parts of the mucous membrane of the body exhibited the same insensibility to pain. Also all those sensations which are classed together under the name of 'muscular sense,' were entirely absent. The patient, when his eyes were closed, could be carried about round the room, his limbs could be placed in the most inconvenient positions without his being in any way conscious of it. Even the feeling of muscular exhaustion was lost. In addition there came on also a complete loss of taste and smell, amaurosis of the left eye, and deafness of the right ear.

"In short, here was an individual whose only connection with the outer world was limited to two doors of sense—to his one (right) eye, and his one (left) ear. Moreover, both these remaining doors could at any time be easily closed, and in this way it was possible to investigate the consequences of completely isolating the brain from all external stimulation through the senses. I have frequently made the following experiment, and often showed it to others:—If the patient's seeing eye was bandaged and his hearing ear was stopped, after a few (usually from two to three) minutes the expression of surprise and the uneasy movements which at first showed themselves ceased, the respiration became quiet and regular; in fact the patient was *sound asleep*. Here, therefore, the possibility of artificially inducing sleep at any time in a person simply by withholding from the brain all stimulation by means of the senses was realised.

"The awakening of the patient was as interesting as the sending him to sleep. He could be awakened by an auditory stimulation, as, for example, by calling into his hearing ear or by visual stimulation, by allowing the stimulus of light to fall upon his seeing eye; but he could not be woke by any pushing or shaking. If he was left to himself he did eventually wake up of his own accord in course of the day, after the sleep had lasted many hours, the awakening being due, it might be, to intrinsic stimuli started in the brain, or it might be to slight external unavoidable stimuli acting through his still functional sense organs, and making themselves felt in consequence of the sensitiveness of the brain being increased during the repose of the sleep."

THE MODERN TELESCOPE[1]

II.

WHETHER the telescope be of the first or last order of excellence, its light-grasping powers will be practically the same; there is therefore a great distinction to be drawn between the illuminating and defining power.

FIG. 5.—Saturn and his moons (general view with a 3¾-inch object-glass.)

The former as we have seen depends upon size (and subsidiarily upon polish), the latter depends upon the accuracy of the curvature of the surface.

If the defining power be not good, even if the air be perfect, each increase of the magnifying power so brings out the defects of the image, that at last no details at all are visible, all outlines are blurred or stellar character is lost. Even with the best telescopes the power should not be strained.

The testing of a glass therefore refers to two different qualities which it should possess. Its quality as to material and the fineness of its polish should be such that the maximum of light shall be transmitted. Its quality, as to the curves, should be such that the rays passing through every part of its area shall converge absolutely to the same point, with a chromatic aberration not absolutely *nil*, but sufficient to surround objects with a faint violet light. With the reflector we have to consider the brilliancy of the surface and the perfection of curvature.

In close double stars, therefore, or in the more minute markings of the sun, moon, or planets, we have tests of its defining power; and if this is equally good in the instruments examined, the revelations of telescopes as they increase in power are of the most amazing kind.

A 3¾-inch suffices to show Saturn with all the detail shown in Fig. 5, while Fig. 6 shows us the further minute structure of the rings which comes out when the planet is observed with an object-glass with an aperture of 26 inches.

In the matter of double stars, a telescope of 2 inches

FIG. 6.—Details of the ring of Saturn observed by Trouvelot with the 26-inch Washington Refractor.

aperture, with powers varying from 60 to 100, should show the following stars double:—

Polaris.	γ Arietis.	α Geminorum.
α Piscium.	ρ Herculis.	γ Leonis.
μ Draconis.	ζ Ursæ Majoris.	ξ Cassiopeæ.

A 4-inch aperture, powers 80–120, reveals the duplicity of—

β Orionis.	α Lyræ.	δ Geminorum.
ϵ Hydræ.	ζ Ursæ Majoris.	σ Cassiopeæ.
ϵ Boötis.	γ Ceti.	ϵ Draconis.
ι Leonis.		

A 6-inch, powers 240–300—

ϵ Arietis.	20 Draconis.	ζ Herculis.
32 Orionis.	κ Geminorum.	ζ Boötis.
λ Ophiuchi.	ι Equulei.	

An 8-inch—

δ Cygni	Sirius.	μ^2 Herculis.
γ^2 Andromedæ.	19 Draconis.	μ^2 Boötis.

The "spurious disk," which a fixed star presents, as seen in the telescope, is an effect which results from the passage of the light through the circular object-glass, or its reflection from a circular mirror; and it is this

[1] Continued from p. 68.

appearance which necessitates the use of the largest apertures in the observation of close double stars, as the size of the star's disk varies, roughly speaking, in the inverse ratio of the aperture.

In our climate, which is not so bad as some would make it, a 6- to an 8-inch glass is doubtless the size which will be found the most constantly useful; larger apertures being frequently not only useless, but hurtful. Still, 4 or $3\frac{3}{4}$ inches are apertures by all means to be encouraged; and by object-glasses of these sizes, made, of course, by the *best* makers, views of the sun, moon, planets, and double stars, may be obtained, sufficiently striking to set many seriously to work as amateur observers, and with a prospect of securing good, useful results.

Observations should always be commenced with the lowest power, gradually increasing it until the limit of the aperture, or of the atmospheric condition at the time, is reached. The former may be taken as equal to the number of hundredths of inches which the diameter of the object-glass contains. Thus, a $3\frac{3}{4}$-inch object-glass, if really good, should bear a power of 375 on double stars where light is no object; the planets, the moon, &c., will be best observed with a much lower power.

FIG. 7.—Appearance of diffraction rings round a star when the object-glass is properly adjusted.

FIG. 8.—Appearance of same object when object-glass is out of adjustment.

Care should be taken that the object-glass is properly adjusted. And we may here repeat that this may be done by observing the image of a large star out of focus. If the light be not equally distributed over the image, or the diffraction rings are not circular, the screws of the cell should be carefully loosened, and that part of the cell towards which the rings are thrown very gently tapped with wood, to force it towards the eyepiece, or the same purpose may be effected by means of the set-screws always present on large telescopes, until perfectly equal illumination is arrived at. This, however, should only be done in extreme cases; it is here especially desirable that we should let well alone. In the case of mirrors, instructions for adjustment are generally given by the maker.

The convenient altitude at which Orion culminates in these latitudes renders it particularly eligible for observation; and during the first months of the year, our readers who would test their telescopes will do well not to lose the opportunity of trying the progressively difficult tests, both of illuminating and separating power, afforded by its various double and multiple systems, which are collected together in such a circumscribed region of the heavens that no extensive movement of their instruments—an important point in extreme cases—will be necessary.

Beginning with δ, the upper of the three stars which form the belt, the two components will be visible in almost any instrument which may be used for seeing them, being of the second and seventh magnitudes, and well separated. The companion to β, though of the same magnitude as that to δ, is much more difficult to observe, in consequence of its proximity to its bright primary, a first magnitude star. Quaint old Kitchener, in his work on telescopes, mentions that the companion to Rigel has been seen with an object-glass of $2\frac{3}{4}$-inch aperture; it should be seen, at all events, with a 3-inch. The bottom star in the belt is a capital test both of the dividing and space-penetrating power, as the two bright stars of the second and sixth magnitudes, of which the close double is composed, are exactly $2\frac{1}{2}''$ apart, while there is a companion to one of these components of the twelfth magnitude about $\frac{3}{4}''$ distant. The small star below, which the late Admiral Smyth, in his charming book, "The Celestial Cycle," mentions as a test for his object-glass of 5·9 inches in diameter, is now plainly to be seen in a $3\frac{3}{4}$. The colours of this pair have been variously stated.

That either our modern opticians contrive to admit more light by means of a superior polish imparted to the surfaces of the object-glass, or that the stars themselves are becoming brighter, is again evidenced by the point of light, preceding one of the brightest stars in the system composing σ. This little twinkler is now always to be seen in a $3\frac{3}{4}$-inch, while the same authority we have before quoted—Admiral Smyth—speaks of it as being of very difficult vision in his instrument of much larger dimensions. In this very beautiful compound system there are no less than seven principal stars; and there are several other faint ones in the field. The upper very faint companion of λ is a delicate test for a $3\frac{3}{4}$-inch, which aperture, however, will readily divide the closer double of the principal stars which are about $5''$ apart.

These objects, with the exception of ζ, have been given more to test the space-penetrating than the dividing power; the telescope's action on 52 Orionis will at once decide this latter quality. This star, just visible to the naked eye on a fine night, to the right of a line joining α and δ, is a very close double. The components of the sixth magnitude are separated by less than two seconds of arc, and the glass which shows a *good wide black division* between them, free from all stray light, the spurious disc being perfectly round, *and not too large*, is by no means to be despised.

Then, again, we have a capital test object in the great nebula to which reference has already been made.

The star to which we wish to call especial attention is situate (see Fig. 4) opposite the bottom of the "fauces," the name given to the indentation which gives rise to the appearance of the "fish's mouth." This object, which has been designated the "trapezium," from the figure formed by its principal components, consists, in fact, of six stars, the fifth and sixth (γ' and α') being excessively faint. Our previous remark, relative to the increased brightness of the stars, applies here with great force; for the fifth escaped the gaze of the elder Herschel, armed with his powerful instruments, and was not discovered till 1826, by Struve, who, in his turn, missed the sixth star, which, as well as the fifth, has been seen in modern achromatics of such small size as to make all comparison with the giant telescopes used by these astronomers ridiculous.

Sir John Herschel has rated γ' and α' of the twelfth and fourteenth magnitudes—the latter requires a high power to observe it, by reason of its proximity to α. Both these stars have been seen in an ordinary 5-foot achromatic, by Cooke, of $3\frac{3}{4}$-inches aperture, a fact speaking volumes for the perfection of surface and polish attained by our modern opticians.

Let us now try to form some idea of the perfection of the modern object-glass. We will take a telescope of eight inches aperture, and ten feet focal length. Suppose we observe a close double star, such as ξ Ursæ, then the images of these two stars will be brought to a focus side by side, as we have previously explained, and the distance by which they will be separated will be dependent on the focal length of the object-glass.

If we take a telescope ten feet long and look at two stars 1° apart, the angle will be 1°; and at ten feet off the distance between the two images will be something like $2\frac{1}{10}$ inches, and therefore, if the angle be a second, the lines will be the $\frac{1}{3600}$th part of that, or about $\frac{1}{1700}$th part

of an inch apart, so that in order to be able to see the double star ξ Ursæ, which is a 1″ star, by means of an eight-inch object-glass, all the surfaces, the 50 square inches of surface, of both sides of the crown, and both sides of the flint glass, must be so absolutely true and accurate, that after the light is seized by the object-glass, we must have those two stars absolutely perfectly distinct at the distance of the seventeen hundredth part of an inch, and in order to see stars $\frac{1}{2}''$ apart, their images must be distinct at one-half of this distance or at $\frac{1}{3400}$th part of an inch from each other.

J. NORMAN LOCKYER

(*To be continued.*)

BIOLOGICAL NOTES

CLASSIFICATION OF DECAPOD CRUSTACEANS.—In this well-defined group, the position of the anomurous forms (hermit-crabs, &c.) has often been the subject of doubt. The special adaptations of some genera for particular modes of life have caused them to be thrown together; and no doubt they agree in possessing neither the powerful abdomen of the lobsters, nor the very much aborted one of the crabs. Yet the anomurous forms include markedly contrasted groups. The family Hippidæ, with its lobster-like cephalothorax and firm abdomen, differs greatly in aspect from the hermit-crabs. *Hippa talpoida*, a small species found along the whole eastern coast of the United States, inhabits sandy beaches exposed to the waves, at a zone very near low-water mark. It has a smooth oval form, and short and stout thoracic legs (second, third, and fourth pairs), enabling it to burrow backwards in the sand with marvellous rapidity. In life the antennæ are peculiarly crossed, with the flagella curved round the mouth so that the setæ, with which they are densely covered, all project inwards, and the function of the antennæ appears to consist chiefly in the removal of all parasitic growths or foreign bodies from the anterior parts of the body. The appendages of the mouth are not adapted for prehension or mastication, and the alimentary canal is found loaded with fine sand. The thoracic appendages have neither external nor superior elements (exopodites, epipodites); while the office of protecting and cleaning the gills is discharged by the small limbs corresponding to the fifth pair of ambulatory legs in lobsters, which are curved upwards and hidden beneath the carapace. The development of this form has been recently carefully described by Mr. Sidney Smith, of Yale College, in the *Transactions* of the Connecticut Academy, vol. iii. p. 311. They pass through larval stages very analogous to the zoëa stages of crabs, only being destitute of a large dorsal spine; and they then assume a form like the brachyuran megalops, with large eyes, and powerful abdominal swimming legs. But in this condition they buried themselves in sand with great alacrity. Thus it is determined that the embryonic development of Hippa, as well as of Albunea, studied by Claus, agrees much more closely with that of crabs proper than with hermit crabs or lobsters; and this publication by Mr. Smith furnishes an important addition to the evidence favouring the view that the Anomura are a heterogeneous group made up of specialised families of Brachyura and Macrura.

THE AMERICAN BISON.—Mr. J. A. Allen's valuable "History of the American Bison," so sumptuously produced by the Geological Survey of Kentucky and the Harvard Museum of Zoology, has excited so much interest that to supply the demand for it Dr. Hayden has republished almost the whole of the text in the ninth annual report of his survey of the territories, and as a separate pamphlet of 150 pages, with considerable additions by the author. One of the most interesting of these consists in the publication of a letter from Mr. J. W. Cunningham, of Howard County, Nebraska, on the domestication of this species. It appears that the bison has been crossed with the ordinary milch cow, and that half- and quarter-breds are not uncommon, and the cows yield extremely rich milk. They prove to be both hardy and tame. The colour of the bison and the majority of the distinguishing characters disappear after repeated crossings. The lump of flesh covering the dorsal vertebræ also becomes diminished. The preservation of a pure domestic breed of the bison does not seem so easy. In some instances where buffaloes have been broken to the yoke they have proved strong and serviceable, but rather unmanageable at times. Unless the breed is maintained in some way artificially, the wild species will no doubt before very long become extinct.

PRODUCTS OF ASSIMILATION IN MUSACEÆ.—Herr Emil Godlewski has recently investigated whether in the case of Musaceæ the first assimilation-product is oil or starch, which latter is the first product in most plants. Sig. Briosi had recently maintained that oil was first produced. The question which had to be solved, therefore, was whether these plants, when decomposing carbonic acid under the influence of light, exhale a volume of oxygen greater than that of the carbonic acid decomposed. If oil is formed from the carbonic acid this must be the case. Measurements which Herr Godlewski made to this end with *Musa sapientium*, gave negative results; the oxygen exhaled was not of greater volume than the carbonic acid decomposed. Sig. Briosi had failed to discover starch in the grains of chlorophyll of the mesophyll-cells of the leaves; while Herr Godlewski was perfectly successful also in this direction, perceiving numerous granules of starch in leaves from young specimens of species of both *Musa* and *Strelitzia*, which had been collected in the evening after a hot day.

FERTILISATION IN THYME AND MARJORAM.—Under the title of "Das Variiren der Grösse gefärbten Blüthenhüllen, und seine Wirkung auf die Naturzüchtung der Blumen," Dr. Hermann Müller reprints from *Kosmos* a paper containing many of the facts which have appeared from time to time with his signature in these columns. The special point to which he calls attention is the occurrence in many species of Labiatæ—*Thymus serpyllum*, *Origanum vulgare*, &c.—of two distinct forms, one with larger hermaphrodite protandrous, the other with smaller female flowers. The second of these two forms can manifestly only be fertilised by the former, and will disappear where the conditions of life are unfavourable; while the propagation of the first form is in no way dependent on the other.

A FOSSIL FUNGUS.—One of the most interesting recent discoveries in palæophytology has recently been made by Mr. Worthington Smith, in the detection, in the coal-measures, of a fossil fungus nearly allied to that which produces the potato blight, and which he has named *Peronosporites antiquarius*. Fossil fungi were not previously altogether unknown. Some years ago Mr. Carruthers, the keeper of the botanical department at the British Museum, detected mycelial threads among the cells of a fossil fern (*Osmunda*) from the Lower Eocene strata of Herne Bay; and Mr. Darwin has stated that fungus threads in a fossil state in silicified wood were shown to him more than forty years ago by the late Mr. Robert Brown. Messrs. Hancock and Atthey have also described in the *Annals and Magazine of Natural History* (4th ser. vol. iv. 1869, p. 121, t. ix. x.), under the name of *Archagaricon*, what may be a fossil *Peronosporites* from the Cramlington black shale. The specimen examined by Mr. Worthington Smith (the fungoid nature of the organism having been first suggested by Mr. Carruthers), was seen within the vascular axis of a *Lepidodendron*,

and is thus described by that gentleman:—It consists of a mass of mycelia and zoosporangia (or oogonia). Beginning with the mycelium, a close examination shows that it is furnished with numerous joints or septa. If, therefore, any reliance is to be placed upon the modern distinguishing characters of the now living species of the genera *Peronospora* and *Pythium*, as furnished by a septate or non-septate mycelium, the fossil parasite belongs to the former, and not to the latter genus, nor to any of the Saprolegnieæ. The oogonia do not agree with those of *Cystopus*. Within many of the fossil oogonia the differentiation of the protoplasm into zoospores is clearly seen; but if any doubt could exist as to the exact nature of this differentiation, then other oogonia (or zoosporangia) on the same slide show the contained zoospores with a clearness not to be exceeded by any living specimens of the present time. It is a very remarkable fact that the oogonium precisely resembles, in size and other characters, average oogonia of the present day, especially those belonging to *Peronospora infestans*. The contained zoospores are, moreover, the same in form and dimensions with those of *P. infestans* when measured to the ten-thousandth of an inch. The organisms are, in fact, apparently identical; and the average number of zoospores in each oogonium is also the same, viz., seven or eight. The aërial condition of the fungus has not yet been observed. Mr. Worthington Smith suggests, in conclusion, that we probably have, in *Peronosporites antiquarius*, one of the primordial plants from which both the great families of fungi and algæ may possibly have descended; but should not this primordial plant have led a non-parasitic life?—for if parasitical, then this fact points to some pre-existing plant.

THE LAWS OF DIGITAL REDUCTION.—Hitherto there has been little explanation of the curious variation in the number and relative size of the digits in the vertebrata. Mr. John A. Ryder (*American Naturalist*, October) suggests that the number of toes is least where the mechanical strains are greatest, and impacts most frequent and severe. He quotes several cases in which the hinder digits are reduced more than those of the fore feet, and shows that in all of them the body in jumping or running pitches mainly upon the hind limbs. He looks upon the outer toes of man as in process of undergoing reduction, being now weaker and shorter than in any of the higher apes. The chrysochloris among moles is an instance of special reduction in the anterior extremity, and here the mechanical strains are most frequent and severe. Among fossorial animals the claws and toes are usually best developed on the fore limbs. The retention by certain groups, of digits in a very equal state of development in manus or pes, or both, is attributed to the equal distribution of strains on all.

THE BIRDS OF GUADALOUPE ISLAND.—This interesting fauna is dealt with by Mr. Robert Ridgway in the *Bulletin* of the Nuttall Ornithological Club for July. It is strange that only eight forms from this island, situated about 220 miles south-west of San Diego, are satisfactorily known, and their affinities are almost entirely with those of western North America. They are recognised by Mr. Ridgway, however, as specifically distinct, differing from their nearest mainland allies in the (1) increased size of the bill and feet, (2) shorter wings and tail, and (3) darker colours.

THE DISTRIBUTION OF FRESHWATER FISHES.—Dr. D. S. Jordan, the well-known American ichthyologist, has contributed to the *American Naturalist* for October some of his conclusions derived from long study of the fishes of rivers flowing in different directions, and under the most widely-varied physical conditions. He finds that in the case of rivers flowing into the ocean, the character of the fishes of the upper waters bears little or no relation to the place of discharge. The higher or the older the watershed between two rivers, the fewer species are common to both. Certain species (not including species of general distribution) occur on opposite sides of even the highest watersheds. When the watershed between two rivers is a swampy district, the same species are found in the head waters of both, though the faunas of the lower courses may be distinct. There is often a great difference between the forms in the upper and lower waters of a river, owing to differences in physical conditions. Some species are strictly confined to one river basin; others are widely distributed. Usually the more southern rivers have the most peculiar and varied faunas. Species of the widest distribution often have breaks in their range which cannot be accounted for by any known facts. The characteristically American forms of freshwater fishes are, generally speaking, absent or rare in the waters of New England and of the Pacific slope. The larger the river-basin, the greater its variety of forms. Seventy species have been taken in the little White River at Indianopolis, representing forty-eight genera, twice as many as occur in all the rivers of New England. Other things being equal, a river whose course lies in a region of undisturbed stratified rocks, or of glacial drift, contains most genera and species. Certain forms appear generally distributed in a definite range, either without regard to the direction in which the rivers flow, or even bounded by parallels of latitude. In any river-basin the most abundant species (of small fishes) are usually (1) those peculiar to it, or (2) those of widest distribution.

EARWIGS (FORFICULIDÆ).—Linnæus seems to have known but two species of earwigs (*Forficula auriculata* and *minor*). Both were European, and had *Elytra dimidiata et Alæ tectæ*, and were placed among the beetles (Coleoptera). There are now about 250 species known which are found all over the world, and grouped in about thirty genera, of which the genus Forficula is by far the richest in species and the widest in its geographical distribution. Happily, too, it still retains the two first-named species, and it has also most justly given its name to the family. Entomologists will be glad to know that Mr. Samuel Scudder has just published a series of critical and historical notes on this family, in which he gives descriptions of all the known genera, and an alphabetical list with full synonyms of all the described species; this most valuable list will make the study of these interesting insects an easy one. It is published in Parts 3 and 4 of vol xviii. of the *Proceedings* of the Boston Society of Natural History.

HUNGARIAN SPIDERS.—The first part of a work on Hungarian Spiders by Assistant-Director Otto Hermann, of Buda Pest, has just reached us. It forms a handsome royal-quarto volume, with three plates, and is printed in double columns, one in Magyar, and the other, fortunately for us, in German. This volume forms part of the *Transactions* of the Royal Hungarian Natural History Society, which is really to be congratulated on the appearance of this and the next work that we will mention. The present volume gives a sketch of the literature belonging to spiders, and forms one of the most generally interesting portions of the work, for it is most carefully elaborated, being divided into the bibliography of the older and the newer times. It further treats of the life-history of spiders in general, and of the geographical distribution of those species to be met with in Hungary. The next volume will contain the spiders met with in Hungary proper.

HUNGARIAN ROTIFERS OR WHEEL-ANIMALCULES.—A memoir on Hungarian Rotifers by Dr. Bartsch Samu is also published under the auspices of the Royal Hungarian Natural History Society, but it is written exclusively in Magyar, if we may except a short appendix containing brief descriptions of the new species determined by the author, and therefore unfortunately we can do no more than call attention to it.

OUR ASTRONOMICAL COLUMN

THE SATELLITES.—The following table presents at one view the mean distances of the satellites from their primaries, expressed in equatorial semi-diameters of the latter, and founded upon the most reliable data hitherto available :—

	The Earth.	Mars.	Jupiter.	Saturn.	Uranus.	Neptune.
I.	60·27	2·72	5·70	2·98	7·71	14·55
II.	—	6 81	9·07	3·83	10·75	—
III.	—	—	14·46	4·75	17·63	—
IV.	—	—	25·44	6 08	23·57	—
V.	—	—	—	8·47	—	—
VI.	—	—	—	19 67	—	—
VII.	—	—	—	24·80	—	—
VIII.	—	—	—	57·28	—	—

It will be seen that the outer satellite of Saturn, Iapetus, is the only one revolving round its primary at a distance similar to that of our moon, with respect to the semi-diameter of the central body. The exterior satellites of Jupiter and Uranus are similarly placed in this respect, and as regards the former planet the reader will remember a suggestion of Sir John Herschel's, that a distant satellite, by which was intended one situate more nearly, as our moon or the Saturnian satellite Iapetus, might be "worth a search." At the end of the last century it was thought that if satellites of Mars existed they might be "distant many degrees from the principal planet," upon which idea the late Prof. D'Arrest argued that a search after a satellite situate many degrees from Mars would be an almost endless task; and further, that a satellite at a maximum digression of seventy minutes of arc would have a sidereal period greater than the synodical revolution of the primary. The same astronomer endeavoured to ascertain, at the opposition of 1864, to what magnitude stars were visible in the vicinity of Mars with the Copenhagen refractor, which has an aperture of about eleven English inches. He considered that a satellite as bright as the twelfth magnitude could hardly have escaped him, and that objects of a fainter class were only visible in such an instrument at distances of eight or ten minutes, and in the case of Mars opportunities of viewing a satellite in such position would occur comparatively seldom. Perhaps the more prevalent idea respecting possible satellites of Mars, prior to their actual discovery, was that they would be "very small and close to the planet." (Hind, in "Solar System," p. 78.)

TYCHO BRAHE'S STAR OF 1572.—It is to be hoped that the vicinity of the famous star in Cassiopeia, with which we are accustomed to associate Tycho Brahe's name, may continue to receive frequent attention, and in particular that the small star, which at present is so near to the most accurate position we are able to obtain of the star of 1572, may be assiduously watched and its brightness determined from time to time by comparison with its neighbours, and not merely by estimation of magnitude. It was Bessel who, as he states in a letter to Olbers, in 1824, first engaged Argelander to work up the position of the Nova Cassiopeiæ, with all possible precision. Forty years later Argelander revised his calculations with improved positions for the reference-stars, and obtained a result differing in no material degree from the earlier one. The small star alluded to is so near to Argelander's last position (differing only fifty seconds of arc), as to be within its possible limits of error; it is No. 129 of the catalogue of stars in the vicinity which was presented to the Copenhagen Academy in January, 1864, and an eleventh magnitude on Bessel's scale. It will be most readily identified by means of the star of the ninth magnitude, No. 300 of Oeltzen's Catalogue from Argelander's northern zones, the position of which for 1878·0 is in R.A. 0h. 17m. 32s., N.P.D. 26° 22′·6; the suspicious object follows Argelander's star 29·6s., and is south of it 10′ 4″. The place of Nova for 1878·0 is in R.A. 0h. 18m. 2·1s., N.P.D. 26° 31′ 43″.

THE AUSTRIAN COMET-MEDAL.—We have received from the Imperial Academy of Sciences at Vienna, the conditions upon which that body has resolved to renew, until further notice, the prizes for the discovery of telescopic comets, and which appear to be similar to those originally issued in June, 1872. The awarding of a prize, which will consist, according to the wish of the receiver, in a gold medal or its money value of twenty Austrian ducats, is connected with the following conditions: (1) Prizes will be awarded only for the first *eight* successful discoveries in each calendar-year, for comets that at the time of their discovery were telescopic, *i.e.* invisible to the naked eye, that had not been previously seen by any other observer, and which could not have been predicted, and it is important to observe that in the case of independent discoveries priority is to be decided by the epoch of the first position. (2) The discovery must be communicated to the Academy of Sciences immediately, by telegraph, where practicable, otherwise by the earliest mail, the Academy undertaking to make it known without delay to several observatories. (3) This first notice must necessarily contain the position and motion of the comet as accurately as they are known, with the place and time of discovery, and is to be supplemented at the next opportunity by later observations. (4) If the discovery should not have been verified by other observers, the prize will only be adjudged "when the observations of the discoverer are sufficient for determining the orbit." (5) The prizes will be awarded in the general sitting of the Academy held at the end of May in each year, and in cases where the first intimation of the discovery arrives between March 1 and May 31, the award will be decided in the general May session in the following year. (6) Application must be made for the prize to the Imperial Academy within three months after the first notice of discovery shall have reached it, later applications being rejected. Finally, the astronomers of the observatory of the University of Vienna are appointed judges, whether the conditions in (1), (3), and (4) have been fulfilled.

GEOLOGICAL WORK OF THE U.S. SURVEY UNDER PROF. HAYDEN DURING THE SUMMER OF 1877

THE necessity of a careful examination of the various geological formations in the field, and a review by a practical palæontologist of the various districts that have from year to year been surveyed by the different geologists of this and other surveys, has been long felt. Such a work, indeed, was imperatively necessary, before a consistent and comprehensive classification of the formations could be established. This duty was assigned to Dr. C. A. White, the palæontologist of this survey, and he took the field at the beginning of the past season and continued his labours until its close. The special duty with which he was charged was to pursue such lines of travel as would enable him to make critical examination of the geological formations in succession as they are exposed to view on both sides of the Rocky Mountain chain, and also on both sides of the Uinta chain; to collect and study the fossils of these formations in such detail as to settle, as far as possible, the questions of the natural and proper vertical limits of the formations, their geographical range, their correlation with each other, and to define the palæontological characteristics of each.

He has pursued his researches with such success during the past season as to demonstrate the necessity of continuing this class of investigations by various lines of travel across what is generally known as the great Rocky Mountain region, especially those portions of it that have

been surveyed, as well as those in which surveys are in progress.

Among other important results, he has shown the identity of the lignitic series of strata east of the Rocky Mountains, in Colorado, with the Fort Union group of the Upper Missouri River, and also its identity with the great Laramic group of the Green River Basin and other portions of the region west of the Rocky Mountains. He also finds the planes of demarcation between any of the mesozoic and cenozoic groups, from the Dakota to the Bridger, inclusive, to be either very obscure or indefinable; showing that whatever catastrophal or secular changes took place elsewhere during all that time, sedimentation was probably continuous in what is now that part of the continent, from the earliest to the latest of the epochs just named.

The general course of travel pursued by Dr. White during the season was as follows, not including the numerous detours, meanderings, and side trips, which the work necessitated. Outfitting at Cheyenne, he journeyed southward, traversing in various directions a portion of the great plain which lies immediately adjacent to the eastern base of the Rocky Mountains in Colorado. The most easterly point thus reached was some sixty miles east of the base of the mountains, and the most southerly point, about twenty-five miles south of Denver. Returning to Denver to renew his outfit, he crossed the Rocky Mountains by way of Boulder Pass, through Middle Park. After making certain comparative examinations of the mesozoic and cenozoic formations in Middle Park, he proceeded westward to the head-waters of Yampa River, following that stream down to the western foothills of the Park Range of mountains. Here, resuming his comparative examination of the mesozoic and cenozoic strata, he passed down the Valley of the Yampa as far as Yampa Mountain, one of those peculiar and remarkable upthrusts of palæozoic rocks through mesozoic strata. In all this area, as well as that between the Yampa and White Rivers, the Laramic group reaches a very great and characteristic development; and it received careful investigation, yielding some of the most important results of the season's work. Crossing the ground between the two rivers named, to White River Indian Agency; thence down White River Valley about 100 miles, thence to Green River, crossing it at the southern base of the Uinta Mountains, making many detours on the way, he reviewed the geology of the region which he had surveyed during the previous season. This review brought out not only the important palæontological facts before referred to but it also added materially to the elucidation of the geological structure of the region which lies between the eastern end of the Uinta mountain range on the west, and the Park range on the east.

Beyond Green River he pursued his travels westward, studying the mesozoic and cenozoic strata that flank the Uinta range upon its south side, and making comparisons of both their lithological and palæontological characteristics.

In this way he traversed the whole length of the Uinta range, crossing at its junction with the Wasatch range over into the valley of Great Salt Lake. Re-crossing the Wasatch to the north side of the Uinta range, he continued his examinations of the cretaceous and tertiary strata into and entirely across the Great Green River basin, leaving the field at the close of the season at Rawlin's station on the Union Pacific Railroad.

A general statement of the results of the season's work has been given in a previous paragraph, but the following additional summary will make the statement somewhat clearer, being made after the route of the season's travel has been indicated. The formations of later mesozoic and earlier cenozoic ages, especially those to which Dr. White, in former publications, has applied the provisional designation of "post-cretaceous," have received particular attention. The extensive explorations of Dr. Hayden in former years, and the palæontological investigations of the late Mr. Meek, pointed strongly to the equivalency of the Fort Union beds of the Upper Missouri River with the lignitic formation as it exists along the base of the Rocky Mountains in Colorado; and also to the equivalency of the latter, with the Bitter Creek series west of the Rocky Mountains. The investigations of the year have fully confirmed these views by the discovery not merely of one or two doubtful species common to the strata of each of these regions, but by an identical molluscan fauna ranging through the whole series, in each of the regions named. This shows that the strata just referred to all belong to one well marked period of geological time; to the strata of which Mr. King has applied the name of "Laramic group" (Point of Rocks, Group of Powell). His investigations also show that the strata which in former reports by himself and Prof. Powell, have been referred to the base of the Wasatch group, also belong to the Laramic group, and not to the Wasatch. He has reached this later conclusion not merely because there is a similarity of type in the fossils obtained from the various strata of the Laramic group with those that were before in question; but by the specific identity of many fossils that range from the base of the Laramic group up, into, and through the strata that were formerly referred to the base of the Wasatch. Furthermore some of these species are found in the Laramic strata on both sides of the Rocky Mountains. Thus the vertical range of some of these species is no less than three thousand feet and their present known geographical range more than a thousand miles.

Besides the recognition of the unity of the widely distributed members of the formation of this great geological period, bounded by those of undoubted cretaceous age below, and those of equally undoubted tertiary age above; his further observations have left comparatively little doubt that the "lake beds" of Dr. Hayden, as seen in Middle Park, the "Brown's Park group" of Prof. Powell, and the "Uinta group" of Mr. King, all belong to one and the same epoch, later than, and distinctly separate from, the Bridger groups. In that portion of the region which lies adjacent to the southern base of the Uinta mountain range, and which is traversed by Lake Fork and the Du Chesne River, not only the Uinta group, but both the Green River and Bridger groups also, are well developed, each possessing all its peculiar and usual characteristics, as seen at the typical localities in the great Green River Basin, north of the Uinta Mountains. This, added to the known existence of Bridger strata in White River Valley, and the extensive area occupied by the Green River group between White and Grand Rivers, has added very largely to our knowledge of the southward extension of those formations.

In all the comparative examinations of the formations or groups of strata that have just been indicated he has paid special attention to their boundaries or planes of demarcation, crossing and recrossing them wherever opportunity offered, noting carefully every change of both lithological and palæontological characters. While he has been able to recognise with satisfactory clearness the three principal groups of cretaceous strata, namely, the Dakota, Colorado, and Fox Hills, on both sides of the Rocky and Uinta Mountains respectively, they evidently constitute an unbroken series so far as their origin by continuous sedimentation is concerned. While each of the groups possesses its own peculiar palæontological characteristics, it is also true that certain species pass beyond the recognised boundaries of each within the series.

The stratigraphical plane of demarcation between the Fox Hills, the uppermost of the undoubted cretaceous groups, and the Laramic group, the so-called post-cretaceous, is equally obscure; but the two groups are palæon-

tologically very distinct, inasmuch as the former is of marine origin, while the latter, so far as is now known, contains only brackish-water and fresh-water invertebrate forms. He reports a similar obscurity or absence of a stratigraphical plane of demarcation between the Laramic and Wasatch groups, although it is there that the final change from brackish to entirely fresh waters took place over that great region. Furthermore, he finds that while the three principal groups of the fresh-water tertiary series, west of the Rocky Mountains, namely, the Wasatch, Green River, and Bridger groups, have each peculiar characteristics, and are recognisable with satisfactory distinctness as general divisions, they really constitute a continuous series of strata, not separated by sharply-defined planes of demarcation, either stratigraphical or palæontological.

During the progress of the field work, as above indicated, large and very valuable collections of fossils have been made, all of which will constitute standards of reference in the future progress of the work, and quite a large number of the species are new to science. These are now being investigated, and will be published in the usual palæontological reports of the survey.

NOTES

AT the moment of going to press we have received the report of the *Inflexible* Committee. The impression a first glance over it gives is that the *Inflexible* is a passable ship, but that the Committee strongly urge the Admiralty not to proceed with any more like it, which practically puts an end, we presume, to the *Ajax* and *Agamemnon*, in their present form, as well as to the fourth ship which the Admiralty proposed to build. It is proper, however, to state that a closer perusal of the report shows the *Inflexible* herself to be open to the gravest objections in several respects, and that the Committee recommend considerable modifications in her. In our next number we shall fully review the report.

WE have received several letters from India, showing that great interest is being taken in that country with reference to the best methods of determining the amount and variation of solar radiation. We may state that both Prof. Stewart and Mr. Lockyer have recently devised instruments to secure these data. The latter proposes to utilise Capt. Abney's method of obtaining photographs of the red end of the spectrum, so that variations in thermal and chemical intensity may both be recorded automatically.

SIR WILLIAM THOMSON has been elected a Foreign Associate of the Paris Academy of Sciences, to fill the place vacated by the death of von Baer.

PROF. SIR WYVILLE THOMSON has been created a Knight of the Royal Order of the Polar Star by the King of Sweden.

M. TEMPEL is to continue henceforth the publication of Donati's *Bollettino* of the Arcetri Observatory, of which only one number had been issued when Mr. Donati died.

GEN. NANSOUTY, Director of the Observatory situated on the top of the Pic du Midi has been nominated "Officier de l'Université" by M. Faye, the new Minister of Public Instruction. The General, as our readers know, spends his winters on that precipitous mountain for meteorological observations. We are glad to register such an acknowledgment of his devotion to science.

DR. BURDON-SANDERSON gives notice that the first of his annual course of lectures on comparative pathology will be delivered at the University of London, Burlington Gardens, on Saturday, December 15, at half-past five o'clock. The subject of the lecture will be, "The Infective Processes of Disease." The succeeding lectures will be on the Monday, Wednesday, and Friday of the following week, at the same hour, for which days "The Nature and Causes of Septic Infection," "The Germ Theory," and "The Theory of Contagium Vivum," are among the topics to be discussed.

THE German postal department has issued a complete series of regulations for the use of the telephone in the various offices where it has been established. In § 15 we notice the rule that the speaker shall pronounce each syllable slowly and separately, and make a pause at the end of every six words to give time for the receipt of the message. The receiver repeats the whole message at the end at an ordinary rate of delivery. Proper names and foreign messages are spelled. The Postmaster-General, Dr. Stephan, who wages an unmerciful war in his department against all foreign words where a German equivalent is possible, has christened the new invention as the *Fernsprecher* (far-speaker), and excluded entirely the Greek *telephone* from his regulations.

IN consequence of the large numbers who were unable to obtain admission to the recent lecture at the Society of Arts on the "Telephone," Prof. Bell, at the special request of the Council of the Society, has consented to repeat his lecture on Wednesday, the 19th inst. As there is certain to be a large attendance, it is suggested that those members who heard the first lecture, should refrain from exercising their privilege of being present on the second occasion.

PROF. KEKULÉ, of Bonn, the originator of the present benzene theory has been nominated for president of the German Chemical Society for the coming year. The policy which the society adopted at its last annual election of choosing its chief officer from among the leading German chemists at a distance from the headquarters of the society, seems to meet general favour, and Prof. Wöhler, the Nestor of organic chemistry, will certainly be ably succeeded by Prof. Kekulé, whose classical researches and theoretical deductions form the basis of the present atomistic theory. The German Chemical Society would do well to copy one of the customs of its sister society in London, viz., to require an inaugural address from its newly-elected presidents. We notice that the library of the society will be enriched by the bequest of the extensive chemical library of the late Prof. Oppenheim, an accession which will double the present number of volumes.

DR. VOHL, of Cologne, has adopted an ingenious method of determining the impurities in the Rhine, which consists in analysing the boiler incrustations of the river steamers, as well as the concentrated residues remaining in the boilers after passing over a certain distance. By this means he has detected the presence of a large amount of arsenious acid in the river water—resulting chiefly from the aniline and dyeing establishments—as well as other poisonous substances. An unusually high percentage of phosphoric acid showed that the sea was daily absorbing vast quantities of the most valuable fertilising material from the soil of Germany.

THE Scientific Congress of France will meet at Nice from January 10 to 20, 1878. The locality is likely to attract many visitors at such a cold period of the year.

ANOTHER sitting of the enlarged Council of the Observatory of Paris was held on December 9. The councillors passed a resolution for an increase of the salary of the astronomers and auxiliary astronomers, the maximum pay of the former to be 10,000 francs instead of 8,000, and of the second 7,000 instead of 6,000. They propose to the Government to place the appointment of the director of the establishment partly in the hands of the Academy of Sciences and partly in the hands of the Council, the Minister to have only the privilege to choose

between both presentations. It should be remembered that under the former organisation the appointment of the director was in the hands of the Bureau des Longitudes, which had to designate yearly their member to preside over the observations. Arago and his illustrious predecessor held their office by a yearly tenure, renewed up to the time of their death. It was only Leverrier who was appointed by the Government; under the old monarchy the director was appointed by the king, A proposal was made for suppressing the general assembly of astronomers, which was established by M. Leverrier, and is to be held at Easter at the Ministry of Public Instruction. The proposal was out-voted. At the next sitting the long-discussed organisation of meteorology will be introduced; the existing order of things continues to enjoy the support of the majority.

A TELEGRAM from Alexandria states that Capt. Burton has started from Suez for Moilah on a second expedition to Midia, accompanied by many Europeans and a large number of native workmen and troops. A dépôt will be formed at Moilah in the Gulf of Akaba, and the expedition will extend to the second range of mountains hitherto unexplored. Capt. Burton expects to discover a rich mineral country between the two ranges of mountains. He will be absent four months.

THE Montsouris Park, in the centre of which the Montsouris Observatory has been erected, is almost ready for public use. Admittance to the observatory grounds will be procured on application to the secretary, for the purpose of inspecting the instruments and the working of the meteorological observations.

IN Würtemberg a remarkable property of ripe grapes has been recently discovered, which the agricultural authorities have now published, so that all proprietors of vineyards may derive benefit from being acquainted with it. It appears that if ripe grapes, which have become frost-bitten, are kept for a little time in some dry place, they entirely lose the bad effects caused by the frost.

IN the *Bulletin* of the French Geographical Society for October are some interesting notes from the Abbé Desgodins, on Tibet. The Abbé gives some information concerning the Brahmapootra, which he obtained from an old lama, whom he believes to be thoroughly trustworthy. This lama has travelled much, and visited nearly the whole of Tibet. His information goes to prove the identity of the Yar-tsiou-tsang-po with the Brahmapootra. He has followed the great river from its source in or near the lakes of Tso-ma-pang in the west of the province of Ngaré, the most western of Tibet, and in making his pilgrimages he has reached the frontiers of the savage tribe of the Lhopa. The lama states that some days to the east of Lassa the river turns towards the south, making a great bend, and traverses the well-peopled and rich district of Hia-zul, just to the north of the Lhopa. Passing through the latter district, it flows among steep and rugged rocks, and after a certain distance forms a great waterfall. This fall of the Yar-kiou-tsang-po, M. Desgodins has no doubt, is identical with the fall of Brama-Khoond, well known to the Assamese. The lama affirmed that the river did not reach so far as the Nahengs (Mishmis), but that it disappeared more to the west, among the Lhopa. The lama gave the Abbé, besides, much information concerning the people and the districts through which the river passes. The *Bulletin* contains, besides, a learned article by M. E. Cortambert on some of the geographical monuments of the middle ages in the National Library, dwelling at considerable length on the well-known Mappemonde of Beatus, a beautiful facsimile of which is given.

THE Italian Geographical Society has received letters from Aden dated November 25, which confirm the arrival of the second Italian (Martini-Cecchi) expedition at Farré. There is no news at all about the supposed defeat of King Menelik of Schoa in connection with which the death of the Marquis Antinori was reported. It seems, however, that King Kassa gained a victory over a Prince Menelik (son of the late King Theodor), who had rebelled in Kassa's camp. The resemblance of names explains the misunderstanding and deprives of any foundation the news about the misfortune said to have happened to the members of the first Italian (Antinori) expedition.

THE French Acclimatisation Society held its anniversary meeting on December 7 last, under the presidency of M. Quatrefages. The Society lost recently M. Drouyn de Lhuys, one of its founders, a former minister of the empire, well known in France as well as abroad.

IN this month's *Geographical Magazine* Mr. C. R. Markham continues his valuable papers on Irrigation in Southern India, and Mr. G. J. Morrison concludes his interesting description of the island of Formosa. In criticising Mr. Trelawney Saunders on the question of "Water-partings *versus* Ranges," Mr. R. B. Shaw appears to have misconceived Mr. Saunders' statements; Mr. Saunders' knowledge is too extensive and accurate to allow him to maintain the identity of the two terms. Sir George Nares contributes an important article on the Greenland Foehn, recently noticed in NATURE.

WE are glad to learn from the *Geographical Magazine* that the Dutch are making active preparations to resume Arctic exploration, to which they have been able to do little since the days of the brave but unfortunate Barentz, and nothing at all, we believe, during the last century. A new schooner is to be built to be sent out in May next year to make a summer cruise in the Spitzbergen and Barentz Seas.

THE hygrometer devised by M. Alluard, described in NATURE, vol. xvii. p. 14, was constructed by M. L. Golaz, of 24, Rue des Fosses St. Jacques, Paris, who contributed some beautifully-constructed apparatus to the recent Loan Collection at South Kensington.

DR. SCHLIEMANN, assisted by Mr. Streatfield, of the Science and Art Department, is busy arranging his Trojan treasures in the South Kensington Museum. Although a large space has been assigned for their reception, it will take considerable ingenuity to get all the interesting articles satisfactorily arranged.

M. DAHLANDER communicates to the Swedish Academy of Sciences the results of his observations on the comparative rapidity with which heated solid bodies are cooled by immersion in various liquids. If the cooling power of water be taken as unity, that of alcohol is 0·58, of mercury 2·07, of a concentrated solution of salt, 1·05, and of a concentrated solution of sulphate of copper, 1·03. The rapidity of the cooling increases with the increased temperature of the liquid.

THE twenty-first annual report of the committee of the Free Public Libraries and Museums of Sheffield, speaks favourably of the progress of these institutions. We are glad to see that the number of scientific works sought for both in the lending and consulting libraries bears a fair proportion to the number in other departments.

ACCORDING to the published reports of the Koenigsberg Board of Trade, the total production of amber in the province of Prussia amounted to 135 tons during the year 1876, of which eighty-five tons were furnished by one mine alone, viz., the mine of Palmnicken. This production considerably exceeded that of the previous year. The amber was exported principally to Austria, France, Russia, America, China, and Japan, while the export to India, Persia, and Australia does not pay the producers, and is therefore extremely limited. The number of workmen in the province who are employed in the production of amber amounts to nearly 1,400.

NOW that the struggle in the East seems to be reaching its crisis, the fine, large, clear map of the Bosphorus and the

Dardanelles, just published by Mr. Stanford, will be extremely useful to those who desire to follow, with intelligence, further movements, military or diplomatic.

MR. HEIGHWAY's handbook of "Practical Portrait Photography" (London, Piper and Carter), has reached a second edition, into which some improvements have been introduced.

THE death is announced of Mr. John G. Anthony, who for some years has had charge of the conchological department of the Cambridge Museum. He was one of the party accompanying Prof. Agassiz in his celebrated scientific expedition to Brazil.

THE number of French communes receiving the daily warnings of the international service for agricultural purposes is increasing daily; the death of Leverrier has not destroyed that extraordinary movement. It is said the twenty districts into which Paris has been divided, will very soon have the daily warnings posted at each of their respective mairies. According to a saying attributed to M. Dumas, 'The existing meteorology had had it own plebiscitum."

PROF. J. PLATEAU, of Ghent, has reprinted (from the *Proceedings* of the French Association for the Advancement of Science, 1876) a paper on the question, Is the instinct of insects deceived by artificial flowers? As far as the series of experiments performed by him—rather few in number, but apparently carried out with great care—can be relied on, although insects may be attracted from a distance by the bright colours of artificial flowers, they are never tempted by the resemblance to alight on them in the hope of obtaining food from them. He concludes, therefore, that insects make use of some other organ than that of sight in the selection of the flowers which they visit.

PROF. E. MORREN, of Liége, has issued the fifth annual edition of his "Correspondance Botanique," which contains a complete list of botanical gardens and museums, and the occupants of botanical chairs throughout the world. Even adding a number of "unattached" English botanists whose names are given, it is instructive to compare the number engaged in botanical research in Great Britain with that in France or Germany, or even in Italy or Russia.

A BRIEF report of the third annual conference of the Cryptogamic Society of Scotland, held at Dunkeld in October last, has been published, from which it is evident the meeting was successful. The first fasciculus of the "Fungi Scotici Exsiccati" will be published in January. Dr. Buchanan White, Perth, will receive orders; no subscribers names can be received after the 20th inst.

WE have received Part 2, 1876–77, of the *Transactions* of the Cumberland Association of Literature and Science, which contains a number of scientific papers of considerable value. Among these are six original papers communicated to the societies connected with the Association during the session, and selected by the Council for publication. Two of these will interest the scientific reader: "Jonathan Otley, the Geologist and Guide," by Mr. Clifton Ward, and "Notes on the Migratory Birds of the English Lake District," by Mr. John Birkett.

THE seventh annual report of the Leeds Naturalists' Club and Scientific Association speaks in the most favourable terms of the continued progress of that society.

IN the *Monatsbericht* of the Prussian Academy of Sciences for July, which has just appeared, we notice papers by H. Anvers, "On the Results of the Transit Observations with Bradley's Quadrant;" by H. Websky, "On the Horn Mercury from el Doctor in Mexico;" by Prof. du Bois Reymond, Prof. Peters, and Prof. Möbius, "On the Amphibious Collections made by Dr. Sachs during his late trip to Venezuela."

IN the *Atti della Reale Academia dei Lincei* at Rome, some interesting investigations are described, which were made by Messrs. A. and G. De Negri at the Chemical Laboratory of the Genoa University, on the purple dies of antiquity. The authors have thoroughly investigated the subject; after an elaborate account and an enumeration of the various historical data with regard to the molluscs from which the ancients obtained their purple colours, they enter into a discussion of the chemical and optical properties of these substances, the methods of dyeing with them, the adulterations found in them, and various other details concerning them. We must refer our readers to the original treatise for further particulars, as our space will not permit us to enter into them. The paper is accompanied by a number of plates, giving the spectra of the colours obtained from species of the genera *Applysia* and *Murex*. The same volume of this publication contains an excellent account, by Signor C. Bagnis, of the fungi species *Puccinia*, illustrated by no less than eleven well-drawn plates.

THE Piscicultural Institution of Schwerin has recently made some important experiments with a view to ascertain whether the artificial culture of river Crawfish (*Astacus fluviatilis*) is possible on a large scale. The experiments were entirely successful. In the spring of last year some 700 crawfish with ova were placed into two circular ponds of only six feet diameter, and for each animal a separate hole had been constructed. At the end of November the ponds were drained in order to separate the young crawfish from the old ones. It appeared that of the latter only three or four were crawling about at the bottom of the pond while all the others had occupied their respective dwellings. The young were of the size of a bee and extremely lively; they were taken out of the ponds and already on the following day could be fed artificially with carrots and meat. Many a land or garden proprietor could thus make crawfish-culture a lucrative pastime at very little cost, particularly since the consumption of these crustaceans increases largely every year.

THE last number of the *Zeitschrift für Ethnologie* contains a most valuable and elaborate review of the entire ethnological and anthropological literature of 1876, prepared by Prof. W. Koner. Over 1,000 pamphlets, periodicals, and books are referred to, and as few subjects are handled in a greater variety of languages than those in question, the labour of compiling such a report can easily be imagined.

WE have received the third (final) part of Herr Axel Blytt's elaborate Flora of Norway, which is published by order of the Royal Norwegian Society of Sciences, and bears the title, "Norges Flora; eller Beskrivelser af de i Norge vildtvoxende Karplanter" ("Flora of Norway; or, Description of the Wild Plants in Norway.")

A CORRESPONDENT asks where he can find a description of the mode of drying sections of trees. He has a transverse section, three inches thick, of an elm tree, and he wants to dry it so that it may be cut in veneer when ready.

THE additions to the Zoological Society's Gardens during the past week include a Diana Monkey (*Cercopithecus diana*) from West Africa, presented by Mr. Walter Mayhew; a Rhesus Monkey (*Macacus erythræus*) from India, presented by Mr. R. S. Cox; two White Storks (*Ciconia alba*), a Common Heron (*Ardea cinerea*), a Greater Black-backed Gull (*Larus marinus*), European, presented by Mr. C. Clifton; a Hobby (*Hypotriorchis subbuteo*), captured at sea, presented by Mr. W. Renney; two Lesser Sulphur-crested Cockatoos (*Cacatua sulphurea*) from the Moluccas, presented by Mrs. Roberts; seven Gelada Baboons (*Cynocephalus gelada*) from Abyssinia, four Barbary Turtle-Doves (*Turtur risorius*) from North Africa, deposited; two Schlegel's Doves (*Chalcopelia puella*) from West Africa, purchased.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—The examination for open scholarships at Christ's College will be held on Tuesday, April 9. Candidates in natural science will be required to show a satisfactory knowledge of elementary chemistry, both theoretical and practical. Candidates are required to send in their names to one of the tutors of the college before April 2. Further information can be obtained on written application to Mr. John Peile, or the Rev. J. W. Cartmell, Christ's College.

SCIENCE AND ART DEPARTMENT.—The list has been published by the Science and Art Department of the successful candidates in honours at the examination of science schools and classes, May, 1877. We give the names of the two first in the first class of each subject:—Subject I. Practical, Plane, and Solid Geometry—John R. Smith, age 32, clerk; William J. Last, age 19, engineer. Subject II. Machine Construction and Drawing—Robert A. Sloan, age 22, engineer; William Sisson, age 24, engineer. Subject III. Building Construction—Crichton Walker, age 34, carpenter; Robert Henry, age 22, draughtsman. Subject IV. Naval Architecture—Frederick B. Ollis, age 18, shipwright's apprentice; George A. Agnew, age 23, shipwright's apprentice. Subject V. Pure Mathematics, Stages One, Two, and Three—George J. T. Harker, age 18, student; Arthur W. Ward, age 18, cotton broker. Stages Four and Five—Frederick W. Watkin, age 18, pupil; Arthur E. Holme, age 18, engineer. Subject VI. Theoretical Mechanics—William Sisson, age 24, engineer; William Martin, age 22, engineer. Subject VII. Applied Mechanics—Frank W. Dick, age 23, engineer; Fred Ogden, age 18, engineer, William J. Last, age 19, engineer, Robert A. Sloan, age 22, engineer, Robert Greenhalgh, age 22, engineer—eq. Subject VIII. Acoustics, Light, and Heat—Frederick E. Boughton, age 20, draughtsman; James Greer, age 31, Inland Revenue officer. Subject IX. Magnetism and Electricity—Robert A. Sloan, age 22, engineer, Frederick E. Boughton, age 20, draughtsman—eq.; William J. Last, 19, engineer. Subject X. Inorganic Chemistry—Charles N. Luxmore, age 19, chemist's assistant; Sidney E. Meates, age 17, chemical student. Subject XI. Organic Chemistry—Charles M. Luxmore, age 19, chemist's assistant. Subject XX. Navigation—George Goodwin, age 14, engineer's apprentice; William Allingham, age 26, clerk. Subject XXII. Steam—Robert A. Sloan, age 22, engineer, William Sisson, age 24, engineer—eq.; Alfred Cliff, age 22, engineer, Jerdan Nichols, age 21, engineer—eq. Subject XXIII. Physical Geography—John S. Harper, age 19, student in training; John Sharkey, age 29, schoolmaster. Subject XXIII. Physiography—George A. Freeman, age 26, schoolmaster; John A. Lakin, age 21, teacher, Fredk. J. Richardson, age 16, teacher—eq. Subject XXIV. Principles of Agriculture—Edward S. Chesney, age 21, student; William E. Akroyd, age 20, student. There have been no first class successes in Geology, Mineralogy, Animal Physiology, Elementary Botany, General Biology, Principles of Mining, Metallurgy, and Nautical Astronomy.

BRISTOL.—The annual meeting of the governors of University College was held on Friday last, when a report, on the whole satisfactory, was presented. The number of students has somewhat decreased, as indeed might have been expected, but there seems every reason to believe that the college has taken its place as an important centre of education in the west of England. The funds of the college, though considerable in amount, are yet not sufficient to keep it going with complete efficiency, and we hope the appeal made by the governors will be satisfactorily responded to. It is proposed to make the college a local centre for the examinations of the University of London.

PESTH.—A commission has recently been appointed by the various faculties, to make fitting preparations for the celebration of the first centennial of the opening of the university, which was performed by Maria Theresa in 1780. The university is wealthy, possessing property to the amount of 6,000,000 florins, and a library of 120,000 volumes, and forms the real centre of Hungarian intellectual life. The other two Hungarian universities, Klausenberg and Agram, were founded respectively in 1872 and 1874. At present the instructors number 150 and the students 2,630.

WÜRZBURG.—Prof. Sachs has declined the call to the vacant chair of botany at the Berlin University, and the authorities are still seeking a successor for the late Prof. Braun.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, December 6.—Points of resemblance between the suprarenal bodies of the horse and dog, and certain occasional structures in the ovary, by Charles Creighton, M.B., Demonstrator of Anatomy, Cambridge University. Communicated by Prof. Humphry, F.R.S.

On the tides at Malta, by Sir G. B. Airy, K.C.B., Astronomer-Royal.

Observations on hermetically-sealed flasks opened on the Alps, in a letter to Prof. Huxley, Sec. R S., by Prof. Tyndall, LL.D., F.R.S. Though the author believes the question of "Spontaneous Generation" is practically set at rest for the scientific world, he has been making some experiments on *Bacteria*.

He took with him this year to the Alps sixty hermetically-sealed flasks, containing infusions of beef, mutton, turnip, and cucumber, which had been boiled for five minutes and sealed during ebullition. These were kept for six weeks, when some were opened in a hay-loft and others on the edge of a precipice.

The two groups of flasks were then placed in the author's kitchen, where the temperature varied from about 65° to 90° Fahr. The result was that twenty-one of twenty-three flasks opened in the hay-loft were filled with organism; two of them remained clear. All the flasks opened on the edge of the precipice remained as clear as distilled water. Not one of them gave way.

Chemical Society, December 6.—The President in the chair.—The following papers were read:—On gallium, by W. Odling. The properties of the metal, its chloride, and sulphate, and their reactions, were given and specimens exhibited.—On nitrification, a report of experiments conducted in the Rothamsted Laboratory, by R. Warrington. Schloesing and Müntz have shown that nitrification is due to the action of an organised ferment whose action is suspended by chloroform. The author has completely confirmed the above statement, and has proved that carbolic acid and bisulphide of carbon also stop the action of the ferment, and moreover that darkness is essential for the process. The author has succeeded in converting a dilute solution of ammonium chloride into a nitrate by seeding it with some earth from a fairy ring and keeping it in the dark for three months.—On potable waters, by E. J. Mills, D.Sc. The author investigates the minute errors incidental to the process of Frankland and Armstrong with great care, suggests a new form of evaporator, and arrives at three natural constants or ratios of organic carbon to organic nitrogen in potable waters.—On some derivatives of allylacetone, by J. R. Crow. By the action of sodium, a secondary alcohol homologous with allyl alcohol was prepared; its acetate and dibromide were also investigated.—On a fourth method for estimating bismuth volumetrically, by M. M. P. Muir. The bismuth is precipitated as oxalate, the latter on boiling is converted into a basic oxalate, the precipitate is well washed, dissolved in hydrochloric acid, and the solution titrated with permanganate.—On the gas of the Grotto del Cane, by T. G. Young. This gas contains 61–71 per cent. of carbonic acid, the residual air having the composition oxygen, 20·25, nitrogen, 79·75.—Note on tetrabromide of tin, by T. Carnelly, D.Sc., and L. T. O'Shea. This body was obtained as a colourless liquid, solidifying to a mass of colourless crystals,—melts at 30° C., boils at 201°.

Meteorological Society, November 21.—Mr. H. S. Eaton, M.A., president, in the chair.—The following gentlemen were elected Fellows of the Society, viz.:—E. D. Archibald, B.A., R. W. P. Birch, Capt. W. F. Caborne, H. Clarke, L.R.C.P., Cohen de Lissa, F.S.S., R. Gordon, J. Hunter, jun., J. J. Lake, Rev. E. A. D. O'Gara, O.S.B., R. Pennington, LL.B., E. E. Prichard, and Rev. S. J. W. Sanders.—The following papers were read:—On the general character and principal sources of variation in the weather at any part of a cyclone or anti-cyclone, by the Hon. Ralph Abercromby, F.M.S. In a cyclone the broadest feature of the weather, as seen on a synoptic chart, is an area of rain about the centre surrounded by a ring of cloud, beyond which the sky is clear. The precise form and position of these areas vary with the type of pressure distribution, with the intensity of the cyclone, and with the rate of its progress; they are also influenced by local, diurnal, and seasonal variations, the general sphere of each of which is indicated. By recording the appearance to a single observer of any part of a

cyclone as it passes over him, it is discovered that the area of rain and cloud-ring may be divided into two portions—the front and the rear—differing in physical appearance and general character of the weather by a line drawn through the centre, in front of which the barometer is falling and in rear of which it is rising. Details are given and it is shown that this character remains constant whatever changes the variations above mentioned may effect. In anti-cyclones synoptic charts show great irregularity in the positions of cloud, &c., owing to local, diurnal, and other variations, but to a single observer, who considers the surroundings and physical appearance, a certain general character can be discovered in every part. A marked contrast is shown to exist between the diurnal variation of the weather in a cyclone or anti-cyclone, and a probable connection is pointed out between the diurnal variation of the weather and the diurnal variation of the barometer.—On a remarkable barometric oscillation on January 30, 1876, by Robert H. Scott, F.R.S. At 8 A.M. a remarkable dip appeared in the barometric curve for Armagh. The total amount of reduction of pressure was ·097 inch in 25 minutes and for a portion of the time, from 8·5 to 8·10, the fall was ·058 inch, being at the rate of ·697 inch per hour. On looking to the other barograms, it was found that while a very similar oscillation of slightly greater amplitude, ·102 inch, appeared at Aberdeen at noon, almost exactly four hours subsequent to the occurrence at Armagh, hardly a trace of disturbance could be detected in the barogram for Glasgow, and yet the last-named observatory lies almost on the direct line between Armagh and Aberdeen. The barograms for Stonyhurst and Halifax showed as little disturbance as Glasgow. At Dunecht Observatory the oscillation took place somewhat earlier and exhibited less intensity than at Aberdeen. At Bidston, however, which lies more out of the probable path of the depression than Stonyhurst, the oscillation is seemingly recorded with considerable distinctness. Temperature showed no appreciable change at Armagh, Glasgow, or Aberdeen. At the time of occurrence of the minimum a temporary change of direction and increase of velocity of the wind is recorded both at Armagh and Aberdeen. —The "arched squalls" of the neighbourhood of the trade-winds and of those regions where the monsoons blow with slight force and with interruptions, by Capt. A. Schück.

Physical Society, December 1.—Prof. G. C. Foster, president, in the chair.—Prof. Graham Bell exhibited and described the telephone before a crowded meeting of the Society, prefacing his account of the apparatus now so well known by a very complete historical sketch of the progress of electric telephony. The first experiments referred to were those of Prof. Page, who, in 1837, was studying the relation of electricity to magnetism, and found that if a coil of wire, traversed by a current, surrounds an iron rod, a sound like a pistol shot proceeds from this latter whenever the current is made or broken. He was followed by De la Rive, Poggendorff, Reiss, and others, but Reiss was the first to employ the human voice in his experiments. After pointing out that in transmitting sounds by electrical means the initial sounds themselves are in no sense transmitted but are only employed to generate currents which reproduce similar sounds, Prof. Bell proceeded to examine the phenomena which take place when sounds are transmitted through the air. It is, of course, not the motion of the vocal organs themselves that is received in the ear, but that of the air set in motion by their means, and all peculiarities in the sound must be peculiarities in the motion of that air. If the rapidity of motion varies it occasions a variation in the pitch, and the loudness is changed by changing the amplitude. The *shape* of the vibration produces timbre. If by moving the air in certain specified ways, certain vowel sounds are given out, then those same sounds will be emitted if an identical movement be occasioned by any mechanical means whatever, and Prof. Bell has found that such a motion may really be given to the air in various ways. Three classes of electrical currents have been employed for transmitting sounds to a distance, and these he denominates intermittent, pulsatory, and undulatory. The first form is obtained when a current passes for a brief interval, is then followed by an interval during which no current passes, and this by a current of the same or opposite sign. In the second class a current is continually passing, but its intensity increases and decreases instantaneously, and finally, in the third class this variation takes place gradually, and may therefore be represented by a sinuous line. In his experiments on the nature of the movement of the air Prof. Bell employed a human ear, a hay style attached to the incus recording the movement communicated to it on a moving sheet of smoked glass. A very interesting series of curves produced by this means was shown upon the screen, and he explained how his experiments in this direction led him to the present form of telephone. Since the very small membrane of the ear was capable of setting in motion comparatively large bones, it seemed probable that it could cause a light piece of iron to vibrate. In the earlier form of apparatus a piece of steel spring was therefore attached to a stretched membrane of gold-beaters' skin and placed in front of the pole of the magnet, but he found on increasing the area of metal that the action of the instrument was improved, and thus was led to do away with the membrane itself. Another branch of the investigation referred to the strength of the magnet employed, and this was modified by varying the strength of current. The battery was gradually reduced from fifty cells to none at all, and still the effects were observed, but in a much less marked degree; the action was in this latter case, doubtless, due to residual magnetism, hence, in the present form of apparatus, a permanent magnet is employed. Lastly, the effect of varying the dimensions of the coil of wire was studied, when it was found that the sounds became louder as its length was diminished; a certain length was, however, ultimately reached beyond which no improvement was effected, and it was found to be only necessary to inclose one end of the magnet in the coil of wire. A number of diagrams were projected on to the screen which showed the various forms the apparatus has taken from the time of Page to the present day. An air sung in a distant part of the building was distinctly heard in the room by the aid of an improved form of Reiss' telephone, lent by Prof. Barrett, and made by Mr. Yates, of Dublin. Prof. Bell, Prof. Foster, and Dr. Gladstone then carried on a conversation with a gentleman at a distance, and utterances were shown to be audible when the transmitting instrument was held about a foot from the mouth. A discussion then followed in which Mr. De la Rue, Dr. Gladstone, Profs. Foster, Guthrie, Atkinson, and others took part. In replying to the various questions, Prof. Bell stated that his attempts to determine the amplitude of the vibrations had not been successful, and he is coming to the conclusion that the movement must be molecular. Very distinct sounds are emitted when a considerable mass of iron is employed; and further, if the iron be glued to a piece of wood an inch thick and this be interposed between it and the magnet the action still continues. Conversation has been carried on through a distance of 258 miles, but a resistance of 60,000 ohms has been interposed without preventing the action. There is a very marked difference in the manner in which letters are reproduced by the telephone. Vowel sounds are more acceptable than consonants, and, as a rule, those letters are best transmitted which involve a large oral aperture in their utterance. Finally, he finds that high sounds are produced more fully than low ones, but this question has not yet received sufficient attention.

Zoological Society, December 4.—Prof. Newton, F.R.S., vice-president, in the chair.—Mr. Henry Seebohm, F.Z.S., exhibited and made remarks upon some of the rarer eggs and birds which he had obtained during his recent visit to the Arctic regions of the Yenesey, in Eastern Siberia, and gave a rapid sketch of his journey. Some of the skins were interesting from the fact that they extended our knowledge of geographical distribution, such as *Phylloscopus trochilus* and *Acrocephalus schœnobænus*, from long. 88° E., *Anthus gustavi* of Swinhoe (*A. seebohmi* of Dresser, and *A. batchianensis* of Gray) from the same longitude, and young in first plumage of this species.—Mr. Saville Kent, F.Z.S., exhibited the plans of a Zoological Station and Museum and Institute of Pisciculture to be established at St. Helier's, Jersey. The object sought in the establishment of this institution was the provision within British waters of facilities for pursuing marine biological investigations similar to those which exist at the Zoological Station of Naples, and at the Andersen School of Natural History at Penikese Island, Buzzards Bay, U.S.A.—The Secretary exhibited, on the part of Mr. Andrew Anderson, F.Z.S., some specimens of natural history collected in India, amongst which were chicks of *Rhynchops* and specimens of *Podiceps cristatus* obtained breeding in North-Western India.—A communication was read from Mr. Henry Lee, F.Z.S., containing an account of the capture of a Risso's grampus at Sidlesham, near Chichester.—Mr. A. G. Butler read a paper in which he gave an account of a collection of lepidoptera made in Northern Formosa by Mr. H. E. Hobson. —A communication was read from the Marquis of Tweeddale,

F.R.S., containing an account of a collection of birds made by Mr. A. H. Everett in the Island of Mindanao, Philippines. Eight new species were found in this collection, and were named *Tanygnathus everetti*, *Mulleripicus fuliginosus*, *Penelopides affinis*, *Criniger everetti*, *Orthotomus nigriceps*, *Æthopyga bella*, *Anthothreptus griseigularis* and *Ptilopus incognitus*.

Geological Society, November 21.—John Evans, F.R.S., vice-president, in the chair.—Oswald Fitch, John Hadkinson, B. Holgate, H. F. Parsons, M.D., and Edgar P. Rathbone, were elected Fellows of the Society.—The following communications were read:—On the glacial deposits of West Cheshire, together with lists of the fauna found in the drift of Cheshire and adjoiding counties, by W. Shone, F.G.S.—The chair was then taken by Warington W. Smyth, F.R.S.—The Moffat series, by C. Lapworth, F.G.S.

Victoria (Philosophical) Institute, December 3.—Mr. C. Brooke, M.D., F.R.S., in the chair. The paper of the evening was read by Mr. J. E. Howard, F.R.S., and referred to the advances that modern science had made in regard to nature, and the value of "a slow but sure path of induction."

Paris

Academy of Sciences, December 3.—M. Peligot in the chair:—The following papers were read:—On artificial production of corundum, ruby, and different crystallised silicates, by MM. Fremy and Feil. In a crucible of refractory earth is put a mixture of equal weights of alumina and minium, and calcined for some time at a bright red heat; after cooling there are found two layers, one vitreous, formed chiefly of silicate of lead, the other crystalline and often presenting geodes full of beautiful crystals of alumina. To obtain the red colour of ruby, about 2 to 3 per cent. of bichromate of potash is added to the mixture of alumina and minium. The silicate of lead on the ruby crystals is removed by the action of fused oxide of lead, hydrofluoric acid, or otherwise. A silicate of alumina (apparently dysthene) is produced by heating for some time a mixture of equal weights of silicon and fluoride of aluminium (fluoride of silicon is disengaged). Other reactions with fluorides giving crystalline bodies are described.—On invariants, by Prof. Sylvester.—On various means of accelerating the service in navigation-locks, by M. de Caligny.—Sir William Thomson was elected foreign associate, in room of the late von Baer, receiving twenty-seven votes against twenty-five for M. Van Beneden.—Report on a memoir of M. Hautefeuille, on reproduction of albite and orthose. M. Hautefeuille's process consists in keeping the elements of these minerals (free or combined), in presence of certain fused salts, such as tungstic acid and the alkaline tungstates. Thus a mixture of silica and alumina, in presence of an acid tungstate of potash at a temperature between 900° and 1,000° produces tridymite, orthose, and triclinic felspars. If the elements have been exactly proportioned, the tridymite and felspar disappear, and their elements go to increase the crystals of orthose.—On the law of absorption of radiations through bodies, and its use in quantitative spectrum analysis (first part), by M. Govi. This relates to a comparison of the curves of absorption given by wedge-shaped forms of the absorbent substance.—Practical tracing of the circle which has to be substituted for a given curve of finite extent, by M. Lécomté.—Battery in which carbon is the electrode attacked, by M. Jablochkoff. Into fused nitrate of potash or nitrate of soda is placed, as the attackable electrode, ordinary coke, and as the unattackable, platina. The electromotive force varies between two and three units, and thus exceeds that of the Bunsen and Grenet batteries. The coke may be lit and put in the nitrate in a powdered state. The gases developed by the battery are utilised. The containing vessels are of iron (that for the carbon, of iron wire).—Action of oxalic acid on silicate of soda, hydrated quartz, by M. Monier.—On M. Allaire's new method of purification of fatty water of surface-condensers, by M. Hetet.—On the respiration of submerged aquatic plants, by M. Barthelemy. These plants, observed in the normal state, do not liberate gas, even in sunlight, any more than aquatic animals (the liberation observed hitherto has been caused by experiment). The true respiratory act of these plants consists in absorption of air in solution in the water, probably by the roots.—A new one-liquid battery, by M. Jourdan. The electrodes are zinc and black-lead, the liquid an aqueous solution of sal alkali.—Occultations; graphic prediction, by M. Baills.—Observations of the spots and the rotation of Mars during the opposition of 1877, at the Rio de Janeiro Observatory, by M. Cruls. The time of rotation obtained from three values is 24h. 37m. 34s.—On a fundamental problem of geodesy; application of a general method of transformation of integrals depending on square roots (continued), by M. Callandeau.—On the rational integrals of the problem of geodesic lines, by M. Levy.—On the superficial tensions of aqueous solutions of alcohol and fatty acids, by M. Duclaux.—On some properties of boric acid, by M. Ditte. A lecture experiment is recommended, which demonstrates the liberation of heat in chemical actions. It is to add 125 grammes of water to 100 grammes of boric acid. The heat is such that an ingot of Darcet's alloy put into the mixture is fused in a few seconds.—On the formation of ultramarines and their coloration, by M. Guinet.—On the alterations of eggs, *à propos* of note by MM. Bechamp and Eustache, by M. Gayon.—On the mechanism of death produced by inoculation of anthrax in the rabbit, by M. Toussaint.—On some new mammalia of New Guinea, by M. Milne Edwards.—On compound machines, their economic produce, and the general conditions of their action, by M. de Freminville.—Process of registration and reproduction of phenomena perceived by hearing, by M. Cros (sealed packet).

Geneva

Society of Physics and Natural History, October 4.—M. Alphonse Favre has found on the Allelin Mountain, dominating the upper part of the Saas Valley near Mont Rosa, a bed of euphotide. This name is applied to a rock formed of two elements, viz., saussurite, a leaden gray mineral, and diallage, a bright green mineral, classed among amphibolites. This bed explains the origin of the erratic blocks of that substance, which are seen in numbers in the plain occupied formerly by the Rhone glacier.—Prof. Wartmann showed two apparatus based upon the properties recently recognised by him among derived currents. One is intended to determine immediately the fraction of an electric current which traverses a given conductor. The other is a current-inverser, in which the production of the extra-current is avoided, because the current always finds its passage.

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THURSDAY, DECEMBER 20, 1877

THE "*INFLEXIBLE*"

ON July 12 last we explained the *Inflexible* case at some length to our readers on the ground that there might be seen in it the beginning of a system which not only involved the safety of the four large and costly ships then commenced or contemplated, but which "having received countenance and sanction in the highest quarters in this country, may not improbably become extended over the navies of the world." A week later we considered the Parliamentary Papers on the subject, and came to the conclusion that the *Inflexible* was not a safe ship for battle, and that the objections brought against her had been much too lightly treated. Now that the Report of the Admiralty Committee is before us we are able—notwithstanding much that appears in it—to point our readers to it as a full and complete justification of the course we and others then pursued, for that Report concludes with these words :—"We therefore desire to bring under the very serious consideration of their Lordships the necessity, before proceeding with the construction of more vessels of the type of the *Inflexible*, of thoroughly investigating whether by more beam their safety may not be largely increased without impairing their speed and efficiency." As this appeal "to the very serious consideration of their Lordship's" in arrest of the construction of other ships of the type closely follows a paragraph in which the Committee show the very great advantages of an alteration in the form and proportions of the *Inflexible's* citadel (without increase of armour), it is not conceivable that the Board of Admiralty will proceed with the other vessels of the type, and it is absolutely certain that no more ships possessing the defects of which we complained in July will be laid down. The great object which we set before ourselves, therefore, is already accomplished, and the extension of a dangerous system of design throughout our own navy, and the navies of the world, has been effectually arrested. As we know that the case of the *Ajax* and *Agamemnon* was actually before the Committee, and as their Report makes no exception of them in their appeal to the Admiralty to stop further proceedings, it is to be inferred, we presume, that the beam of these two ships will have to be increased in accordance with the Committee's views. With these results before them all those who took part in bringing about this inquiry may, we think, be congratulated on the success and value of their labours.

There only remains the *Inflexible* herself to consider in the light of the Committee's Report; and in discussing this part of the subject we must not forget that no inconsiderable portion of the report, and especially the aspect which its opening pages presents, has been greatly influenced by the form of the Admiralty reference. We take leave to say that the first of the questions put by the Admiralty to the Committee has little or nothing to do with the subject. We do not remember that even Mr. Reed, who has most strongly condemned the *Inflexible's* design ever contended that "the blowing out of *the whole* of the stores and cork by shell-fire" would occur very early in an engagement; and if he had, the elicitation of a contrary opinion from the Committee would have no serious bearing upon the subject, simply because experiment, and experiment alone, can determine the degree and rapidity of the injury to which thin iron chambers filled with cork are liable. Mere opinions, in the absence of experiments, are comparatively valueless in such a case. But what we should have thought was absolutely self-evident, even without any experiment, is that shell-fire from modern ordnance would certainly blow cork packing out of thin iron chambers at some rate or other; and yet, strange to say, this is what the Committee appear to doubt, and even to deny, for they say that in their opinion that which may "be fairly assumed to represent the greatest amount of damage the ship would be likely to suffer in any action" is the condition in which, although the unprotected ends are completely riddled and water-logged, the cork and stores remain in place, and add to the buoyancy. It is fair to assume, then, according to the Committee, that in no naval action will the cork be blown out of place by shell-fire, and this in face of the fact that when an experiment was actually tried at Portsmouth the contrary result was experienced. It is of the utmost importance to note carefully that it is only by making the above extraordinary assumption—an assumption which we believe will not meet with the concurrence of scientific artillerists and seamen—that the Committee are able to oppose in any degree the opinions of the ship's danger which we and others expressed in the autumn. It is on this assumption that they rest their opinion that "the unprotected ends are as well able as the armoured citadel to bear the part assigned to them," and that therefore "a just balance has been maintained in the design." It unfortunately is made perfectly clear afterwards by the Committee that the "part assigned to them" is to hold the citadel and the rest of the ship upright, and it is clear that they cannot be presumed fit to perform this part if shell-fire can blow out the cork This is the weak point—we venture to think the dangerously and even fatally weak point—of the Committee's Report, and one which the common sense and observation of men will prevent them from assenting to, and consequently the *Inflexible's* safety is so ill-assured that we doubt if responsible persons will sanction the completion of such a ship.

The committee have fallen, as it appears to us, into a grave inconsistency, likewise, as regards the *Inflexible* herself. They say, as we have seen, that the unprotected ends are, as designed, well able to perform their part, and well balanced with the citadel. In subsequent passages, nevertheless, they go on to disclose and assert even more serious defects in them than any of us adverse critics of the ship have alleged, and to recommend an enormous extension of the cork chambers. What is the meaning of a scientific committee dealing with an extremely grave public question in this way? Either the unarmoured ends are well designed at present, or they are not; if they are, why alter them to the very large extent—far larger than a cursory perusal of the report might lead readers to expect, for the increase of cork chambers recommended is enormous? If they are not, why has the contrary statement been made and circulated? The truth is they have not been satisfactorily designed, as we shall presently prove from the Committee's own report. But first let us

dispose of a long argument which the Committee enters upon and pursues with the object of proving that by lengthening the citadel you would thin the armour upon it, and thus reduce its defensive power. Assuredly you would: nor can any one doubt for a single moment that it would be far better to reduce the armour a little for the purpose of making the citadel stable enough to hold the ship upright in spite of any injury to the unprotected ends, rather than to keep the present thickness, and to reduce its length sufficiently to cause the whole to capsize when the unarmoured ends only are badly damaged. We know how naval officers answer this question. But, in truth, the whole argument of the Committee on this point is beside the mark, and a mere beating of the air, for no one that we know of has urged the change which the Committee take so much pains to discuss. What we have always understood Mr. Reed to allege, and certainly what we have in NATURE maintained is, that in the *Inflexible* the citadel and unarmoured ends were neither well-formed, well-proportioned, nor well-balanced against each other, and that a ship of her type should have embraced a larger area of flotation within the citadel and a less area within the unarmoured ends. And this is precisely what the Committee themselves declare, and thus refute their own assertion that the ship is properly designed at present. Near the end of the Report they say:—

"Results which have been obtained in the course of the experiments at Torquay on the resistance of ships, show that a considerable increase of the extreme breadth of the *Inflexible*, if accompanied by a corresponding fining of the ends so as to keep the displacement unaltered, would, if anything, diminish the resistance of the intact vessel to propulsion at full speed. Supposing the ship thus increased in beam 10 feet, and the citadel shortened so as to retain the same perimeter and thickness of armour, her transverse stability would then be about doubled in the *e* and *f*[1] conditions, and in the riddled and gutted condition, would be more than it now is in condition *e* or *f*. Her longitudinal stability in the riddled and gutted condition would be reduced 10 per cent. (*t*, Appendix No. 15), but would not be diminished in condition *e*, and scarcely appreciably so in *f*. The increase of beam would also add to the area of the citadel in a horizontal plane, and thus increase the buoyancy in the riddled condition."

When the Committee, who lay down these clear and cogent proofs that the *Inflexible* is vastly inferior, in respect of stability and safety, to what she might have been, also tell us that "a just balance has been maintained in the design" of that ship, and that "a good result has been obtained," we find great difficulty in reconciling their statements, and feel strongly that if the public were to trust only to the language of the Report they might readily be led to draw extremely wrong inferences. We much fear that the gentlemen composing the Committee have thought too much of the Admiralty, with which they are all more or less connected, and too little of the public, who have been waiting for many months for their verdict. That verdict has been pronounced in a manner which, speaking on behalf of scientific men, we lament. It is inconsistent, and, so far as we can understand it, contradictory, in its several parts, and is in large part likely to beget in certain quarters a fatal confidence in a ship the defects and dangers of which the Committee evidently well understand. So uncertain and indefinite is it that it does not make it unquestionably clear even that the *Ajax* and *Agamemnon* are included in their desire to have progress arrested, for although after speaking of the *Inflexible* only they ask that no more vessels of the type may be proceeded with, and thus employ terms which cannot well be otherwise interpreted; the absence of all mention of their names nevertheless leaves room for the suggestion of a doubt on the point. It was clearly due to all concerned that their views on so weighty a matter should have been placed beyond all hesitation and question.

But those who would understand the full significance of this Report must not be deterred from perusing it carefully through, for if after reading thirteen out of its sixteen pages they were to throw it aside they would have derived from it, we say without hesitation, not only a very insufficient but a very untrue conception of the *Inflexible's* actual condition. Up to that point both a hasty and a deliberate perusal of it yields, to our minds at least, the impression that the Committee are admirers of the existing ship in almost every particular. But the disclosures which the scientific conscience of the Committee demanded and enforced commence on p. 14, and thence to the end facts of an appalling nature respecting her are gradually unfolded with so much effect that even the Committee themselves end by imploring the Admiralty not to repeat such a design! Let us briefly observe what these disclosures are.

The first relates to the inclining force which the action of the rudder exerts upon a ship of small stability. The Committee made experiments with the *Thunderer* expressly to acquire facts illustrative of the *Inflexible's* case, and the conclusion at which they arrived is thus stated:— "The *Inflexible* riddled and gutted,[1] and without water ballast, going at 7·24 knots, and turning in the circle of 1,210 feet in diameter, would require a righting lever or GZ of ·13 feet, and as the value of GZ at her maximum stability in this condition is only ·12 feet, *she would on this supposition overset*." To soften down this alarming fact the Committee add: "It is, however, not to be expected that the ship under this condition could be driven at this speed"—a speed of 7¼ knots only round the circle, corresponding to only eleven knots in the *Thunderer* when steaming on a straight course! And this the reader will bear in mind is true of the *Inflexible*, not when her armour has been pierced by huge shells, or her bottom knocked about by rams and torpedoes, but when nothing but her exposed unarmoured ends have been badly injured. Her armour and her bottom may be perfectly intact, ay, untouched, and yet her own rudder would capsize her in steaming at a low speed. No statement ever made about the *Inflexible* by those who condemn her has gone or ever could go much beyond this. And what can be thought of the figures given? The line GZ is the lever or arm, at the ends of which the gravity and buoyancy of the ship act in opposite directions. The length required for withstanding the rudder's action under

[1] These references *e* and *f*, are to the Parliamentary Papers, and represent the ship with the ends riddled and water-logged, *e*, showing the coal as well as the cork, &c., in place, and *f* with the cork, &c., in, but not the coal.

[1] This phrase, "The *Inflexible* riddled and gutted" is (improperly) employed by the Committee when they speak of the unarmoured ends being riddled and having the cork blown out.

the given conditions was ·13 of a foot ($1\frac{1}{2}$ inches only [!]), but even that is more than has been allowed in the design of this ship (viz. ·12) in which the Committee say "a just balance" has been preserved.[1] And this inability of the ship to withstand her own rudder's action, and that at a low speed, even with virgin armour and a bottom untouched by ram or torpedo, having been asserted and urged by others, elicits no remonstrance or objection whatever from the Committee. And yet, when a little later on in their Report they have disclosed a somewhat similar degree of longitudinal instability—discovered, as they suppose by themselves, but already well-known and obvious, doubtless, to others—and have shown that the ship would not be safe at seven and nine knots speeds because of her tendency to capsize lengthwise (so to speak), and so more or less down headforemost, then the Committee see grave danger showing itself, for they say, "We consider that any large limitation in the ship's speed may expose her dangerously to the attack of ram or torpedo," and in the summary they incidentally tell us, in the mildest terms, that a blow from either would be fatal; "the small residuum of stability she would possess would not avail to render such an attack other than fatal." The only difference in the two cases is (and this is possibly the reason why the Committee lay the greater stress on this case) that it is not here necessary to suppose the cork or stores blown out, for a single shot or shell making a large wound near the stem, bulging a skin plate outwards, and completely rupturing the internal bulkheads, would so destroy the longitudinal stability of this large ironclad, costing more than half-a-million sterling, that even at seven knots' speed she would run her bows under; "and again," we are told by the Committee, "her speed is similarly limited to nine knots by wounds of a much less critical character in other parts of her sides." We have said that even this danger was doubtless foreseen by others—as it certainly was by ourselves, whether mentioned or not—before the Committee's Report appeared; but the Committee certainly have carried the subject a step forward by the experiments they have made with the model, and by their positive declaration that "on the whole the effect of sea-waves must be to aggravate, and in some circumstances greatly to aggravate," this very serious and certain source of danger. In a word, the very Committee who have in another place asserted that in the *Inflexible* the balance is fairly maintained between the armoured and unarmoured ends, have elsewhere in their Report shown that that balance is so ill maintained between the two, that with all the cork in place one or two shot and shell penetrating the unarmoured parts would so reduce her stability that she could not be steamed ahead with any reasonable speed, but would of necessity become a prey to any ram or torpedo craft that might evade or disregard her guns!

Another disclosure of the Committee is that the mere running out of the guns "would become a serious element of danger as the ship approaches the riddled and gutted condition." Here again they employ the phrase to which we have already intimated an objection in a foot-note, and speak of "the ship" approaching "the riddled and gutted condition." They mean nothing of the kind; by "the ship" they mean the exposed unarmoured ends only; and here again it is to be observed that the danger disclosed is not one contingent upon the blowing out of all the cork, &c., but arises before, when the unarmoured part only *approaches* that condition. That the danger is, again in this case as in the previous one, a very serious and practical one is shown by the Committee recommending an alteration in the gunnery arrangements, proposing that the travel of the gun on the slides should be restricted, lest by running the guns out to the full extent at present arranged for, they should capsize the ship.

We have now sketched, sufficiently for our present purpose, the substance of the Committee's Report. We may end this article as we ended that of July 19, and repeat: "The conclusions we have arrived at are, that the *Inflexible* is not a safe ship for battle in her present state, that the objections brought against her have been much too lightly treated, and that the disclosure of her condition, with the circumstances that have followed it, have excited just surprise and dissatisfaction." That surprise and dissatisfaction will be greater than ever when the Committee's Report has had time to produce its full effects, both intended and unintended.

[1] The Committee, at the bottom of page 15, give us the means of readily illustrating the amount of stability which the *Inflexible* has [in the case above considered, for they tell us that 60 tons in the bottom of the ship, which herself weighs over 11,000 tons, would alter the length of G Z to ·12 of a foot.

HYDROPHOBIA[1]

II.

WE do not intend to do more than allude in a cursory manner to the prophylactic treatment of hydrophobia, *i.e.* to the treatment adopted to prevent the occurrence of the disease in those who have been bitten by mad dogs. The general experience of the past sanctions, as might be expected, the practice of attempting to prevent the absorption of the poison of rabies by excising or destroying by caustics the wounds inflicted by rabid animals; of the innumerable internal remedies which have been proposed and made use of with the object of preventing the development of hydrophobia in those bitten by rabid dogs, it may be said with justice that nothing whatever is known which warrants the assertions of their advocates. This is indeed a case in which the fallacies which beset all therapeutical inquiries, especially when attempted by ignorant persons and fanatics, are specially liable to obscure the truth. Of all dogs supposed to be rabid, only an infinitesimal proportion really are so, and it is but rarely that the fact of a dog being rabid is tested by having it watched until it dies, or by the unfortunate fact that some of those bitten perish by hydrophobia; then, of all persons bitten by certainly rabid dogs, only a small proportion become affected with hydrophobia, even when no treatment is adopted, so that the value of any drug or remedial measure as a *prophylactic* could only be tested by an experience such as no one ever has had.

Less uncertainty prevails in reference to the effects of treatment when hydrophobia has been developed. According to the best observers this disease has hitherto been invariably fatal. There are, it is true, a few cases—and of these two have been recorded within a comparatively recent period—in which a cure is said to have been effected, but when examined with care the gravest doubts

[1] Continued from p. 119.

of their real nature must be entertained. Here again there are fallacies to be encountered of no insignificant nature. The continued and terrible anxiety lest a bite which has been inflicted should be followed by its terrible consequences has led, in the case of persons of a susceptible and unstable nervous system—as all other long-continued worries and anxieties are liable to do—to the development of hysteria and insanity and, in the attacks of acute mania which have occurred in such individuals, many of the phenomena of hydrophobia have been imitated. An admirably-narrated case of this "hydrophobie non rabique" of French writers was lately placed on record by Mr. Hugh Norris, of South Petherton ("Hydrophobia or its Eikon—which?"—*Lancet*, September 22, 1877):—"A gentleman underwent terrible anxiety on account of his young son having been bitten by a dog which suddenly had become ill and strange in its behaviour. The danger which threatened the boy caused the father to become intensely emotional, and prolonged sleeplessness ensued. Other worries superadded themselves to this one great gnawing anxiety, and the poor man fell ill; at first there appeared symptoms which simulated very closely the hydrophobic phenomena, but these were succeeded by a genuine attack of acute mania, which necessitated the patient's removal to an asylum, where he died on the seventh day of his illness." In this case there was no evidence that the patient was actually bitten by a dog, his statement that he had been so injured having been made, in a peculiarly suspicious manner, only after his illness had commenced, and being apparently but one of the delusions which afterwards haunted him. Had this man been certainly bitten, and had his illness been followed, as it might have been, by recovery, his case would in all probability have been quoted as one of the few instances of recovery from hydrophobia, though the impartial critic would have pointed out some anomalies which rendered the conclusion doubtful. The knowledge of the undoubted occurrence of such cases necessarily imposes great caution in the examination of alleged instances of recovery from hydrophobia.

The drug to which attention has of late been directed for the treatment of hydrophobia is curare or woorara, a substance used by the natives of South America as an arrow poison. This poison has been known since the end of the sixteenth century, when Sir Walter Raleigh made the conquest of Guinea; but attention was drawn to it in a special manner by the celebrated traveller, Waterton, who first made experiments on animals with it, which were continued by Sir Benjamin Brodie and Dr. Sibson, and were succeeded by the now celebrated researches of Claude Bernard, Kölliker, and other eminent physiologists. Curare, the active principle of which is derived from a strychniaceous plant, when directly introduced into the blood or injected under the skin, produces paralysis of all voluntary movements; this paralysis depends upon its exerting a special action upon the terminations of motor nerves in the muscles, especially in voluntary muscles. The poison leaves intact the sensory nerves of the body, and at least in the early stages of its action, the nerve-centres. An animal subjected to its influence becomes absolutely motionless, and dies rapidly of suffocation from paralysis of the respiratory muscles; if, however, respiration be kept up by artificial means, life may be prolonged for long periods, and, the poison becoming eliminated, recovery may ensue. As long, however, as the stage of complete paralysis continues, the creature is entirely unable to communicate with the external world. There is no proof that external stimuli do not affect it; that it does not feel—but the channels by which the evidence of sensibility reach us are for the time interrupted. Curare has been suggested as a remedy for many diseases of a spasmodic character, but a great obstacle to its use is presented by the danger which attends its employment. A dose which will be sufficient to arrest an abnormal spasmodic contraction of a muscle or group of muscles, will presumably cause a stoppage of respiratory movements, and the medical man, if haply he be near the patient, will find himself compelled to keep up artificial respiration—no easy task to accomplish, especially with the means which the physician, as distinguished from the physiologist, could employ. Hitherto the attempts to use curare have been few, and the results (if we except the two supposed cases of cure of hydrophobia) we think worthless. Curare is indeed a drug the employment of which must be attended with so great a risk that a very strong case should be made out in its favour before patients are exposed to it.

Knowing as we do the physiological action of curare, we may ask whether it is a drug at all likely to be serviceable in spasmodic affections generally, and specially in hydrophobia. The majority of spasmodic diseases are due to a central cause, or to a cause acting through a preternaturally excitable centre. Any drug which will cut off—as curare does—the communication between the nerve-centre and the muscle will prevent its spasm, and will of course obviate any evil results which follow directly from the spasm; but, necessarily, under these circumstances the abolition of the spasm is no index that any change has been effected in the morbid state to which it owed its existence. In hydrophobia there occur, it is true, as prominent phenomena, spasms of the muscles concerned in inspiration and in deglutition. The administration of curare in doses sufficient to stop the respiratory movements would doubtless prevent these spasms, though we must not forget that it would do so at no mean risk. But are these spasms the proximate or remote cause of death in hydrophobia? We believe not. They are spasms which, as we tried to show, are probably dependent upon a morbid state of the medulla oblongata, with which is connected a morbidly heightened reflex excitability of that portion of the nervous system. But there is no evidence that curare would exert any influence in diminishing this abnormal excitability.

From our knowledge of the physiological action of curare we should not then be inclined to believe that it could affect in a beneficial manner the progress of the essential morbid processes of hydrophobia; it could but modify some of the painful phenomena which belong to it. Actually curare has been tried in several cases, but only twice has its administration said to have been attended with success, and these two cases cannot be accepted as having been certainly cases of hydrophobia. We have seen one case of hydrophobia treated with curare without any influence having been exercised by it. There are drugs, however, which the physician is habitu-

ally employing that possess actions which would cause one to predict that they would be of special use in controlling the terrible spasms of hydrophobia, *e.g.*, bromide of potassium, chloral, and Calabar bean. These drugs all diminish in a signal manner the reflex excitability of the nerve-centres; and the second in the list possesses in addition powerful properties as a hypnotic; they have all been used in the treatment of hydrophobia; and one of them—chloral—has, in sufficiently large doses, been successfully used, in so far as relief of suffering was concerned. But as for a *cure* for hydrophobia it has yet to be discovered; and this remark applies to all zymotic diseases. The majority of these diseases, unlike hydrophobia, tend naturally towards recovery rather than death, and the physician is undoubtedly able, by judicious measures, to obviate the tendency to death. He cannot *cure* the disease, however, in the sense in which he might be said to cure it were he able to destroy the poison which is its cause; apparently, once introduced into the system the poison must produce its effects—intense or slight—which must have a certain course, and then cease, because the poison which induced them has passed away, or because the soil which nourished the poison supplies it no longer with the conditions which it required. But the day may come, and we believe will come, when even this great result may be achieved; when not only shall we know the conditions which attend the spread of zymotic diseases so accurately that we shall be able to prevent their spread, but when medicine may supply us the means of dealing directly with the *materies morbi* of the diseases, as, for instance, by "sterilising" the soil in which they are implanted.

Our general review of the main facts in the history of rabies and hydrophobia has naturally brought out with considerable clearness how little is known concerning it, and how much remains to be done. We hail, therefore, with satisfaction the appointment of a Committee of the British Medical Association charged with the investigation of this important and interesting subject. This Committee consists of Dr. Lauder Brunton, Mr. Callender, Mr. Ernest Hart, and Prof. Burdon-Sanderson.

Before concluding, a few words concerning the measures to be adopted for preventing the spread of hydrophobia.

From our insular position we are in a better position than any neighbouring nation for holding a check, or even "stamping out" such a disease as hydrophobia, which, as the vast mass of evidence tends to show, does not originate spontaneously, nor does it appear that it would be difficult to effect this result were the suggestions which have been made by several recent writers carried into effect. It is certain that the number of dogs kept in England is enormously in excess of any requirements, and it is equally certain that this state of matters might promptly be put a stop to. The existing dog tax need not be increased in amount, but it should be enforced in the most stringent manner, the business of collecting, prosecuting, &c., being handed over to the police authorities of each district. Each dog should have a separate number on the local register, and might be the wearer of a collar bearing its registered number. Further, the licence should bear a description of the premonitory symptoms of rabies, and owners of dogs should be cautioned instantly to report any suspicious case to the police. These regulations would, as a necessary result, lead to each dog being individually looked after and cared for, and would, we believe, in the course of very few years, lead to the disappearance of rabies.

DIEN'S "CELESTIAL ATLAS"

Atlas Céleste, comprenant toutes les Cartes de l'ancien Atlas de Ch. Dien. Rectifié, augmenté, &c., par Camille Flammarion. 3e édition. (Paris: Gautbier-Villars, 1877.)

THIS is a new and enlarged edition of Dien's "Atlas Céleste," which first appeared in 1864, with the co-operation of M. Babinet, and is brought out under the editorship of M. Camille Flammarion. That the formation of the atlas, both in its original and extended plan has involved a great amount of labour will be evident upon a very superficial examination. The first issue was said to contain upwards of 100,000 stars and nebulæ, of which 50,000 had been observed by Lalande, projected on the development of a sphere, sixty-five centimètres in diameter, their places being reduced to the year 1860, and this scale was stated to be sufficiently large to allow of the insertion without confusion of all stars to the ninth magnitude inclusive. The charts were said to contain "nearly the totality of stars in the catalogues of Lalande, Herschel I., Piazzi, Harding, Struve, Bessel, Herschel II., Groombridge, and Argelander," while for the southern heavens recourse was had to the catalogues of La Caille and Brisbane. This description of the authorities consulted is not very definite. The reference to Harding must apply to his atlas; that to Bessel may be supposed to at least include the catalogue of equatorial stars observed by the Königsberg astronomer, which was prepared by Weisse of Cracow, and published in 1846, if not the second catalogue founded upon Bessel's observations, containing stars from 15° N. to 45° N. declination, also reduced by Weisse, which appeared in 1863. The reference to Argelander is especially indefinite; we have the well-known catalogue of 560 stars, and the "Uranometria Nova," but previous to the year 1864, when "Dien's Atlas" was published, astronomers were also in possession of vols. 3, 4, and 5 of the "Durchmusterung," with the results of the survey of the whole northern heavens.

The programme originally prepared was a very extensive one. The new edition is stated to have received numerous corrections and considerable enlargement to bring up the work to the actual state of astronomical science, and there is sufficient evidence that an attempt has been made in this direction, but we regret to have to express the opinion, after a close examination of the "Atlas," that in its present state it does not fulfil the programme upon which it was formed. It will soon be evident, on comparing the maps with the charts issued by the Berlin Academy, or more generally with those in the original edition of Harding's Atlas, that so far from containing stars to the ninth magnitude inclusive, numerous eighths, and even stars of 6·7 magnitude, are omitted, and it is not easy to see from what cause. It might be inferred that Bessel's catalogue of equatorial stars had not been utilised, since stars of the seventh and eighth magnitude observed by Bessel and not observed by Lalande, are wanting. But in addition we soon miss stars that do occur in the "Histoire Céleste," as for

instance L. 39836, a star which Lalande considered a sixth magnitude.

Different views will be taken with regard to the proper contents of a celestial atlas, intended for general use, and it is not nerefore desirable to be too critical upon this point, but to take, we will say, two extreme uses to which an atlas of the pretensions of Dien's may be applied, first for following a small planet with the aid of a chart professing to contain stars to a less degree of brightness, and secondly, for identifying the naked-eye stars by the general maps including only these brighter stars, an elementary purpose for which an atlas may be quite as readily adapted as a globe. In the former case Dien's maps are not sufficiently filled in to allow of a planet equalling in brightness stars of Bessel's ninth magnitude being identified without some trouble and disappointment, and in the latter case we meet with a failing which is only too common with star-atlases—the outlines of constellations are so prominently drawn as seriously to interfere with, if not entirely to obliterate the naked-eye stars of the lower magnitudes, in using the "Atlas" in the open air. As a model of what an atlas should be in the latter respect, we must still refer to Argelander's "Uranometria," which, in our opinion, has yet no equal for the more elementary uses of such a work.

Among the best features in the new edition of Dien's "Atlas" are the delineation of the southern heavens, in which Brisbane's stars are laid down, the view of the distribution of double and multiple stars by M. Flammarion, the orbits of some of the principle revolving double-stars, and figures of remarkable nebulæ and clusters of stars.

OUR BOOK SHELF

Horticulture. By F. W. Burbidge. With Illustrations. (London: E. Stanford, 1877.)

THIS is one of the series of small handbooks on the British maufacturing industries, edited by Mr. G. Phillips Bevan, of which we have already noticed several volumes. A compact work on practical gardening, to serve as a guide to the amateur gardener and fruit-grower, was much wanted, and this volume to a certain extent supplies the desideratum. After a short chapter on commercial gardening, the author treats of the cultivation of fruit, and of the various descriptions of vegetables and herbs; and then of gardening in its various departments, but more from the economical than from the amateur's point of view. If the owner of a garden wants to turn his bit of land to the most profitable account, he will find Mr. Burbidge an admirable guide; but if he infers from the title of the book that he will obtain from it advice as to the treatment of his pelargoniums, fuchsias, and chrysanthemums, or the management of his hothouses, he will be disappointed. We fancy that information of this kind would commend itself to a larger number of readers than the guide-book information of the exact number of acres in each of our London parks, and the annual cost of maintaining them. The advice as to the culture of fruit and vegetables seems to us very good; but the rather poor woodcuts do not add to the value of the volume.

Mittheilungen aus dem k. zoologischen Museum zu Dresden. Herausgegeben mit Unterstützung der General-direction der königlichen Sammlungen für Kunst und Wissenschaft, von Dr. A. B. Meyer, Director des königlichen zoologischen Museums. Zweites Heft mit Tafel. (Dresden, 1877.)

IN a former volume of NATURE (vol. xiii., p. 464) we have given some account of the origin of this meritorious work, of which the second portion is now before us. Like the former half of the first volume of the contributions the present section is chiefly occupied with memoirs based upon the collections made by Dr. A. B. Meyer during his well-known expedition to New Guinea and the adjacent islands. Herr Th. Kirsch, the entomologist of the Dresden Museum, commences with two articles upon the lepidoptera and beetles collected by Dr. Meyer in New Guinea. Of the former Herr Kirsch enumerates 167 species, of which 133 belong to the diurnal section. Several novelties are described and well figured. The next article is by Dr. Meyer himself, and gives us an account of a large series of Papuan skulls which he collected on the mainland of New Guinea and in the Island of Mysore, in the Bay of Geeldink. The collection, embracing altogether 135 examples, is, we believe, by far the finest of this branch of the human family ever made, and should, we suppose, lead to some definite results upon that somewhat mysterious subject—the differentiation of the various races of mankind by their skulls. A second article by Dr. Meyer relates to the specimens of anthropoid apes in the Dresden Museum. We cannot say that the photographic plates of the stuffed specimens of these creatures are either elegant or likely to be of very great use, but it is satisfactory to have the vexed question of the identity of the celebrated "*Mafoka*" lately living in the Zoological Gardens at Dresden, and long supposed to be a gorilla, finally set at rest, as is done by von Bischoff's article on its anatomy, which follows that of Dr. Meyer. A memoir on the Hexactinellid Sponges collected by Dr. Meyer in the Philippine Seas, in the preparation of which Herr W. Marshall has given his assistance, concludes this interesting volume, of which we may say that it adds materially to the status of the Dresden Museum, and to the scientific fame of its energetic director.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

The Radiometer and its Lessons

I AM obliged to ask you to allow me to add a few words, by way of further explanation, to my letter printed in NATURE, vol. xvii. p. 80.

In trying to estimate the effect of the communication of heat between a solid body and contiguous gas, I have assumed that certain simplifying suppositions may be legitimately made, for the most part identical with what are very commonly adopted in discussing the pressure exerted by a gas on a solid in contact with it. That is to say, I have assumed, first, that we may resolve the velocities of the molecules of gas into three rectangular components, one perpendicular to the surface of the solid and the other two parallel to it; second, that we may conceive of the whole number of molecules as divided into three equal parts, one-third moving in the direction of each of the resolved components of the velocity respectively; third, that the mutual pressure between the solid and the gas, and any communication of heat from one to the other, may, for the purpose in hand, be attributed to *direct* impacts of molecules against the solid surface; fourth, that all the molecules endowed with a velocity perpendicular to the solid surface, and contained within a layer adjacent to this surface of a thickness not greater than the mean length of path, will strike the surface, while none of those which are outside this layer will ever reach it; fifth, that the particles which have struck the solid surface will return from it with an average velocity corresponding to the temperature of the surface, and will retain this velocity until they arrive at the farther side of the layer before-mentioned. It was on the supposition that these are legitimate assumptions that

I spoke of heat passing across a stratum of gas from one solid surface to another "*as though* there were, in contact with each solid surface, a layer of gas whose temperature is throughout the same as [it would perhaps have been been better to have said "determined by"] that of the contiguous solid."

I am fully aware of the ease with which one may be led into serious mistakes by trusting too implicity to such simplifying assumptions, and also that some of the particular suppositions made above would be inadmissible in a discussion of the general problem of the conduction of heat in gases; but I do not see any fallacy in employing them for the special purpose which I had in view in my last letter, namely, to show why I think that the flow of heat across a thin stratum of gas must be facilitated by diminishing the pressure of the gas. Prof. Osborne Reynolds's argument that "if there were a layer of uniform temperature, no heat would be transmitted," does not appear to me to be applicable to the case in question. It seems conceivable, as an extreme case, that, in a very thin layer of gas, between parallel solid surfaces maintained at different temperatures, the molecular movements might take place exclusively in the direction of the perpendicular to the bounding surfaces. In such a case the particles would move from side to side of the layer of gas with a uniform velocity, though the velocity one way would be greater than the velocity the other way, and heat would be transmitted across a layer of gas having the same temperature throughout. Such a condition, whether practically realisable or not, would, if I understand him aright, be the limiting case in one direction of what Mr. Stoney has called for shortness a "Crookes's layer:" the limiting case in the other direction being the ordinary condition of a gas, where the average velocity of the molecules is independent of direction. I venture to think that, in pointing out the results which must follow from the existence of a predominating direction of molecular motion, Mr. Stoney has made a very important contribution to the kinetic theory of gases; and I do not see that his conclusions are in any way invalidated by its being shown that they are not in harmony with "the generally-accepted laws of gases," inasmuch as these laws are deduced from suppositions which expressly exclude the conditions he has investigated. G. CAREY FOSTER

December 17

ALLOW me to say a few words on what I believe to be the correct theory of the radiometer. This theory was given to me by Prof. Osborne Reynolds during spring of 1875, and I have found it capable of explaining every experiment on the subject with which I am acquainted.

The conservation of momentum is one of the laws of nature which even molecules do not break, and that law puts some restraints on the wonderful things which the shocks of molecules can accomplish. Imagine a vessel full of gas at a certain temperature. The centre of gravity of the gas and that of the vessel are supposed to be at relative rest. Suppose now that I increase the velocity of a certain number of molecules in a given direction, the centre of gravity of the gas will move relatively to the centre of gravity of the vessel, and no number of encounters between the molecules can alter that motion until the momentum has been taken up by the vessel. If in any gas we have a passage of heat in a certain direction, we shall have a propagation of momentum owing to the fact that the molecules move more quickly in one direction than in the opposite one, and no number of encounters can alter that propagation. Where the momentum enters the gas and where it leaves it we observe certain forces. This is Prof. Reynolds's theory of the radiometer. It has been objected that an increased pressure on the cool side of the vanes of a radiometer will counterbalance the force acting on the blackened sides, when the dimensions of the vessel are large compared with the mean path of a molecule, but I do not think that such is the case. The following special case may make this point a little clearer. If the forces on the vanes are counterbalanced, the forces on the vessel must be counterbalanced as well. In the case of an ordinary radiometer the forces reduce to a couple, and I do not see how any crowding of molecules in one part of the vessel more than in another can produce a couple on the vessel. The whole problem is one of conduction of heat. All the experiments made by Mr. Crookes on cups, inclined vanes, &c., admit of the same easy explanation as the fact that when a long and a short wire are connected with the poles of a battery, the current in the shorter wire will be the strongest. In a radiometer with inclined vanes, for instance, the temperature is the same on both sides, but the gradient of temperature is much larger on one side, and hence more heat will escape on that side. The dimensions of the vessel also have to be taken into account in the same way as the length of a wire has to be taken into account when the strength of an electric current flowing through it has to be calculated. It is difficult to say exactly what takes place within very small distances from the hot surface, but it seems clear that any phenomenon, such as Prof. Carey Foster supposes to exist, must affect the passage of heat in the same way as the force on the vanes. As the careful researches of Messrs. Kundt and Warburg have shown that under great exhaustion the conduction of heat decreases and does not increase, I do not see how an increase in the force can take place.

The scientific world will judge how far Prof. Stoney has succeeded in establishing any new laws on the conduction of heat through gases. In justice, however, to Messrs. Provostaye and Dessains, whose experiments he calls to his aid, I wish to point out that their numerous experiments, with two exceptions, are in entire accordance with existing theories. At the time these experiments were made, no distinction was drawn between convection and true conduction. In order to deduce, therefore, the loss of heat due to true conduction, Prof. Stoney is obliged to subtract the effect due to convection currents. He draws, therefore, a curve representing the loss of heat due to this cause. All his conclusions must stand or fall with this curve, and I am afraid they must fall.

After Professors Clausius and Maxwell had deduced theoretically the coefficient of conductivity for gases, a series of celebrated experiments were made by Stefan, by Narr, by Plank, by Winkelmann, and last, but not least, by Kundt and Warburg. The influence of convection currents has been fully discussed in these papers and eliminated, and the conclusions arrived at by all these experimenters are fully in accordance with each other and with theory. It appears, as was expected, that when the effects of convection currents are eliminated, the coefficient of conductivity is independent of pressure until the dimensions of the vessel are comparable with the mean free path of a molecule, and that then the conductivity rapidly diminishes. It also appears that at the pressures at which Messrs. Provostaye and Dessains found that the loss of heat was independent of pressure, convection currents must have ceased to be appreciable, and therefore the great mass of their experiments is fully in agreement with later researches.

The only exception is found in the case of carbonic acid and nitrous oxide. These abnormal results were not confirmed by Messrs. Kundt and Warburg in the case of carbonic acid, the only one of the two gases which they examined. Whoever reads their account of the difficulty they had in excluding the last traces of moisture, and considers the increased conductivity which such an admixture would produce as the pressure diminishes, will have no difficulty in explaining the anomaly. At any rate I do not think Prof. Stoney will be inclined to base important conclusions on unconfirmed experiments on two gases in which we should expect the effect, owing to their density, to be particularly small. The discovery of Master Gerald Stoney, who found that a red hot wire was cooled when a tin can containing water was brought sufficiently close might, I think, have been foretold by the recognised theory. Prof. Stoney, no doubt, will find on reading over the literature on the subject, that what he calls penetration of heat, has hitherto been known under the name of conduction of heat, that it takes place at all pressures, and begins to disappear at the exact point at which he makes it appear.

The timely calculation of Mr. S. T. Preston in the August number of the *Phil. Mag.*, shows that any theory of the radiometer which makes the action depend on the comparatively large ratio of the mean free path to the dimensions of the vessel, must necessarily be wrong. ARTHUR SCHUSTER

The Proposed Channel Islands' Zoological Station, Aquarium, and Piscicultural Institute

I AM very anxious that this project[1] should succeed, mainly because of the facilities it will afford to inland aquaria, in procuring living animals cheaper, better, more variedly, and more systematically, than at present. This, I believe, will form the most profitable part of the undertaking.

[1] Referred to in NATURE, vol. xvii. p. 102.

I hope, also, it may succeed as a sort of living museum, without any of the kind of attractions which are not biological ones, and which, indeed, are not scientific in any sense, as common and ribald music, theatricals, acrobatic and jugglery performances, and so forth. Only, no aquarium has ever permanently thriven without these things.

I greatly doubt whether anyone yet possesses the requisite knowledge to be able to rear any marine crustacean from the egg state to an adult condition, and to feed it in captivity in such manner as to be able to sell it in the open market at rates below those sold under present circumstances. Yet this is put forward, conspicuously, as one of the aims of the scheme. In Britain are eaten as human food about a dozen species of crabs, lobsters, prawns, and shrimps, and most of these have been occasionally bred in aquaria as far as the Zoea state, when they are free swimmers, and they then generally die. Rarely, some few have been brought up to a higher stage, but I know of no instance, during twenty-five years of experience, where any marine crustacean of any kind has been reared to an adult condition in an aquarium. And if such a thing could be done, I believe that to feed them in any state of captivity, with animal food, which they require in great abundance and variety, and which must be purchased, would be very expensive, indeed, far too costly I think, to be practically and commercially remunerative.

I am glad to see that in last week's NATURE, vol. xvii. p. 133, it is stated that that excellently-tasted little fresh-water lobster, *Astacus*, has been bred artificially by a Piscicultural Institution at Schwerin. If so, why should it not be similarly bred in Britain, where it is much seldomer eaten than in France and Germany. In Berlin, Hamburg, and Dresden, I have often purchased it at sixpence a dozen, while in Paris I have given as much as sixpence each for it. It is a pity, however, that the Schwerin account is not more full and explicit. It is stated that in the spring of 1876, 700 *Astacus* in egg, were placed in two round ponds, each of six feet diameter. Holes were made in these, and recently, on draining the ponds, only three or four adult crayfish were found straying about the ponds, the rest each being in a separate hole, and a large number of young ones were found, as big as bees, and very lively. What size were the crayfish at birth, and if very small, and swimming, how were they prevented from escaping from the ponds? Information is wanted as to the shape, length, breadth, direction as to angle, and distance apart of these holes, and their position in the ponds, whether in the sides or base, or both. If nearly 700 animals occupied as many holes, where were the young ones? How many young were there? If each female had only as few as 100 eggs hatched out, then 70,700 must have been the population of these two little pools. When, and in what manner, were the males introduced? We require also to be told of the material of which the ponds were constructed, and if the sides were upright, and the bottoms flat, or if rounded or basin-shaped. If water ran in and out, how much in a named time, of what quality, as to foreign substances it contained in solution and suspension, and what was its temperature at various periods of the year? In what direction and in what amount was light admitted? How much vegetation, and of what kinds, grew in the ponds? What kinds of animal food was given them, and how much and often, and was it cooked or raw? Carrots appear an odd food for crayfish. Let all these things and more, be carefully ascertained, to see if they can be applied to the culture of *Homarus*, the near marine relative of *Astacus*, before much money is spent.

It would be an excellent thing for students to have a place to study at, such as is proposed to be provided for them at Jersey, and similar to the zoological station and aquarium at Naples, in the arrangement of which I had much to do. But would students be content to go only so far as Jersey? Is not the access too easy, and too cheap, as it is not easy or cheap to go so far as Naples, and to have the *name* of so going? I have often thought it odd, and evincing not at all a really zealous spirit in my own direction on the part of my fellow-naturalists, that such a thing should be, that though the Crystal Palace Aquarium has existed for seven years within less than one hour's railway ride from London, and though it contains a constantly advertised collection of living marine animals exceeded in variety and interest by none in Britain, or even Europe, yet no scientific man, except the late Edward Newman, has ever applied for permission to carry on any course of inquiry here on any subject, continuously or occasionally, connected with the habits of living creatures, in the spirit of say, Gilbert White of Selborne. Yet we offer all such advantages as table-space, good light, and the use of any animals in our tanks not having a considerable money value—lest injury be done to such specimens—absolutely free of all charges. At this moment we possess many Italian animals in our collection, as fishes, crustaceans, mollusks, zoophytes, &c., which can be seen alive nowhere else, save on the shores of the Mediterranean, and yet no professed zoologist known to me ever comes to see them, or takes the smallest interest in them. They are therefore beheld only by the general public, who only look at them for their mere prettiness, or for what untrained observers are complacently pleased to term "ugliness." No student ever asks us for more than any dead animals we may chance to have, and which we give away gratis, and these apparently afford far more pleasure than the sight of living specimens. It is not at all uncommon to meet with biologists who openly and avowedly proclaim their contempt for collections of living animals in aquaria, which they regard as being "well enough for women and children," but for *men* they say there is nothing like seeing such animals in rows of glass jars of alcohol on the shelves of a museum. My last contribution of any length to NATURE was made so long ago as October 12, 1871, when I gave a description of the Crystal Palace Aquarium, then only just opened. Since then we have had no cause to complain of the appreciation of the world of sightseers. But as regards the indifference of the scientific world, that has been and is so great that the place might never have existed. William Yarrell, the British ichthyologist of the generation just passed away, used to tell me how glad he would be to see a live John Dory (*Zeus faber*), and how much he would give to behold one swimming. But here, at Sydenham, this fish can be seen alive and in perfect health for months together, in crystalline sea-water. Yet no Yarrell ever comes to see them. Are there no Yarrells, and Whites, and Watertons, and Newmans now? or has their very spirit passed away into the region of apathy where the affectation of caring for nothing, and of being never moved to zeal in anything, in this observation of live animals, seems to be regarded as a very high accomplishment?

W. A. LLOYD

Crystal Palace Aquarium, December 15

The "Challenger" Estimates of the Volume of the Gulf Stream

IN the interesting "Voyage of the *Challenger*," just published, Sir Wyville Thomson states (vol i. p. 371) "that the Gulf Stream in its restricted sense was, early in May, 1873, at the point where we crossed it and made our observations, about sixty miles in width, 100 fathoms deep, and its rate three knots an hour." I was much surprised at reading this, as the Admiralty Report on Ocean Soundings, No. 7, p. 12, estimates it at the same point as "100 fathoms deep, and running at the rate of three miles an hour *for a width of fifteen miles*, discharging four and a half cubic miles of heated water per hour."

As no reference is made by Sir Wyville Thomson to the extraordinary discrepancy in these two estimates of the same thing at the same time—one being four times the volume of the other—and as he says he makes the statement "thus guardedly" I think, in the interest of scientific accuracy, an explanation is required.

T. MELLARD READE

Liverpool, December 8

The Fossil Peronospora as a Primordial Plant

THE concluding sentence of your notice (vol. xvii. p. 128) of my observations on a fossil fungus is so important, that I shall be glad of a word of reply. You say, "But should not this primordial plant have led a non-parasitic life? for if parasitical, then this fact points to some pre-existing plant."

Although the specimen I have figured is shown as growing within the decayed tissues of a Lepidodendron, yet it does not follow that the same fungus could not perfect itself on humus alone. Recent species of Peronospora show a tendency to grow upon the ground, as several species, including the fungus of the potato disease, will grow and produce fruit on the naked earth. A truly terrestrial species is found in the allied *Botrytis terrestris*, Persoon, and many of the Mucedines grow freely in cellars, on damp walls, or in any moist place.

WORTHINGTON G. SMITH

15, Mildmay Grove, N.

THE "*CHALLENGER*" IN THE ATLANTIC[1]

THE *Challenger* left Portsmouth on December 21, 1872, and on the evening of May 24, 1876, she dropped her anchor at Spithead after an eventful voyage, which lasted three and a half years. Shortly after her arrival we gave a sketch of her cruise over the Atlantic and Pacific Oceans. The two volumes just published consist chiefly of an abstract of the less technical portions of the journal kept by Sir Wyville Thomson during the first year of the *Challenger's* voyage, and during the early part of the fourth year's voyage, when she was on her way home. During both these periods the *Challenger* was in the Atlantic, so that we now obtain the record of her survey of this great ocean in a very complete form, and are led to look forward to several additional volumes, in which the account of her cruise in the Pacific Ocean and amongst its fair islands will appear. A great deal of credit must be given to the author of these two splendidly illustrated volumes for his so speedily publishing them. A large portion of one of them was actually passed through the press while the *Challenger* was at sea, and the preparation of the second volume had to be carried on amid the cares not only of professional duties, but also of getting the immense collections made into order, and of making arrangements for the thorough working out of the scientific results of the voyage. May we express the hope that his energy will enable him speedily to complete the popular narrative of this cruise thus so auspiciously begun. The strictly scientific records of the *Challenger* voyage cannot be published for some time; the working out of old forms, the describing and illustrating of new ones, takes time; such work, to be done well, must necessarily be done slowly, and hence we all the more urge on Sir Wyville Thomson to let us have, as soon as can be, the completion of the popular narrative of the general results of his four years' work. This preliminary account is indeed not solely a popular one, for we find in these two volumes a mass of exact scientific details that will make them always works of reference to the scientific student; and while some few of the wondrous new species of animals and plants are but incidentally introduced to us, their descriptions are often so well written, and their forms are so exquisitely portrayed, as to leave us for the time somewhat independent of their more exact scientific diagnosis.

In our previous sketch of the voyage of the *Challenger* we dwelt somewhat in detail on the work accomplished by her during the first six months of 1873. About the middle of June in that year she left the Bermudas for the Açores and Madeira, establishing twenty-five stations on her way, some of these showing ocean depths of 2,800 fathoms. A few pleasant days were spent (July 1873) at Ponta Delgada, the capital of San Miguel and the chief town of the Açores. On acccount of the presence of an epidemic of small-pox no delay was made at Madeira, but the vessel's course was struck for the Canaries and Cape de Verde Islands, keeping somewhat parallel to the Coast of Africa until nearly opposite Cape Palmas, when they turned westward and shaped their course to Saint Paul's Rocks. These solitary rocks are nearly under the equator, midway between the coasts of Africa and of South America. They were visited in 1832 by the *Beagle*, and are noticed in Darwin's charming "Voyage of a Naturalist." Merchant-vessels usually give them a wide berth. They seem to have struck the travellers by their small dimensions; it being rather under a quarter of a mile from the one end of the group to the other, they form quite little specks of rocks out in mid-ocean. Landing on these rocks was no easy matter. A loop of eight or ten ply of whale-line was passed round one of the rocks; to this a hawser was run from the ship lying about seventy yards out, with her bows in 104 fathom water; the hawser was made fast to the whale-line, and the ship thus moored to the rocks. Having landed on the rocks a line was laid across the mouth of the cove, which made the landing easier for the next parties.

Only two species of birds were found on the rocks, the "booby" (*Sula fusca*) and the "noddy" (*Sterna stolida*), both being widely distributed birds on tropical islands and shores. They were here in enormous numbers, were quite tame, even allowing themselves to be taken up with the hand. The breeding season was over. No land plants were found, not even a lichen. The terns used a green alga to line their nests; all the crannies of the rock were crowded with an amphibious crab (*Grapsus strigosus*), which was much more wary than the birds, though "wherever a morsel of food came within their reach there was instantly a struggle for it among the foremost of them, and they ambled away with their prize wonderfully quickly: their singular sidelong gait and a look of human smartness about them had a kind of weirdness from its being exhibited through a set of organs totally different in aspect from those to which one usually looks for manifestations of intelligence."

Leaving these desolate rocks on August 29, the island of Fernando Noronha was in sight on September 1, rising like most of the ocean islands, abruptly from deep water, the depth of the ocean within six miles of the island being more than 1,000 fathoms. This island presents a most remarkable appearance; the land is generally not very high, but there is an irregular cliff which rises to a height of about 100 feet from the sea, succeeded by undulating land and conical hills, usually covered with luxuriant vegetation. The Peak is an extraordinary-looking mountain, formed of a column of rock which starts up to a height of 600 feet from a more or less level plateau of rock, itself some 400 feet above the sea. There is a village and a citadel, the place being a penal settlement belonging to Brazil. There were at the time on the island nearly 1,400 convicts and a garrison of 200 soldiers. The convicts enjoyed a considerable amount of liberty, each of them occupying a hut, and being allowed to cultivate a little piece of garden ground, though their time and labour from six in the morning until four in the evening belonged to the Government. Sir Wyville Thomson and his assistants were extremely anxious to investigate thoroughly the flora and fauna of this island, but unfortunately the military commandant set his face against this, and the land work had to be abandoned.

"The coast scenery was here and there very beautiful, little sandy bays with a steep cultivated slope above them, or a dense tangle of trees absolutely imbedded in one sheet of matted climbers, separated by bold headlands of basalt or trap stuff. Besides the tropic birds, there were to be seen beautiful little terns, snowy white, which usually flew in pairs a foot or two apart, one following all the motions of the other, like a pair of paper butterflies obedient to the fan of a Japanese juggler. They could be seen flying over the land, and often alighting upon the trees. The noddy was very common, and the booby was in considerable numbers. High upon the cliffs the nests of the frigate bird (*Tachypetes aquila*) could be seen, and from time to time these splendid birds moved in slow and graceful circles overhead." No wonder that the author adds, "We lay for some time below the cliffs admiring the wonderful wealth of animal and vegetable life ere we returned slowly to the ship."

On September 14, as they neared the coast of Brazil, a shower of butterflies fell on the ship, fluttering in multitudes over it; and over the sea as far as the eye

[1] "The Voyage of the *Challenger*. The Atlantic: a Preliminary Account of the General Results of the Exploring Voyage of H.M.S. *Challenger* during the Year 1873 and the Early Part of the Year 1876." By Sir C. Wyville Thomson, Knt., LL.D., F.R.SS. L. and E., &c., Regius Professor of Natural History in the University of Edinburgh, and Director of the Civilian Scientific Staff of the *Challenger* Exploring Expedition. Two volumes. Published by Authority of the Lords Commissioners of the Admiralty. (London: Macmillan and Co., 1877.)

could reach they quivered in the air. Looking up into the sky where they were thickest, they were seen to be close together and had much the appearance and peculiar motion of large flakes of snow. Amidst such a downpour the entrance to Bahia was seen. It is very beautiful; the coast is not elevated; it is neither mountainous nor hilly, but rises fiom the sea-shore in even terraces, broken here and there by ravines and wooden knolls, every space gloriously clothed with vegetation, and the sky-line broken by long lines of palm trees—from the sea it reminded one of Lisbon, but its splendid luxuriance of vegetation gives it a character of its own.

The scientific work of the *Challenger* was to be on the ocean, and Sir W. Thomson properly discouraged his staff from expending too much of their time or energies on investigating the natural history of the few spots of North or South America that they from time to time landed on. We therefore in these volumes meet with very few references to the glimpses that they got of this continent, but some time had to be spent at Bahia, and we cannot avoid giving the following interesting extract which describes a visit made by Sir W. Thomson to Santo Amaro.

"Mr. Wilson was obliged to be next day at Sto. Amaro, a little town about thirty miles distant, across one of the ridges on another river where he had a line of steamers plying, and he asked us to ride there with him; so we went back to his house and dined, and spent the evening at his window inhaling the soft flower-perfumed air and gazing at the stars twinkling in their crystal dome of the deepest blue, and their travesties in a galaxy of fire-flies glittering and dancing over the flowers in the garden beneath us. It was late when we tossed ourselves down to take a short sleep, for two o'clock was the hour fixed to be in the saddle in the morning. We rode out of the town in the starlight, Mr. Wilson, Capt. Maclear, and myself, with a native guide on a fast mule. We were now obliged to trust entirely to the instinct of our horses, for if a path were visible in the daylight there was certainly none in the dark, and we scrambled for a couple of hours right up the side of the ridge. When we reached the top we came out upon flat open ground with a little cultivation, bounded in front of us by the dark line of dense forest. The night was almost absolutely silent, only now and then a peculiar shrill cry of some night-bird reached us from the woods. As we got into the skirt of the forest the morning broke, but the *réveil* in a Brazilian forest is wonderfully different from the slow creeping on of the dawn of a summer morning at home, to the music of the thrushes answering one another's full rich notes from neighbouring thorn-trees. Suddenly a yellow light spreads upwards in the east, the stars quickly fade, and the dark fringes of the forest and the tall palms show out black against the yellow sky, and almost before one has time to observe the change the sun has risen straight and fierce, and the whole landscape is bathed in the full light of day. But the morning is for yet another hour cool and fresh, and the scene is indescribably beautiful. The woods, so absolutely silent and still before, break at once into noise and movement. Flocks of toucans flutter and scream on the tops of the highest forest trees hopelessly out of shot, the ear is pierced by the strange wild screeches of a little band of macaws which fly past you like the wrapped-up ghosts of the birds on some gaudy old brocade. There is no warbling, no song, only harsh noises, abrupt calls which those who haunt the forest soon learn to translate by two or three familiar words in Portuguese or English. Now and then a set of cries more varied and dissonant than

FIG. 1—The *Challenger* at St. Paul's Rocks.

usual tell us that a troop of monkeys are passing across from tree to tree among the higher branches; and lower sounds to which one's attention is called by the guide indicate to his practised ear the neighbourhood of a sloth, or some other of the few mammals which inhabit the forests of Brazil. And the insects are now all awake, and add their various notes to swell the general din. A butterfly of the gorgeous genus *Morpho* comes fluttering along the path like a loosely-folded sheet of intensely blue tinsel, flashing brilliant reflections in the sun; great dark blue shining bees fly past with a loud hum; tree-bugs of a splendid metallic lustre, and in the most extraordinury harlequin colouring of scarlet and blue and yellow, cluster round a branch so thickly as to weigh it down, and make their presence perceptible yards off by their peculiar and sometimes not unpleasant odour; but how weak it is to say that that exquisitite little being, whirring and fluttering in the air over that branch of *Bignonia* bells, and sucking the nectar from them with its long curved bill, has a head of ruby, and a throat of emerald, and wings of sapphire—as if any triumph of the jeweller's art could ever vie in brilliancy with that sparkling epitome of life and light.

"It was broad day when we passed into the dense forest through which the greater part of the way now lay. The path which had been cut through the vegetation was just wide enough for use to ride in Indian file, and with some care to prevent our horses from bruising our legs against the tree-trunks, and we could not leave the path for a single foot on either side, the scrub was so thick, what with fallen tree-trunks, covered with epiphytes of all descriptions, and cycads, and arums, and great thorny spikes of *Bromelia*, and a dense undergrowth, principally of melastomads, many of them richly covered with blue and

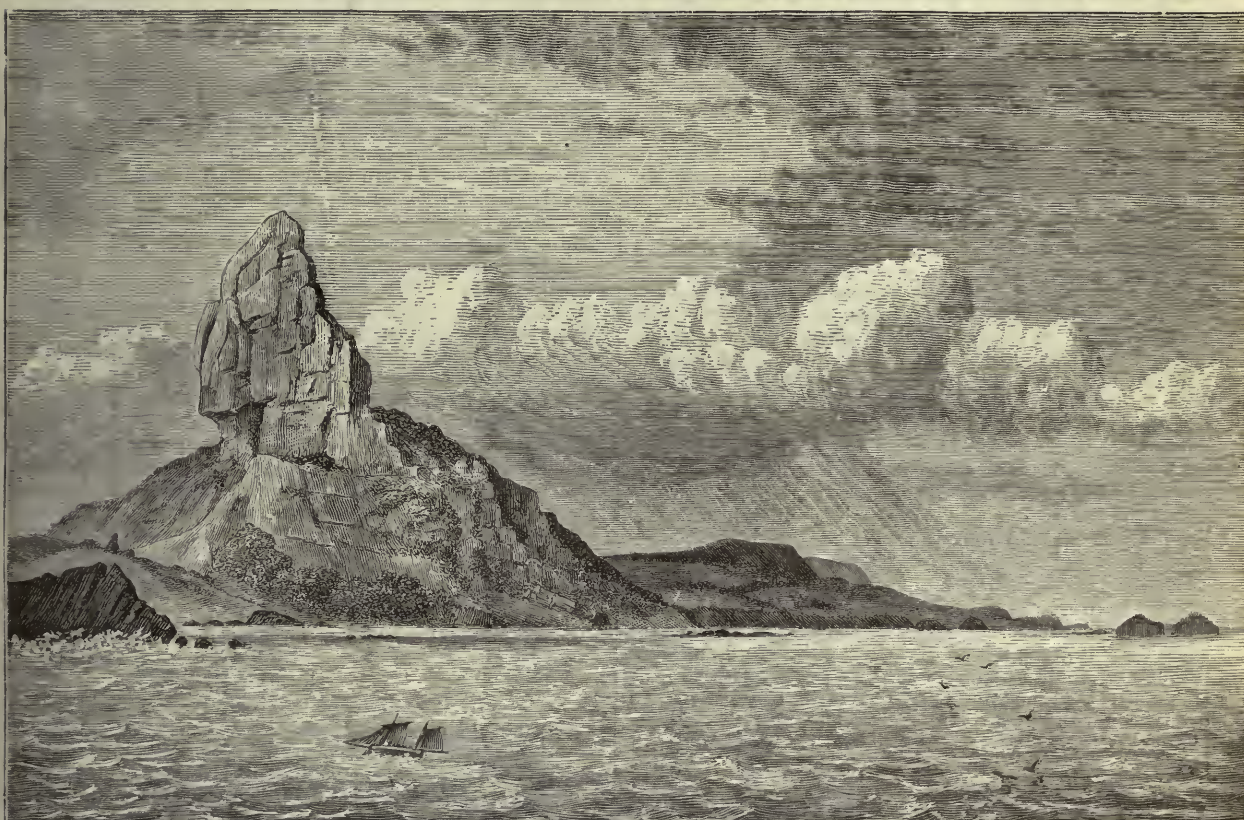


Fig. 2.—Fernando Noronha.

purple flowers. Above the undergrowth the tall forest trees ran up straight and branchless for thirty or forty feet, and when they began to branch, a second tier of vegetation spread over our heads, almost shutting out the sky. Great climbing *Monsteras* and other arals; and epiphytic bromeliads; and orchids, some of them distilling from their long trusses of lovely flowers a fragrance which was almost overpowering; and mazes of *Tillandsia* hanging down like tangled hanks of grey twine. Every available space between the trees was occupied by lianas twining together or running up singly, in size varying from a whipcord to a foot in diameter. These lianas were our chief danger, for they hung down in long loops from the trees and lay upon the ground, and were apt to entangle us and catch the horses' feet as we rode on. As time wore on it got very close and hot, and the forest relapsed into silence, most of the creatures retiring for their noonday siesta. The false roof of epiphytes and parasites kept off the glare of the sun, and it was only at intervals that a sheaf of vertical beams struck through a rift in the green canopy, and afforded us a passing glimpse of the tops of the forest trees, uniting in a delicate open tracery far above us.

"For some hours our brave little horses struggled on, sometimes cantering a little where the path was pretty clear, and more usually picking their way carefully, and sometimes with all their care floundering into the mud-holes, imperfectly bridged over with trunks of trees.

"As we had made our ascent at first, all this time we had been riding nearly on a level on the plateau between the two river valleys. Suddenly the wood opened, and we rode up to the edge of a long irregular cliff bounding

the valley of Sto. Amaro. The path ran right up to the edge and seemed to come to an end but for a kind of irregular crack full of loose stones which went zigzagging down to the bottom at an angle of about 70°, and we could see the path down below winding away in the distance towards the main road to Sto. Amaro. We looked over this cliff and told Mr. Wilson firmly that we would *not* go down the side of that wall on horseback. He laughed, and said that the horses would take us down well enough and that he had seen it done, but that it was perhaps a little too much ; so we all dismounted, and put the horses' bridles round the backs of the saddles and led them to the top of the crack and whipped them up as they do performing horses in a circus. They looked over with a little apparent uneasiness, but I suspect they had made that precarious descent before, and they soon began to pick their way cautiously down one after the other, and in a few minutes we saw them waiting for us quietly at the bottom. We then scrambled down as best we might, and it was not till we had reached the bottom, using freely all the natural advantages which the *Primates* have over the *Solidunguli* under such circumstances, that we fully appreciated the feat which our horses had performed.

"The next part of the road was a trial ; the horses were often up nearly to the girths in stiff clay, but we got through it somehow, and reached Sto. Amaro in time to catch the regular steamer to Bahia."

And here is an uncommonly good anecdote about a parrot :—

"At Sto. Amaro a line of tramways had lately been laid down also under the auspices of our enterprising friend, and we went down to the steamboat wharfs on one of the trucks on a kind of trial trip. The waggon went smoothly and well, but when a new system is started there is always a risk of accidents. As the truck ran quickly down the incline the swarthy young barbarians, attracted by the novelty, crowded round it, and suddenly the agonised cries of a child, followed by low moanings, rang out from under the wheels, and a jerk of the drag pulled the car up and nearly threw us out of our seats. We jumped out and looked nervously under the wheels to see what had happened, but there was no child there. The young barbarians looked at us vaguely and curiously, but not as if anything tragical had occurred, and we were just getting into the car again, feeling a little bewildered, when a great green parrot in a cage close beside us went through no doubt another of his best performances in the shape of a loud mocking laugh. A wave of relief passed over the party, but we were rather late, and the drivers expressed to the parrot their sense of his conduct, I fear strongly, but in terms which, being in Brazilian *patois*, I did not understand."

In another notice we will tell of the *Challenger's* doings between Bahia and Cape Town, and from the Falklands home, and we will also more particularly allude to the general results of the scientific work she has so successfully accomplished.

(*To be continued.*)

ON THE PRESENCE OF OXYGEN IN THE SUN

I HAVE spent the greater part of last winter and the beginning of this in an investigation of the spectra of oxygen. My experiments will be published, I hope, in another place ; but there are one or two points of more immediate interest, and, I venture to think, of some importance, which I trust you will allow me to discuss in your columns.

Prof. Draper has lately announced the important discovery that the lines of oxygen are found to be present in the sun. These lines, however, are bright, and not dark, as the Fraunhofer lines. I had found that at a certain temperature, lower than that at which oxygen shows its well-known lines, it gives another spectrum, and it occurred to me, when I heard of Prof. Draper's discovery, that if the temperature of the sun, at some point intermediate between the photosphere and the reversing layer was the same as that at which the spectrum of oxygen changes, the fact that the *known* spectrum of oxygen appears bright would be fully explained. The spectrum of lower temperature, which, for reasons to be given, I shall call the compound line spectrum of oxygen, ought in that case to be found reversed in the solar spectrum, like the remainder of the Fraunhofer lines.

I have consequently devoted all my time during three weeks to the exact measurement of these four lines, and I do not think that the evidence which I am about to give will be considered to fall far short of an absolute proof that the spectrum is really reversed in the sun.

Two difficulties have put themselves into the way of exact measurement. The first is due to the extreme weakness of the spectrum. The light itself is not stronger than that of a non-luminous Bunsen burner ; and after that light has passed through four prisms, as in most of my experiments, or through seven, as in some of them, there is not much of a spectrum left to be measured. It is only after having been in the dark for half-an-hour that the eye is able to do the work, and there are a good many days when the eye never obtains sufficient sensitiveness to make any trustworthy measurements. But whenever my eyes were in sufficiently good condition, my measurements agreed so well, that I have no hesitation in saying that they are as accurate as the measurements of the solar lines which will be found by their side. The second and more serious difficulty is due to the fact that the lines in question widen to a great extent with increased pressure and in such a way that the brightest part, and still more, the centre of the band, is displaced towards the red. I have not been able to get the lines perfectly sharp, and the measurement of the centre of the band will give, therefore, too high a value of the wave-length. The following table contains the numbers which I have obtained :—

Oxygen.	Width.	Solar Lines.	
		A.	S.
α 6156·86	±0·3	6156·70	6156·69
β 5435·55	±0·3	5435·44	5435·56
γ 5329·41	±0·6	5329·3	5329·10
δ 4307·62		4367 58	

The first column contains the wave-length of the compound line spectrum of oxygen. The second column contains the number which has to be added or subtracted from the wave-length, in order to get the edge of the lines, as it is their centres which are given in the first column. The third and fourth columns give the wave-lengths of the corresponding solar lines as observed by Angström (A.) or myself (S.). The greatest difference is found in the line γ, but even this difference only amounts to the twentieth part of the distance between the sodium lines, and it would require a spectroscope of very good dispersive power and definition to separate two lines which would be that distance apart from each other. Nevertheless the amount in question is greater than the possible errors of observation, and I believe the difference to be due to the fact mentioned above, that the lines widen unequally. It will be seen from the table that the solar line would fall within the oxygen line, but about one-third of the distance between its most refrangible and least refrangible edge. At a higher pressure the brightest part of the band lies about 5331. None of the other lines widen nearly as much, and δ is always perfectly sharp. Angström gives it as an iron line, but according to Kirchhoff, the solar line is composed of two lines, and separated by a distance of about 0·1.

The average distance between the solar lines in the green, which have not yet been identified, is about 4·4, or more than fourteen times the difference between the centre of the oxygen line and the corresponding solar line. The average distance between the non-identified lines near O α is 4·9, or about twenty-nine times the corresponding difference. In judging, however, of the value of the evidence, I should like the reader to leave the line δ out of account. Although the agreement seems perfect, I have not the same confidence in the correctness of the wave-length as I have with the other lines. The line β is weaker than the others, and the error of observation may be a little larger than with α and γ, which will, I think, be found correct to the decimal place.

Let me point out in a few words the importance of the results obtained. The compound line spectrum of oxygen can only exist under a limited range of physical conditions. It is broken up at a higher temperature into the elementary line spectrum, and at a lower temperature it tumbles together into a continuous spectrum. During its existence its lines may be subject to variations owing to pressure. The spectrum of oxygen is therefore pre-eminently fitted to be at once the pressure gauge and thermometer of the sun. We cannot at the present moment give the exact temperature of the points at which the changes take place; but we can say with certainty why it is that the line spectra of many metalloids are not found reversed in the sun, for the temperature which gives these line spectra is higher than that which gives the compound line spectrum of oxygen, and therefore higher than that of the reversing layer of the sun. Consequently we must look for their band spectra and not for their line spectra. The same may be true for the spectra of some of the heavier elements like gold, silver, and platinum, which have not yet been discovered in the sun. The continuous spectrum of the base of the corona is most likely the continuous spectrum of the cooler oxygen.

As the science of spectroscopy advances we shall be able to determine the physical conditions which exist on the surface of the sun with as great a degree of certainty and a much smaller degree of discomfort than if we were placed there ourselves. I hope that this communication will prove to be a step in that direction. All my experiments were made in the Cavendish Laboratory.

ARTHUR SCHUSTER

St. John's College, Cambridge, November 30

OUR ASTRONOMICAL COLUMN

JUPITER'S SATELLITES.—Amongst the recorded phenomena connected with the motions of the satellites of Jupiter are several notices of observed occultations of one satellite by another, and of small stars by one or other of the satellites. The following cases may be mentioned:—On the night of November 1, 1693, Christoph Arnold, of Sommerfeld, near Leipsic, observed an occultation of the second satellite by the third at 10h. 47m. apparent time. On October 30, 1822, Luthmer, of Hanover, witnessed an occultation of the fourth satellite by the third at 6h. 55m. mean time.

Flaugergues, writing to Baron de Zach, from Viviers, on November 18, 1821, says: "I begin with an observation, very useless, no doubt, but extremely rare, for I have not found a similar one in the collections of astronomical observations which I have examined; *i.e.*, the occultation of a very small star by the third satellite of Jupiter." He proceeds to mention that on August 14, 1821, he repaired to his observatory very early to observe an eclipse of this satellite, and having looked at Jupiter with the telescope, he remarked a very small star near the third satellite. The satellite approached this star, and at 1h. 47m. sidereal time, it appeared to touch it, and at 1h. 56m. 52s. it was not possible to distinguish the star—it had disappeared. The satellite became fainter and disappeared in its turn at 1h. 59m. 10s. sidereal time, on August 13, or 16h. 30m. 8·5s. mean time at Viviers. The sky was perfectly clear, and Flaugergues considered his observations very exact. He adds that he continued to observe for a long time after the immersion of the satellite, hoping to see the star reappear, but he could not again distinguish it; the twilight had much increased, and small stars in the neighbourhood of Jupiter were soon effaced.

There is a similar observation by Mr. G. W. Hough, at Cincinnati Observatory, communicated in a letter to Dr. Brunnow, when Director of the Observatory at Ann Arbor, Michigan, and published in his "Astronomical Notices" Mr. Hough states that on March 28, 1860, he witnessed the end of an expected occultation of a star 9·5 mag., by Jupiter, and the occultation of the same star by the first satellite. When first seen it was distant from the limb of the planet about one diameter of the satellite, or one second of arc, so that the real separation had taken place about six minutes before (or about 8h. 9m. sidereal time), though he was not able to see it. At 10h. 27m. sidereal time the star was occulted by the first satellite and remained invisible eight minutes. Mr. Hough further says that the star is found in the "Redhill Catalogue," an obvious oversight; it would appear to be No. 1630 of Zone + 22° in the *Durchmusterung*, a star of 9·3m. the approximate place of which for 1855 was in R.A. 7h. 8m. 5s., N.P.D. 67° 3'·3.

DONATI'S COMET OF 1858.—This comet which attained so great a celebrity in the autumn of 1858, makes a very close approximation to the orbit of Venus near the descending node, and it may be reasonably inferred that the actual form of its path round the sun may be due to a very near approach of the two bodies at some distant epoch. The discussion of the totality of observations was undertaken some years since by Dr. von Asten, who has published his results in a dissertation entitled "Determinatio orbitæ grandis cometæ anni 1858, e cunctis observationibus." The comet was discovered by Donati on June 2, and was observed until the beginning of March, 1859, at the Cape of Good Hope and at Santiago de Chile; consequently the observations extended over a very wide arc of the orbit, and there have been very few cases where careful discussion could be expected to lead to more reliable results. The period of revolution deduced by Dr. von Asten is 1,880 years, and there is a high probability that this does not differ materially from the true one, applying to the time of the comet's appearance. Prof. Hill, of Washington, also by a complete investigation, obtained a somewhat longer period, but the general character of the orbit remains the same. Employing Dr. von Asten's elements, it will be found that in heliocentric longitude 343°·7, the distance of the comet from the orbit of Venus, is only 0·0047 of the mean distance of the earth from the sun. In 1858 the two bodies came into pretty near proximity, their mutual distance on October 17 being 0·088. It has been mentioned above that the point of closest approach of the orbits of the planet and comet is situated near the descending node; the opposite node falls in the region of the minor planets.

THE OBSERVATORY OF LYONS.—The *Bulletin Hebdomadaire* of the French Scientific Association reports that M. André is actively employed in the establishment of this new astronomical institution and is energetically supported by the Government. M. Raphël Bischoffsheim, the munificent donor of the meridian circle, lately mounted at the Observatory of Paris, has also intimated his intention to present the Lyons Observatory with its fundamental instrument, a meridian-circle of dimensions but slightly inferior to those of the circle, for which the Observatory at Paris is indebted to him. It will also be constructed by Eichens. The Paris meridian-circle is intended to replace the instruments of Gambey, which are now placed in one of the saloons of the institution with other instruments which have seen their day. M. Wolf

remarks that the scientific zeal and liberality of M. Bischoffsheim "inaugurates in France a path long followed in England by wealthy amateurs of astronomy."

THE METEORITE OF JUNE 14, 1877.—M. Gruey has calculated the orbit of this meteorite with the assistance of the Observatory of the Puy-de-Dôme, and accounts obtained through the press of Clermont, where he observed it at 8h. 55m. P.M. local time. Observations made at Bordeaux and at Angoulême were combined with those at Clermont. He obtained for the velocity of the meteor relatively to the sun 93 kilometres in a second, in the direction – heliocentric longitude 15° 17′, latitude – 17° 3′, and neglecting the insignificant effect of the earth's attraction upon a velocity so great, and the unknown effect of atmospheric resistance, he found for the heliocentric motion of the meteor the following elements of a hyperbolic orbit. Eccentricity, 7·079, semi-axis, 0·137. Ascending node, 83° 49′, inclination, 18° 14′, perihelion from node, 286° 50′, longitude at appearance, 263° 49′; the meteor approaching its perihelion was thus distant 23° from it.

This adds another case to several previous ones in which hyperbolic orbits have been obtained for meteorites by Petit, Galle, Tissot, &c.

PROF. NEWCOMB.—At the meeting of the Royal Society on Thursday last, the distinguished mathematical astronomer, Prof. Simon Newcomb, of Washington, was elected one of its foreign members. There was previously on the list only a single American, viz., Prof. Asa Gray. Prof. Newcomb's important contributions to astronomical science will be admitted to have richly entitled him to an acknowledgment at the hands of our leading society.

CHEMICAL NOTES

MINERAL OIL IN A LAVA OF MOUNT ETNA.—In the basaltic zone which reaches from the foot of Mount Etna in a south-south-easterly direction, near the village of Paterno there is a prehistoric doleritic lava containing olivine, which surrounds the clay deposits of a mud volcano and which has been examined by Sig. Orazio Silvestri. Under the microscope the lava shows an augitic principal mass with a quantity of olivine and many white transparent crystals of labradorite. The lava contains numerous round or irregular cavities which are coated with arragonite and which are filled with mineral oil. This oil, of which there is about 1 per cent. by weight in the whole mass, was taken from one of the cavities at 24° C. At about 17° C. it begins to solidify and is of a yellowish green tint by transmitted light, while by reflected light it is opalescent and light green. Chemical analysis of the liquid proved it to contain :—

Liquid hydrocarbons (boiling point 79°·28) ...	17·97 per cent.
Hydrocarbons solidifying under 0° (b. p. 280°–400°)	31·95 ,, ,,
Paraffine, melting point 52°–57°	42·79 ,, ,,
Asphalt (leaving 12 per cent. of ashes) ...	2·90 ,, ,,
Sulphur	4·32 ,, ,,
	99·93

FORMATION OF CERTAIN BODIES AT TEMPERATURES ABOVE THAT OF THEIR DECOMPOSITION.—MM. Troost and Hautefeuille have lately demonstrated that under certain circumstances it is incorrect to suppose that bodies undergoing decomposition or rather dissociation at a low temperature may not exist as definite compounds at higher temperatures. Their arguments are founded on the decomposition of silicon sesquichloride (Si_2Cl_6) at 800°, which may be represented as—

$$2Si_2Cl_6 = 3SiCl_4 + Si,$$

if, however, the reaction be carried on at a temperature above 1200° the following change takes place—

$$3SiCl_4 + Si \quad 2Si_2Cl_6.$$

If the tube in which this reaction takes place be cooled suddenly the sesquichloride is found, but if cooled slowly it undergoes gradual decomposition. They also find that although ozone is converted into oxygen at 250°, if a silver tube inclosed in a porcelain tube be kept at about 1300° a deposit of dioxide of silver is produced due to the formation of ozone. They state that the ozone can be recognised by the usual tests if the gas be rapidly drawn off and quickly cooled. They have also examined certain similar phenomena in the production of oxide of silver at 1400°.

IODATES OF COBALT AND NICKEL.—Prof. F. W. Clarke describes these salts, which were prepared by dissolving the carbonates in aqueous iodic acid, and allowing the solution to evaporate spontaneously when salts of the composition $CoI_2O_6.6H_2O$ and $NiI_2O_6.6H_2O$ crystallise out. If the solution of the carbonate of cobalt in iodic acid is evaporated rapidly, then the iodate of Rammelsberg, containing $1\frac{1}{2}$ molecules of water may be obtained, but not otherwise. The cobalt iodate loses four molecules of water at 100°, but the remaining two molecules cannot be driven off without partial decomposition of the salt. The specific gravities of the two salts are almost identical, the cobalt iodate at 21° being 3·6893, the nickel iodate at 22° being 3·6954. No numbers of the solubilities of the two salts are given by Prof. Clarke, but these, when obtained, will be of some interest.

ORIGIN AND FORMATION OF BORACIC ACID.—M. Dieulafait (*Comp. Rend.* lxxxv. 605) finds that under certain conditions by spectrum analysis $\frac{25}{1000000}$ grammes of boron, and by the colour imparted to a hydrogen flame $\frac{1}{1000000}$ grammes may be detected. He considers boracic acid to be a normal constituent of sea-water and salt marshes lying above beds of carnallite. M. Dieulafait finds that this acid may be recognised in a drop of sea-water weighing about 0·0378 grammes, and that the minimum quantity found in the Mediterranean is two decigrammes per cubic metre of water. He arrives by geological reasoning at conclusions differing from those of Dumas and others with regard to the origin of this body in the lagoons of Tuscany, and thinks that the source of boracic acid in this district may be found in a relatively modern formation.

NEW MODES OF FORMING ETHYLEN OXIDE.—In the *Comptes Rendus*, lxxxv. 624, Mr. H. Greene mentions the results of experiments on the action of certain metallic oxides on the bromide, iodide, and chloriodide of ethylene. Oxide of silver has a rapid action on ethylen iodide at a temperature of 150°, forming ethylen oxide; its action on ethylen bromide produces the same result but requires a higher temperature. Ethylen bromide and chloriodide both act upon sodium oxide at 180°, the latter of the ethylene compounds being the one found most advantageous by the author in preparing ethylen oxide. He has also studied the action of these substances on the oxides of the diatomic metals barium and lead. These oxides do not give ethylen oxide when heated with bromide or chloriodide of ethylene. These experiments show, on the one hand, the analogy between the silver and sodium oxides confirmed by the isomorphism of their anhydrous sulphates and chlorides, and on the other their difference from the group of diatomic oxides.

THE ACTION OF CERTAIN ANTISEPTIC VAPOURS ON THE RIPENING OF FRUITS.—MM. Lechartier and Bellamy give an account in the *Comptes Rendus*, lxxxiv. 1,035, of some experiments they made on the fermentation of apples when inclosed in vapours such as carbolic acid, camphor, and potassium cyanide. From their results it appears that no fermenting action took place in the apples surrounded by vapour of carbolic or hydrocyanic acids, and a slight action only in the one surrounded by camphor vapour. The camphor vapour, in fact, diminishes without entirely destroying the vitality of the cells. In this journal, also, there is an account of

experiments performed in the same direction by M. Gayon. He incloses the apples in vapours of chloroform, ether, and carbon disulphide, and his results agree with those of the first observers. The chloroform and ether act in the same manner as the carbolic and hydrocyanic acids; the carbon disulphide in the same way as camphor, permitting partial fermentation only.

A PROBLEM IN CHEMICAL AFFINITY.—In his work on "Gasometric Methods," Prof. Bunsen details an interesting series of experiments on the phenomena accompanying the explosion of hydrogen and carbon monoxide with a volume of oxygen insufficient for its complete combustion. From the results he deduces the conclusion that the ratio between the products of combustion ($H_2O : CO_2$) can always be expressed by *small whole* numbers (1 : 2, 1 : 3, 1 : 4, &c., and that it alters suddenly from one figure to the next by gradually increasing the amount of hydrogen. Deeming the nine experiments upon which the conclusion was based as insufficient for the establishment of a general principle, Prof. Horstmann, of Heidelberg, describes in the *Verhandlungen des heidelb. naturf. med. Vereins*, an extensive series of observations designed to test the truth of the law. Among his results the following facts are of interest. In exploding CO with gradually increasing quantities of $H_2 + O$, while the ratio between H and CO increased from 0·25 : 1 to 2·33 : 1, the ratio between H_2O and CO_2 gradually increased from 0·8 : 1 to 4·5 : 1, with no evidence of a predilection for rational numbers. Experiments on a mixture of CO and H, with gradually increasing amounts of O, led to the same results, showing no such regularity in the division of O between the two combustible gases as Bunsen's law would indicate. When aqueous vapour is present in the mixture less H and more CO unites with O, while the presence of CO_2 reverses the case. By gradually increasing the amount of O in the explosive mixture, it was noticed that the ratio between the resultant H_2O and CO_2 increased until it attained a maximum, when 35 per cent. of the combustible gases were oxidised, and then sank regularly to the ratio denoting complete combustion. The oxygen appears to be divided among the two gases according to the following law:—The ratio between the resultant H_2O and CO_2 is equal to the ratio between the residual H and CO multiplied by a co-efficient of affinity which is independent of the ratio between the combustible gases but dependent on the relative quantity of O present. This co-efficient of affinity varied between 4 and 6·4, showing that always more H relatively than CO is consumed, and hence that the affinity of O to H is greater than that to CO.

HALOGEN DERIVATIVES OF AMINES.—An attempt has frequently been made by chemists to replace the hydrogen in the hydrocarbon group present in amines, by Cl, Br, or I. These efforts have hitherto resulted simply in the substitution of the basic H atoms of the amine by halogens—as $C_2H_5.NCl_2$—or in complete decomposition. A. Michael (*Berl. Ber.*, x., 1644) has devised a method for accomplishing this end, which consists in first replacing these basic H atoms by acid residues, and then exposing to the action of a halogen ethyl-phthalimide,

$$C_6H_4(CO)_2N.C_2H_5,$$

yields in this way with Br a tribromo-ethyl-phthalimide.

DOUBLE SALTS WITH CYANIDE OF GOLD.—C. G. Lindbom publishes in the *Univ. Arsskrift* of Lund an exhaustive account of these compounds, which may be regarded as salts of the two acids, HCy.CyAu and $HCy.CyAu.Cy_2 + 1\frac{1}{2}$aq., neither of which, however, can be obtained pure for analysis on account of their tendency to decompose. Most of the auro salts unite directly with a molecule of the halogens; for example aurocyanide of sodium, $NaCy_2Au$, forms bromo-auricyanide of sodium, $NaCy_2AuBr_2 + 2$aq. Aurocyanide of ammonium, $AmCy_2Au$, is decomposed at 100°.

THE FOURTH NITROBENZOIC ACID.—Prof. F. Fittica has discovered lately a new nitro-benzoic acid, making the fourth of the isomeric acids, which has been contested by other chemists, especially as it fails altogether to harmonise with the theories at present accepted in regard to the structure of benzene derivatives. In the October session of the Deutsche chemische Gesellschaft, he strengthens his position by announcing the discovery of a fourth nitro-benzaldehyde, obtained by the action of H_2SO_4 on benzaldehyde and ethylic nitrate, which on oxidation is changed into the new nitrobenzoic acid, $C_6H_4.NO_2.COOH$.

INFLUENCE OF ISOMERISM ON THE FORMATION OF ETHERS BETWEEN ACIDS AND ALCOHOLS.—In the September session of the Russian Chemical Society, Prof. H. N. Menschutkin presented an elaborate paper on this subject based on observations of the formation of acetic ethers. The process consisted in inclosing molecular weights of an alcohol and acetic acid in glass tubes, immersing it in a glycerine bath at 154° for a certain time, and then rapidly cooling it, and titrating the unaffected acetic acid with baryta water. The results show that in regard to the rapidity and limits of etherification, the primary alcohols are sharply divided from the secondary, and the latter from the tertiary; and the saturated alcohols from the non-saturated. A regular decrease in the rapidity coincides with an increase in the molecular weight of the alcohol. As in many other series of experiments, methylic alcohol shows considerable deviations from the laws governing its higher homologues. In the case of non-saturated alcohols the rapidity is less than that of the corresponding primary alcohols, but greater than that of the corresponding secondary alcohols.

PHOSPHIDES OF TIN.—Since the introduction of phosphorus bronze, the compounds of phosphorus and the metals are receiving more attention. S. Natanson and G. Vortmenn describe (*Berl., Ber.*, x. 1459), several methods of preparing phosphides of tin, viz., throwing P on molten tin, melting a mixture of vitreous phosphoric acid, charcoal, and tin, and passing phosphorus vapours over molten tin in a hydrogen stream. These processes all yield a crystalline silvery white compound, containing from $1\frac{1}{2}$ to 3 per cent. of P, and leaving on treatment with HKO a residue of pure SnP.

CHEMICAL ACTION OF LIGHT.—In a late number of the *Annales de Chimie et Physique*, M. Chastaing advances, in connection with a variety of observations on this topic, the theory that the chemical action of the various coloured rays on inorganic substances is dependent on refrangibility, blue and violet acting as reducing agents, red and yellow causing oxidation. Prof. H. W. Vogel attacks this opinion vigorously in the last session of the German Chemical Society, claiming that the nature of the substance causes the action to be one of reduction or oxidation. The union of H and Cl, which takes place so rapidly in violet light, is regarded as purely analogous to oxidation, and he alludes to Timiriazeff's late experiments, showing that the reduction of CO_2 by plants, proceeds more rapidly in red light than in green.

NOTES

AT the meeting of the Royal Society, on Thursday last, the *Times* states, the following were elected foreign members:—Marcellin Berthelot, of Paris; Joseph Decaisne, of Paris; Emil Dubois Reymond, of Berlin; Adolph Wilhelm Hermann Kolbe, of Leipsic; Rudolph Leuckart, of Leipsic; Simon Newcomb, of Washington; and Pafnutij Tschebytschow, of St. Petersburg. By this election the foreign list of the society is made up to its full complement of fifty members.

MR. ALEXANDER AGASSIZ, it is understood, proposes to spend the winter in the prosecution of scientific research in the Florida

seas, and will carry a line of dredgings and trawlings from Key West to Yucatan. Bearing in mind the very great success that has been experienced by the use of steel wire in taking soundings, he proposes to try the experiment of a steel rope $1\frac{1}{16}$ inches in diameter in the work of dredging and trawling. This, he thinks, will reduce the friction to such an extent as to greatly diminish the time and power necessary in making a cast of the dredge.

THE Emperor of Russia has conferred the order of St. Anne on Mr. Carl Bock, F.G.S.

THE *Monthly Microscopical Journal* expires with the number just issued for the last two months. It was edited from the commencement by Dr. Henry Lawson—who, after a long period of failing health, died on October 4 last—and has been in existence for nine years. Many valuable papers are contained in it, by distinguished authors, including the *Proceedings* of the Royal Microscopical Society, which will in future be published independently.

WITH reference to the brilliant meteor of December 6, we learn from Capt. Tupman that it will take him some time to determine the most probable path from the immense number of observations, good, bad, and indifferent, sent to him. Meantime he thinks that Prof. Herschel's preliminary calculation, not yet published, that it began fifty-three miles over Wigan, and burst thirty-three miles over a point half way between Great Orme's Head and Douglas in Man, with radiant $78° + 6°$ (γ Orionis), agrees better with the observations than any other path. We hope to publish Capt. Tupman's conclusions when his calculations are completed.

THE subject of Prof. Tyndall's six Christmas lectures to juveniles is to be Heat, Visible and Invisible. They commence on Thursday week.

MM. FEIL and FREMY, at last week's meeting of the Paris Academy of Sciences, read a paper describing a new process for the manufacture of rubies and other precious stones. The sensation created by these wonderful experiments has been so general that the Association of Jewellers have written to some of the papers stating that it was impossible for human art to compete against nature, that mysterious maker having at her disposal an indefinite number of centuries, which is not the case with any human worker. M. Daubrée, the Director of the School of Mines, has expressed the wish to open, in the public museum of that magnificent establishment, a gallery for the exhibition of minerals produced artificially. M. Feil has already produced in his glass foundry, and by the same process as rubies, an immense number of stones which can be compared with the most admirable crystalline productions of nature. Some of them are so inexpensive that they may be used for ordinary decorative purposes.

AN extraordinary but happily unsuccessful attempt was recently made upon the life of Mr. Russell, the Government Astronomer at Sydney, New South Wales. On September 8 a lad of about nineteen years of age left a box at the observatory for Mr. Russell, who, under the impression that it contained instruments of some kind, proceeded to open it. He found the lid a sliding one, similar to those adapted to ordinary instrument cases, and he had not drawn it far when he discovered that the affair partook more of the character of an infernal machine than anything else. The movement of the lid became rather stiff, and upon inspecting it and the box a little more closely he discovered at one end of the latter several grains of powder. The box was then taken into the open air, where it was investigated with special care. The lid was released, and there were found in the box at least 4½ lbs. of blasting-powder. In it were no less than sixteen matches, stuck with their sulphurous points in dangerous proximity to a sheet of sand-paper fastened to the under-side of the lid, the design being evidently to cause an explosion by the friction of the sand-paper against the matches; and there can be little doubt that this would have been effected had not great care been exercised in handling the affair. Besides the matches and powder, dangerous enough in themselves, a ginger-beer bottle, filled with gunpowder, and evidently intended to act as a shell, was found in the box; Mr. Russell has expressed his belief that altogether there was a sufficient quantity of explosive material present in the box not only to destroy life, but to blow the building down. One of the workmen at the observatory was arrested on suspicion.

THE first number is announced to appear on January 3 of a new weekly *Revue Internationale des Sciences*, under the editorship of Dr. De Lanessan, Professor of Natural History in the Medical Faculty of Paris. The publisher is Doin, of the Place de l'Odéon, Paris. Among the *collaborateurs* are several well-known names in France and Germany, England being represented by Mr. Francis Darwin.

THE expected change has taken place in the French Ministry. M. Faye has resumed his place as one of the Inspectors of Public Instruction, and Member of the Bureau des Longitudes. M. Bardoux, one of the most able members of the republican party, has been appointed Minister of Public Instruction. M. Bardoux is the President of the General Council of Puy de Dôme, who constructed, at the expense of the department, the observatory built on the top of the mountain of the same name.

M. BARDOUX is preparing a bill granting to the rectors of the several French academies (there is one in each of the eighty-two departments) the right to appoint the teachers in the public schools. Up to the present time these nominations were made by the prefects and too often the choice was influenced by political considerations.

THE enlarged council of the Paris Observatory held last Saturday a very interesting meeting. M. Faye has not resumed his seat as councillor. Several reclamations were read against the resolutions which had been adopted in the previous sitting. One of them was on behalf of the Bureau des Longitudes, asking to be allowed to have a voice in the presentation of the Director of the Observatory, as well as the Council and the Academy of Sciences. From the foundation of the Bureau des Longitudes up to 1854, when M. Leverrier was appointed director for life by Napoleon III., the Bureau des Longitudes had the control of the observatory. Each year the Bureau appointed one of its members to superintend the observations, and the custom was to reappoint the same member up to his death. Arago thus held his office by yearly tenure for more than a quarter of a century. The discussion of meteorological matters was begun, and the meeting adjourned till to-day. No formal proposition will be made to sever the International Bureau from the Observatory, the aim of certain members being confined to the establishment of a Central Board for Meteorology, which will give its advice on the organisation of the International Bureau, the Montsouris Central Observatory, the Puy de Dôme, the Pic du Midi, and any other establishment which may be founded for meteorological purposes.

WE are happy to state that the rumour widely spread in Paris of the death of Drouyn de Lhuys is unfounded, the learned gentleman having recovered, against almost all hopes. He will very likely resume his place in the several scientific societies which he had resigned.

M. MILNE EDWARDS has been appointed president of the French Scientific Association, which was founded by M. Leverrier thirteen years ago. Under the direction of M. Leverrier the association spent not less than 250,000 francs for scientific purposes, and has accumulated a sum of about 400,000

francs. The association is supported solely by voluntary contributions, and meets yearly at Paris. Many improvements are contemplated by the new president.

A CHIMPANZEE, about $2\frac{1}{2}$ years old, has been recently placed on view at the Westminster Aquarium by Mr. Farini. It is very gentle in disposition, and is undergoing an education in the usages of civilised life at the hands of its keeper, Mr. Zack Coup. For some time it has been in one of the private houses at the Zoological Gardens, and there it caught cold. On its removal to the room at the Aquarium, where a temperature of about 70° is maintained, it improved, but the fog of yesterday (Wednesday) seemed to oppress it very much. It is suffering both in head and lungs, though it still struggles very successfully to be cheerful and entertaining. It is curious that Pongo's lungs were found all sound, though the few chimpanzees that have been exhibited in Europe have succumbed to lung disease. With the chimpanzee are also a very fine cynocephalus, a "sacred" monkey from India, and a number of monkeys less rare. There are close by a python, a l oa, and two anacondas, and in order to insure that they hall not be hidden in rugs when visitors want to see them, they are at intervals exhibited by an Abyssinian girl, who goes through the ceremony of an incantation each time.

IN his introductory address at the opening meeting of the Royal Society of Edinburgh Sir Alexander Grant stated that the society was an emanation from the University of Edinburgh, from which it sprang on the suggestion of Principal Robertson in the latter part of 1782. Thus, in the same year that the University would celebrate its tercentenary the society would be able, perhaps conjointly, to celebrate its 100th birthday. In one essential particular it differed from the Royal Society of London. From the first the promotion of literature as well as science was the object of the Royal Society of Edinburgh. But it had been observed that the literary element in their proceedings had been gradually dwindling away. Sir Alexander had inquired as to the number of papers not connected with philosophical science which had been contributed during the last fifteen years, and it appeared to be considerably less than forty, or little more than two per annum. In the last fifteen years, out ot about 370 ordinary Fellows of the Society, only about twenty had come forward to contribute papers other than philosophical or mathematical. The Council of the Society have awarded the Macdougall-Brisbane prize, consisting of a gold medal and 15*l.* 14*s.* 7*d.*, to Mr. Alex. Buchan, for his paper on "The Diurnal Oscillations of the Barometer." Prof. Balfour reported that the membership of the Society at present was 427, consisting of 373 ordinary and fifty-four honorary or non-resident Fellows.

ADVICES from the Howgate Arctic Expedition have been received up to the date of October 2, at which time the vessel had reached Niantitik Harbour, in Cumberland Gulf. The passage of forty-three days from New London had been very stormy, but, as far as reported, without any disaster. Mr. L. Kumlien, the naturalist of the party, had gathered some specimens, but did not find the promise of suitable collecting ground in the immediate vicinity very good. He hoped, however, to change his quarters to a better location.

WE learn from the *Isvestia* of the Russian Geographical Society, that at the beginning of September M. Prshevalsky had already started for Tibet. He is accompanied by an aid, M. Ecklon, four cossacks, and two soldiers. Having arrived at the conclusion that it will be impossible to reach Tibet by way of Lake Lob-Nor, he will try the route through Guchen and Hami, and thence to Zaidam and the upper parts of the Blue River. He expects to be at Lassa next year, about May or June, and if he succeeds, he will remain in Tibet for a year.

AFTER having penetrated last year for 160 miles up the Amu Daria, the Russian steamer *Samarkand* has penetrated this year as far as the fortified town Chardjui. A complete survey ot the river was made, and considerable botanical and zoological collections were brought back by the officers on board.

RUSSIAN newspapers announce that the *Morning Dawn* reached St. Petersburg on December 1, having left the mouth of the Yenissei on August 21. This ship, or rather boat, 56 feet long, 14 feet wide, and drawing only 6 feet water, was built for the transport of wares up the Yenissei from Kureïka. It was never intended to go to sea, and "never," Prof. Nordenskjöld says, "so wretched a boat dared to enter the waters of the Arctic Ocean." Nevertheless Capt. Swanenberg, with a crew of four men, safely crossed on board this boat the Kara Sea, and reached the Russian capital after a hundred days' cruise. With a compass almost useless because of the deviation occasioned by the iron on board, and struggling with ice, he reached, on August 30, the Kara strait, where he experienced a heavy gale. On September 11 he was at Vardö. Thence, after a fortnight's stay, the *Morning Dawn* went in tow of a Norwegian steamer to Christiania, and further, in the same manner, to Göteborg, which was reached on November 3, and to Helsingfors, and finally to St. Petersburg The reception the gallant crew met with in the Norwegian towns was everywhere the most enthusiastic.

AT the last meeting, December 7, of the Russian Geographical Society, Col. Tillo read a report on the magnetical measurements made by M. Smirnoff in Russia. These measurements, accomplished with the utmost accuracy, embrace no less than 548 places, the declinations and inclinations having been measured at 287 places, and the former alone at 261. At the same meeting the Society resolved to enter into relations with different governments in reference to the establishment of polar meteorological stations, and to submit an elaborate scheme in connection with that subject to the next International Meteorological Congress.

GERMANY is still waging war against the illegal use of the doctor's title. A "Dr." Harmuth in Berlin who received his diploma from Philadelphia, was lately sentenced to pay 300 marks for using the prefix publicly.

M. POLYAKOFF, who was sent by the St. Petersburg Academy of Sciences for the exploration of the mammoth remains in the Government of Tomsk, has now returned to St. Petersburg after having made a journey in the Western Altai, the Kirghiz Steppe, and in the Seven Rivers' Province, where he visited the lakes Alakul and Balkash. He brings back very rich collections of animals and plants, and the results of his varied observations will appear in the *Memoirs* of the Academy.

THE scheme for telegraphing without wires, the *New York Tribune* states, by means of aërial currents of electricity, has been revived by Prof. Loomis. He has met with success in using kites for this purpose, a copper wire being substituted for the usual kite string. Signals were transmitted thus between kites ten miles apart. His new experiments are made in the mountainous regions of West Virginia, between lofty peaks. Continuous aërial currents are found at these altitudes, which will serve the purposes of the telegraph, except when rarely interrupted by violent disturbances of the atmosphere. A scheme is now on foot to test the merits of aërial telegraphy in the Alps.

THE *Journal of Forestry*, which started in the month of May last, in the interest of forest conservancy and management generally, maintains the reputation which the early numbers indicated. In recent numbers, Prof. Boulger, F.L.S., has contributed some papers, which are being continued, on "Cultural First Principles," in which he considers (1) "climate," (2) the "nature of the soil," and (3) "theoretical considerations (as to the treatment of the soil." In this latter portion of the subject drainage forms of course no inconsiderable part. Sloping plantations, it is shown, will seldom require artificial drainage,

for such a situation on high soils is quite suited for conifers, and if on heavy soils sufficient for oak and other hard-wooded trees. Mr. Boulger points out, what ought to be apparent to all, that the growth of rushes, the wood rush (*Luzula*), the bog asphodel (*Narthecium*), a yellow star-like plant, the tufted hair-grass (*Aira cæspitosa*), or of mosses on the surface, are sure indications of the absolute necessity of drainage. Though these are facts with which a practical forester is well acquainted, it is nevertheless necessary to impress them upon the minds of all interested in forest produce. Some useful hints as to the preservation of timber for fencing, or in damp underground situations, are given in the number of the *Journal* for the current month.

R. PICTET describes some interesting experiments (*Arch. Sc. Phys.*, lix.) made for the purpose of determining the conditions under which transparent or non-transparent ice is formed. It was ascertained that water frozen in a vessel dipped in a cold glycerine solution formed perfectly transparent ice as long as the temperature ranged between 0° and − 1·5°. If the solution was cooled below − 3°, the ice was whitish and of a less specific gravity, these properties being intensified with the lowering of the temperature. No difference in the melting-point or amount of warmth required for melting was observable among the various varieties of ice. The opacity of ice results from an irregular arrangement of the ice-crystals, as well as from the presence of small bubbles of air—less than ⅓ mm. in diameter—which are mechanically inclosed. They can be removed by slowly conducting through freezing water large bubbles of air which carry with them the small bubbles.

AT a public meeting held in the Cheltenham Masonic Hall on December 15, Sir F. Abbott in the chair, it was resolved to institute a "Cheltenham Philosophic Society," which should hold its meetings during the winter months. Upwards of fifty gentlemen signified their desire to become members, and a committee was formed to draw up rules to be submitted at a future date to a general meeting for their sanction.

DR. JOHN RAE asks us to state that in his paper on Eskimo skulls read at the Anthropological Institute on May 8 last, and published in the newly-issued number of the *Journal* of the Institute, he by mistake called the Western Eskimo Brachycephalic, whereas they are Mesocephalic.

IN the letter on the meteor last week, p. 124, "8 (± 2) χ Lyræ," should be 8 (± 2) times Lyræ.

THE additions to the Zoological Society's Gardens during the past week include a pair of Musk Deer (*Moschus moschiferus*), from Central Asia, presented by Sir Richard Pollock; a pair of Axis Deer (*Cervus axis*), from India, presented by Dr. Carl Siemens; a Diana Monkey (*Cercopithecus diana*), from West Africa, presented by Mr. P. Spink; a Bonnet Monkey (*Macacus radiatus*), from India, presented by Mr. T. H. Evans; a Patas Monkey (*Cercopithecus ruber*), from West Africa, a Red-backed Saki (*Pithecia chiropotes*), a Red-faced Spider Monkey (*Ateles paniscus*), two Kinkajous (*Cercoleptes caudivolvulus*), a Coati (*Nasua nasica*), an Azara's Fox (*Canis azaræ*), a Black Vulture (*Cathartes atratus*), from South America, deposited two Schlegel's Doves (*Chalcopelia puella*), from West Africa, two Lesser Razor-billed Curassows (*Mitua tormentosa*), from South America; two Waxwings (*Ampelis garrulus*), four Bullfinches (*Pyrrhula rubicilla*), European, purchased; four Common Waxbills (*Estrelda cinerea*), two Cinereous Waxbills (*Estrelda cærulescens*), six Orange-cheeked Waxbills (*Estrelda melpoda*), two African Silver Bills (*Munia cantans*) seven Yellow-rumped Seed Eaters (*Crithagra chrysopyga*), from West Africa. received in exchange; a Chinchilla (*Chinchilla lanigera*), born in the Gardens.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—The Syndicate, appointed in May, 1875, to consider the requirements of the university in different departments of study, have just issued their fourth report on the subject. After stating that in their opinion the inter-collegiate system should be further stimulated and supplemented by the institution of university readerships, and that by a more complete organisation the requisite provision for teaching and the encouragement of research might be to a considerable extent made, they nevertheless are of opinion that certain subjects of great importance are not now represented in the professoriate, the absence of which from such representation constitutes a serious defect in the Cambridge system, and they recommend that professorships amongst other subjects should be created as soon as the resources of the university permit, in comparative philology, mental philosophy and logic, physiology, English language and literature.

The Syndicate, with regard to existing professorships, suggest that the Professorship of Mineralogy should be for the future a Professorship of Mineralogy and Mineral Chemistry.

There are other subjects of scarcely less importance of which there ought always to be recognised teachers in the University, although the Syndicate are not prepared to say that the teacher ought always to have the status of a professor. Such are analytical chemistry, morphological and physiological botany (as distinguished from systematic botany), comparative anatomy (as distinguished from zoology), pathological anatomy.

In other subjects, again, it is desirable that the University, without establishing permanent offices, should have the means of appointing professors or other teachers from time to time when there is the opportunity of securing the services of a specially competent person. Such are the theory and history of education, as also some special departments of natural science.

The following is the Natural Science Tripos' list for this year:—Class I.—(2) Ds. Bower, Trinity; (4) Cullen, Christ's; (1) Fenton, Christ's; (1) Greaves, Christ's; (2 and 3) Hill, Downing; (1) Ohm, Emmanuel; (3) Sedgwick, Trinity. Class II.—Ds. Harrison, Christ's; Holthouse, Trinity; Houghton, St. John's; Murton, St. John's; North, Sidney; Taylor, E. F. Vinter, Sidney. Class III.—Ds. Allen, St. John's; Buckmaster, Downing; Foster, Trinity; Wallis, St. John's; Weldon, Caius. The numbers indicate the subjects for knowledge of which the candidates are placed in the first class as follows:—1. Physics, chemistry, and mineralogy. 2. Botany. 3. Zoology and comparative anatomy, human anatomy, and physiology. 4. Geology.

EDINBURGH.—The second meeting of the session of the Edinburgh University Chemical Society, was held on the 12th instant, John Gibson, Ph.D., F.R.S.E., vice-president, in the chair. J. S. Thomson communicated a paper on solutions of litmus, in which he explained the preservation of the colour of such solutions on exposure to the air by the action of the air preventing fermentation. He also read a paper on the Determination of Melting Points.

TECHNICAL UNIVERSITY.—It may be remembered that a Committee of the City Companies has been for some time at work elaborating a scheme for founding a technical university in London. The last step taken by the Committee was to procure reports and suggestions from six specially nominated referees, viz., Prof. Huxley, Col. Donnelly (of the Science and Art Department), Capt. Douglas Galton, Mr. H. T. Wood (Assistant-Secretary of the Society of Arts), and Mr. Bartley (of the Science and Art Department). After having decided on these names, the Committee adjourned to allow time for the preparation of the reports. On Thursday last week they met again, and the reports were laid before them. After some discussion the further consideration of the subject was adjourned till January 17.

TAUNTON COLLEGE SCHOOL.—The Science Scholarship at Keble College, Oxford, is awarded to Mr. R. G. Durrant, of the Taunton College School. This is the fourth brilliant success that the school has gained in the last five months, and it is probably the last. The able science master, Mr. Shenstone, leaves at Christmas to take a Science Mastership at the revived Exeter Grammar School. He will not be replaced at Taunton, and the science teaching, which, after years of toil against obstacles, is just beginning to bear fruit, will become a thing of the past.

AUSTRIA.—The Austrian Government has for a number of years been accustomed to bestow liberal grants to the more promising students in the universities, under the condition that the recipients shall make use of them to undertake a course of study in the German universities. The results of this plan seem to be satisfactory, for we notice that this winter an unusually large number of students in all branches have been sent to the various universities in Germany.

A BERLIN POLYTECHNIC.—Berlin, with all her numerous educational establishments, has lacked hitherto a polytechnic such as is to be found in most of the German industrial centres at the present day. This want will soon be repaired, a commission having completed the plans for an extensive institution which will embrace nearly every branch of technical education. The plans for the necessary buildings have already been prepared, and as there is but little doubt that the Prussian Chamber of Deputies will grant the 9,300,000 marks required, the work of erection will commence next spring. On account of the extensive character of the proposed edifices, five years will be required for completion.

SOCIETIES AND ACADEMIES

LONDON

Mathematical Society, December 13.—C. W. Merrifield, F.R.S., vice-president, in the chair.—The Rev. W. Ellis was elected a member.—Mr. S. Roberts read a paper on normals, which contained theorems depending on the invariants and covariants of the quartic equation representing a pencil of four normals to a conic, and drew attention to the remarkable cubic locus of the points of possible concurrence of these normals at the vertices of a given inscribed triangle.—Dr. Hirst and Mr. J. J. Walker spoke on the subject. Prof. Cayley, F.R.S., read a paper on "the geometrical representation of imaginary quantities and the real (m, n) correspondence of two planes."

Linnean Society, November 15.—Dr. Gwyn Jeffreys, F.R.S., vice-president, in the chair.—Mr. J. Jenner Weir exhibited a case of Alpine butterflies, interesting for their similarity to, though not specifically identical with, those obtained by the naturalists of the Polar Expedition.—Three papers on the Arctic fauna followed. I. Report on the Insecta including Arachnida, collected by Capt. Feilden and Mr. Hart during the recent Arctic expedition, by R. McLachlan. It seems there were obtained of Hymenoptera 5, Coleoptera 1, Lepidoptera 13, Diptera 15, Hemiptera 1, Mallophaga 7, Collembola 3, Araneida 6, and of Acarida 6 species, namely, a total of 57 species. Bearing in mind these are from localities between 78° and 83° N. lat., that among them are thirty-five specimens of gaily-coloured butterflies and two species of humble-bees, and it becomes evident the insect-fauna of this so-called "land of desolation" is, after all, not so meagre as anticipated. The paucity of beetles and abundance of butterflies are each striking features. From variations in certain well-known species obtained, Mr. McLachlan suspects they represent a local fauna, and he regards the latter as having affinity to the Lapland fauna, inclining to think both are but lingering remnants of a once former and extensive circumpolar fauna.—II. Preliminary notice on the surface fauna of the Arctic Seas as observed in the recent Arctic expedition, by Dr. Edw. L. Moss (late surgeon, H.M.S. *Alert*). The author observes that the seas north of the Greenland settlements are subject to such varying conditions at different seasons of the year that their surface-fauna cannot be supposed to be very constant. Nevertheless, judging from what fell under his observation during the voyage, he divides the watery area into three zoological regions: (*a*) A district in the latitude of Melville Bay, temporarily monopolised by infusoria, *Peridinea*: (*b*) a north-water region inhabited by Pteropods, Tunicates, and Hydrozoa; and (*c*) a sub-glacial region comparatively lifeless, so far as sea-surface implies.—III. On the annelids of the British North Polar Expedition (1875–76), by Dr. W. C. McIntosh. This collection, dredged between 70° and 82° N., was not so rich in numbers or species as that procured by the storeship *Valorous* in Davis Straits, but some eight species were got which were not among the latter's collection. None are new, but notwithstanding they help to render clear some points in the geographical distribution of the marine worms, so far as the circumpolar area is concerned.—Dr. H. Trimen exhibited specimens of the Olibanum, or Frankincense tree (*Boswellia carterii*, Birdw.), gathered by Mr. J. Collins from the trees planted at Aden. Dr. Trimen, in making some remarks on the variability of the foliage of the species of *Boswellia*, expressed the opinion that *B. Bhau-Dajiania*, Birdw., was not specifically separable from *B. Carterii*. *B. Frereana*, which yields the fragrant resin called "Luban Metyi," and which Hanbury considered to be the African "Elemi," is much chewed by Orientals, but rarely imported into England. It is found in the Somali land, where Hildebrandt recently collected it.—The following gentlemen were elected Fellows of the Society: Mr. W. S. Lawson, Mr. W. Joshua, and the Rev. M. A. Mactherd.

Geological Society, December 5.—Prof. P. Martin Duncan, F.R.S., president, in the chair.—Dr. Isaac Bayley Balfour, David Burns, Samuel Cooke, Henry Drummond, Sandford Fleming, Rev. John Hodgson, William Etheldred Jennings, Henry Merryweather, Robert Robinson, Martin Stewart, George Eastlake Thoms, Robert F. Tomes, and Irwine J. Whitty, were elected Fellows of the Society.—A portrait of Mr. J. Evans, D.C.L., F.R.S., V.P.G.S., was presented by the President.—The following communications were read:—On the building-up of the White Sinter Terraces of Roto-Màhànà, New Zealand, by the Rev. Richard Abbay, M.A., F.G.S.—Additional notes on the Dimetian and Pebidian Rocks of Pembrokeshire, by Henry Hicks, F.G.S. The additional facts communicated by the author show that at a distance of about ten miles to the east of the Dimetian axis of St. David's there is another ridge of these rocks, which also runs nearly parallel with it. This is also flanked by Pebidian and Cambrian rocks, and made up of rocks like those in the St. David's axis. The Dimetian formation, so far as it is at present known, consists chiefly of the following rocks:—1. Quartz porphyries, containing frequently perfect quartz crystals (double pyramids), subangular masses of quartz, and crystals of felspar in a felspathic matrix. 2. Fine-grained greyish quartz-rocks, very compact, and interstratified with the above. 3. Ashy-looking shales of a dull green colour, sometimes highly indurated, but usually showing lines of lamination. Microscopically these show basaltic characters, and are probably greatly altered interbedded basaltic lavas. 4. Compact granitic-looking rocks. 5. Quartziferous breccias. 6. A series of compact quartzites and crystalline schists, interstratified by green and purple altered basaltic lavas, with a slaty and schistose foliation, and by some dolomitic bands. Of the *Pebidian* formation new areas were added, and the portions described in the author's previous paper were further extended, and details as to the chief mineralogical characters added. At the base of the series resting unconformably on the Dimetian is seen an agglomerate composed of large angular masses of a spherulitic felstone, pieces of quartz and quartzites, indurated shales, crystalline schists, &c., cemented together by a sea-green matrix of felstone. These are followed by conglomerates of the same materials, which are again succeeded by indurated shales, often highly porcellanitic in character, with a conchoidal fracture. These are followed by a thick series of silvery white and purplish shales and green slates, alternating with fine and rough ashes, often conglomeritic, hornstone breccias, felstone lavas, &c. The series, as exhibited at St. David's, has a thickness of over 8,000 feet; and as it is everywhere, so far as yet seen, overlapped unconformably by the Cambrians, it may probably be of much greater thickness. It evidently consists very largely of volcanic materials, at first derived from subaërial, but afterwards from submarine, volcanoes. These materials, however, were also undoubtedly considerably aided by sediments of a detrital origin. The whole series shows that the sediments have undergone considerable changes, but yet not sufficient to obliterate the original characters, and the lines of lamination and bedding are usually very distinct. That they were altered nearly into their present state before the Cambrian sediments were deposited upon them, is clear from the fact that the pebbles of the Cambrian conglomerates which rest immediately on any portion of the series are almost invariably made up of masses of the rocks below, cemented by gritty materials on an unaltered matrix, and from which the pebbles may be easily removed. The great conglomerates at the base of the Cambrians, everywhere in Wales, indicate that there were beach- and shallow-water conditions over those areas at the time, and that the sea was then encroaching on an uneven land, becoming gradually depressed to receive the subsequent Cambrian sediment.—On some pre-Cambrian (Dimetian and Pebidian) rocks in Caernarvonshire, by Henry Hicks, F.G.S. In this paper the author gave an account of the special examination of the great ribs of so-called intrusive felspathic and quartz porphyries which are found associated with the Cambrian rocks in Caernarvonshire, made by him in company with Prof. Hughes,

Mr. Hudleston, and Mr. Homfray last summer.—On the pre-Cambrian rocks of Bangor, by Prof. T. McKenny Hughes, F.G.S. The author described a series of slates, agglomerates, and porphyritic rocks which, near Bangor, are seen to pass under the Cambrian and seem to rest conformably upon the quartz felsites and granitoid rocks of Caernarvon. He considered that in the main the Bangor beds were the equivalents of the Pebidian of Dr. Hicks, while the Caernarvon beds nearly represented his Dimetian. But he thought there was as yet no proof of an unconformity between these formations.—An appendix by Prof. Bonney, on the microscopical examination of the rocks referred to, accompanied this paper.

Royal Microscopical Society, December 5.—Mr. H. C. Sorby, president, in the chair.—The president announced that in consequence of the death of Dr. Lawson it had become necessary to reconsider the subject of publication, and the Council had, after careful attention to the matter, decided in future to publish their own proceedings.—A paper by Herr Zeiss on Abbé's apertometer was read by Mr. Ingpen, who exhibited the apparatus to the meeting and further explained its construction and method of application by means of black-board diagrams. Mr. Ingpen also described the method of measuring angular apertures last adopted by Mr. F. H. Wenham.—A paper by Mr. F. A. Bedwell on *Cephalosyphon* was read by Mr. Slack, who afterwards explained the structure of this rotifer, and pointed out the special features to which attention was drawn by the author of the paper.—Another paper by the same author on a new method of examining *Actinia mesembryanthemum*, was read by Mr. Chas. Stewart; it was illustrated by drawings, some of which were enlarged upon the black-board.

Institution of Civil Engineers, December 11.—Mr. George Robert Stephenson, president, in the chair.—A description of Cofferdams used at Dublin, Birkenhead, and Hull, by Mr. William James Doherty, Assoc. Inst. C.E., was read.

GÖTTINGEN

Royal Academy of Sciences, August 24.—The division of a language into several different languages, by M. Benfery.—On the earthquake of Iquique on May 9, 1877, and the tidal movements thereby produced in the ocean, by M. Geinitz.

November 7.—Report on the Physical Institute (department of experimental physics), from 1871 to 1877, by M. Riecke.

November 14.—D instead of N, by M. Benfery.—Contributions to physiography of rock-forming minerals, by M. Lang.

November 21.—Antiquities in the south-west of Switzerland, and in Turin, by M. Wieseler.—On the secondary intestine of the echinoideæ, by M. Ludwig.—Obituary notices of M. Hartmann and M. Marx.

PARIS

Academy of Sciences, December 10.—M. Peligot in the chair:—The following papers were read:—On some applications of elliptical functions (continued), by M. Hermite.—On invariants, by Prof. Sylvester.—On the arrangements which, in the system of a navigation sluice with single oscillation, conduce to the maximum of production and the minimum of expense of construction, by M. De Caligny.—On the development of eggs of the phylloxera of the oak, and the phylloxera of the vine, by M. Boiteau.—M. Volpicelli sent a note tending to prove, by means of potential, that induced electricity of the first species has no tension.—Application of Leyden jars of large surface for distributing, at various points, the effect of the current from a single source of electricity, with strengthening of the effect, by M. Jablochkoff. Connecting one surface of such an apparatus (called in this case an *exciter*) with one of the conductors of a machine which gives alternate currents, an alternating current is got by the other surface of the exciter and the second conductor (or the earth), more powerful than the current given directly by the machine. If a series of exciters with surfaces of nearly 500 square metres be thus connected with a machine which gives a spark equivalent to that of six or eight Bunsen elements, a voltaic arc of 15 to 20 mm. is obtained, and carbons of 5 mm. diameter are reddened to an extent of 6 to 10 mm. from their extremity. Such effects are utilised in electric lighting.—On the law of absorption of radiations through bodies, and its employment in quantitative spectrum analysis (continued), by M. Govi. He shows how *surfaces of chromatic absorption* may be obtained by means of the analysing photometer, measuring the various simple radiations which take part in a complex radiation.—On some properties of chloride of calcium, by M. Ditte. He deals with the calorific phenomena accompanying the reaction of water with this chloride. If the latter be anhydrous, a heating is observed, and fresh additions of water cause successive heatings; but if the chloride be hydrated, its mixture with water produces at first a considerable cooling followed by heating if some more of the solvent be added.—Application of palladium wire to determination of the hydrocarbons mixed in a small proportion with air, by M. Coquillion. It is necessary to operate with a cherry-red, near white-red. The results agreed with theory.—On the development of the functions of M. Weierstrass according to the increasing powers of the variable, by M. André.—On the lesions of the nervous system in diphtheritic paralysis, by M. Dejerine. There is an atrophy of the anterior roots, which follows destruction of the cells of the anterior horns of the spinal cord, by a process similar to that of myelitis.—Orography, by M. Schrader. The author presented a geographical map of Mont Perdu, made with his orograph, which consists of a circular paper-covered plate with central vertical axis carrying a sleeve which can turn round freely. On the top of the sleeve is a telescope, the movements of whose frame in the vertical direction are communicated to a pencil, and transformed by gearing into to and fro movements. If the telescope describes a circle round the horizon, the style describes a corresponding circle on the plate; if the telescope goes up or down, the trace produced is further from or nearer to the central axis. A spirit level being fixed to the telescope, the circle made when it is even, gives a means of estimating the heights and depressions.—On the folding of the lacustrian strata of Auvergne in Central Limagne, and its consequences, by M. Olivier.—Influence of soil and forests on climate; temperatures of air layers over woods; consequences as regards vegetation; effects of currents arising from differences of temperature under wood and beyond wood, by M. Fautrat. The frigorific action of the forest is very manifest in the hot season. Under pines in September the temperature is lowered 1·60°. Pure sand raises the temperature of a place. Leafy woods, during vegetation, produce a slight lowering of temperature in the atmosphere above. Above pines, in the daytime, there is always a rise of temperature, from the solar heat being retained by the vapours enveloping the tree-tops. From the differences of temperature within and without woods, a current arises in the wood from below upwards, and round the woods course lateral currents from the wood to the plain.—On the disinfecting properties of cellulosic substances carbonised by concentrated sulphuric acid, by M. Garcin.

CONTENTS

THURSDAY, DECEMBER 27, 1877

THE METROPOLITAN SEWAGE

THE question of the effect of the main outfall sewers of the metropolis on the reaches of the Thames below London has occupied the attention of engineers not only since the completion of the works, but throughout the long series of years when those works were under consideration. Some persons qualified to make accurate observations and draw correct deductions from them, asserted that large masses of deposit were directly due to these outfalls, and were daily increasing in magnitude, while others, demanding equal confidence in their statements, asserted that no such deposits existed—in fact, that the sewage outfalls tended to improve the bed of the river by increased scour; thus the bulk of engineers for a long time held diverse views or suspended judgment on the subject, while the general public, not knowing whom to believe, trusted it would turn out all right in the end. Inasmuch as the Metropolitan Board of Works is bound, under the Thames Navigation Act of 1870, to keep the Thames free from banks and other obstructions to the navigation due to the flow of sewage from their outfalls, and to carry on all dredging operations required for that purpose, at their own expense, the vision of the possible cost of these works to the London ratepayer is unlikely to be pleasing; still less could any interference with the highway to the most important port in the world be tolerated by the Board who were looked to for its preservation. In 1869, the metropolitan main outfalls having been opened in 1863–64, the Home Secretary appointed Mr. Rawlinson to hold an inquiry on the reported silting up of the Thames, which was then causing great alarm; such, however, was the contradictory nature of the evidence, that the result was almost nugatory, and the question still remained in abeyance. In the course of the last summer the Thames Conservators requested Capt. Calver, R.N., F.R.S., to direct his attention to the subject, and report to them thereon. Before pointing out the conclusions arrived at by Capt. Calver,[1] it will be as well to direct attention to the part of the river under consideration. The northern outfall is situated immediately above Barking Creek, which forms the embouchure of the river Roding, and is about two miles below Woolwich; the southern outfall is about 2¼ miles lower, or 4½ miles below Woolwich.

In the face of the fact that this special inquiry was held and many competent witnesses examined with the sole object of determining whether or no the sewage outfalls have caused a silting up of the river in their neighbourhood, or the formation of shoals and mudbanks, and that so many observations and statements have since been made with the same view, it seems perfectly monstrous that the question should still remain unsettled. In the report now before us we have the last contribution on the subject, or perhaps, with more fairness it might he said the last but one, as since its publication Sir Joseph Bazalgette has addressed to the daily press a letter containing a direct denial of many of the conclusions there arrived at. When professional experts differ so entirely not only in their conclusions, but also in the facts upon which these conclusions are based, we see no other course open but to appeal to the cooler and more unbiased judgments of pure science.

In comparing the analyses of Thames mud from various parts of the river, given in Capt. Calver's report, embodying a series taken in 1867, and another in 1868, by the late Dr. Letheby, with those given by Dr. W. A. Miller, and Dr. W. Odling in 1869, so close an agreement is manifest that a safe conclusion can be drawn from them. The analyses are as follows:—

Average Percentage Composition.

Organic matter	15·00	14·19	1867	Dr. Letheby.
Mineral ,,	85·00	85·81	1868	,,
	100·00	100·00		

On these analyses Dr. Letheby remarks that the above percentage proportions did not differ materially from the quantities of organic and sewage matters which he found suspended in water at London Bridge, and in the mud at London Bridge, Chelsea, and Westminster, when the sewage was discharged at low water. The next table gives nearly identical results from the analysis of the mud at the outfalls in 1867, and those of the suspended matter in the Thames water at Greenwich, Woolwich, and London Bridge in 1862, by the same chemist. Now, Dr. W. A. Miller so far agrees with these results that in his evidence, given at the inquiry before referred to in 1869, he states the percentage of organic matter in the mud taken from Barking Creek to be 16·2, from the Thames between Chiswick and Westminster, 15·8, and further, that of these two quantities 3·1 and 3·05 respectively consists of nitrogen, and finally, in answer to the question: "But there is nothing special and differing in the mud at Barking from the ordinary mud of the River Thames?" he says: No, the composition is as nearly the same as may be. With these observations Dr. Odling's evidence closely agrees.

Here, then, we have an agreement which nobody appears to dispute, and which leads inevitably to the conclusion that the great bulk of noxious putrescible matter left uncovered at low water throughout the whole of the tidal portion of the Thames owes its deleterious character mainly, if not entirely, to the presence of sewage matters.

Having carefully pointed out and established this identity of composition, Capt. Calver proceeds: "It is, however, equally necessary to prove that there is enough of this material in the sewage discharged from the outfalls to account for the large accumulations of it which have found a resting-place in the Thames channel." Here we are met by estimates differing in the wildest manner, and varying from 35 to 100 grains per gallon, and again to nearly double that amount, but fortunately we are here even given material for a trustworthy estimate. In the table of analysis given by Prof. Williamson of samples taken from the northern outfall in September of this year we find 108·01 and 151·45 grains per gallon as the actual amount of suspended solid matter at different times, the samples being collected in fine weather. Now abundant evidence has been given at various times, showing that after heavy rain the sewage contains an amount

[1] "Report upon the Discharge of Metropolitan Sewage into the River Thames at Barking Creek and Crossness." By Capt. E. R. Calver, R.N., F.R.S.

of solid impurity equal to, if not greater than, that in the fine-weather flow; thus there can be no doubt that the lower of these two figures is not in excess of the average. Capt. Calver takes the amount at 100 grains per gallon, and multiplying by the daily discharge quoted as 120 million gallons, he obtains a result of 279,225 tons per annum. This probably does not exceed one-half the true amount, as the water supply of the metropolis alone reaches the amount assumed for the daily discharge, and the rainfall over the drainage area gives nearly an equal amount, which, for the reason just stated, must be taken into account. We thus appear to have at command upwards of half a million tons of suspended matter discharged into the Thames in each year, which is amply sufficient to account for the deposits observed. Thus we read in the report that "Mr. Leach (the engineer of the Thames Conservancy Board) reported in December (1871) that a deposition of 7 feet 9 inches of mud had formed between the upper end of the southern embankment and the White Hart Draw Dock, Lambeth; that another bank 100 feet wide and 6 feet thick occupied the river-frontage of St. Thomas's Hospital, &c. By July of last year a material portion of these masses had been cleared away by excessive rainfalls." Are we to be left to the mercy of such an unpleasant remedy as the floods of last autumn to abate a nuisance of such magnitude, threatening, as it does, the existence of such an institution as St. Thomas's Hospital, and showing how soon we may return to the unsanitary state of affairs that existed twenty-five years ago? We have purposely avoided dealing with an equally important part of Capt. Calver's report, in which he points out the danger of the silting up of the navigable channel of the Thames below London, as he has not shown that the sectional area, though varying from year to year, has at any point permanently diminished, still the destructive elements have been shown to exist, and the forces which now hold them in equilibrium may at any time be thrown out of balance and the evil creep on imperceptibly if once the eyes of the public are closed to its existence. Without going into the question of the value of the sewage estimated by the highest authorities at 1,000,000*l.* per annum, thus not only wasted but employed as a powerful obnoxious agent, enough has been shown from the report before us to, we hope, show the suicidal folly of discharging sewage wholesale and unpurified into tidal rivers. Yet even now a scheme is under consideration for the collection of the sewage from a large area in the Thames Valley and for its discharge into the tidal waters of the Thames. We believe that a careful perusal of Capt. Calver's Report will dispel from the minds of the Thames Valley Joint Board all hopes of a satisfactory though expensive solution of their difficult problem being arrived at in this manner. As a remedy for the state of things he has shown to exist Capt. Calver recommends that in pursuance of the powers they possess the Conservancy Board call upon the Metropolitan Board to dredge away the obstructions they have caused; this may be indispensable at present and may be an unavoidable and constantly recurring expense until some profitable scheme is devised for utilising the metropolitan sewage; in the meanwhile the example of the inhabitants of Abingdon, as shown by the letter of their medical officer of health in the *Sanitary Record* of November 30, shows the inutility of other towns in the valley of the Thames striving to follow the example of London, and further increasing its difficulties. We learn from Dr. Woodforde's letter that the whole of the sewage of the town of Abingdon is purified by filtration through natural soil being frequently absorbed by one acre of land, and that the amount of organic and inorganic impurity contained in the effluent water after passing through the land is far less, in some cases less than one half that contained in the well water used for drinking purposes in the town. As this unprecedented result has been obtained on land of a character which exists in abundance throughout the Valley of the Thames we think that the towns situated therein have not far to look for the solution of their difficulties.

BOTANY IN GERMANY

Jahrbücher für wissenschaftliche Botanik. Herausgegeben von Dr. A. Pringsheim. Elfter Band. Erstes und Zweites Heft. (Leipzig: W. Engelmann, 1877.)

THE second decade of volumes of the Jahrbücher is now begun, and up to the present shows no sign of any falling off from the high standard of excellence attained by the former parts. It is somewhat remarkable that such a work can be carried on successfully. Profusely illustrated (having about 500 plates in the ten vols.), and containing papers of great merit, it is at once evidence of the marvellous botanical activity of the Germans, and the energy of their publishers. A glance at the list of papers in the ten volumes shows that the Jahrbücher contain papers that have become classical, and have been contributed by men who have risen to the highest eminence in botanical science. Comparatively few of the papers are contributed by Russians or Italians, hence this one work may be looked on as almost wholly the result of German research. The papers contributed are chiefly morphological and physiological, although occasionally one having immediate bearings on taxonomy is introduced. There can be little doubt that the German university system tends greatly to foster original research, not only in botany, but in all other departments. The botanical institutes, with laboratory, garden, and herbarium attached, the way in which the students are induced not only to learn but to work under the superintendence of the professor, the whole system of private teachers and mode of promotion of the professors fosters research, and gives a thoroughness and heartiness to the work. In certain departments of botany, Britain is second to none with her Hooker, Bentham, and Darwin, but when we consider the enormous "microscope" power of Britain, we cannot help thinking that much of it goes to waste. There must be hundreds of microscopists residing near our coasts, yet what do we know of the reproduction of our algæ? A glance at the "Botanischer Jahresbericht" shows how few British botanists there are, and also that each contributes comparatively few papers per year. But quality is better than quantity—work slowly and well. The time is no doubt coming when we may look for increased botanical activity, perhaps the union of botanical studies to medicine has had something to do with the comparative depression, and if botany be-

comes a preliminary instead of a purely professional study by becoming more diffused, a greater taste for the subject may arise.

Prof. Pringsheim contributes the first paper, one part dealing with the interesting subject of the budding of the fruit of mosses, the second on the alternation of generation in the Thallophytes, a subject suggested by the first part. If the seta of the ripe fruit of the moss be cut into pieces, and the pieces cultivated on wet sand, protonema threads will grow from the cut portions, and produce the usual buds, exactly like protonema threads developed from the spores or stem and leaves of mosses. The anatomical connection of the protonema with the tissue of the seta can be observed in good longitudinal sections. Not all the cells can give rise to protonema, but only those of the middle zone, situated between the peripheral cortical cells and the central bundle. These cells contain abundance of reserve matter, such matter being found in many parts of the moss-fruit. The product of protonema by the seta of the moss is to be compared to the budding of the prothallium of ferns described by Farlow. Pringsheim figures in the two plates illustrating the paper, the protonema developing from the seta of *Hypnum serpens*, *H. cupressiforme*, and *Bryum caespitosum*, and he shows the stem and seta to be identical structures.

The second part of the paper, on the alternation of generation in Thallophytes, is difficult to follow without illustrations, as it takes for granted that the reader is acquainted with all the recent researches on the lower plants. Pringsheim distinguishes between sexual alternation of generations and vegetative alternation of generations (sprosswechsel), the fructification and vegetative propagation. All the generations of Thallophytes (as well as of the Cormophytes) begin with one free cell (the spore). The generations in the Thallophytes represent free individual plants, while in the Cormophytes the generations remain in organic connection and in their individual sequence appear only as two portions of one series of developments. From this it follows that the "fruits" of Thallophytes never have the value of a "generation," and also that where the development is due to sexual influence, they are only sexually influenced organs of the female plant. Such parts are the fruits of *Florideæ*, also apparently the Perithecia and Apothecia of Ascomycetes, which do not behave differently from the calyptra of the moss or the thickened tissue (gewebepolster) of the prothallum, in which the embryo of the vascular cryptogams is developed. Pringsheim believes that in the trichogyne and ascogon the influence of fertilisation is spread from cell to cell until it reaches the spores, just as in mosses and ferns the reverse process occurs, and the influence spreads from the fertilised germinal cell to the archegonium. Carpospores and ascospores are therefore to be regarded not as sexually-produced spores of a sexually-produced generation, but as truly sexually-produced spores, developing in the sexually-influenced organ of the mother plant.

The second paper, illustrated by two plates of diagrams, and occupying nearly half the part, is by F. G. Stebler, "Researches on Leaf-growth." The numerous observations made on *Allium Cepa*, *Secale cereale*, *Triticum vulgare*, *Cucurbita melanosperma*, are detailed at full length, and the following summary of the result of the paper is given at the conclusion. The leaf begins to grow very slowly, then growth becomes more rapid until a maximum of rapidity is reached; then growth becomes slower and slower until at last it ceases. The leaf thus behaves like other growing parts of plants. The growth of the linear monocotyledonous leaf is basipetal. The apex zone of the leaf ceases earliest to grow, then succeeding zones in basipetal order, until lastly the growth of the basal zone terminates the growth of the entire leaf. Most productive of increase in length is the growth in the basal zone, but at different times the maximum activity is in different zones, the absolutely greatest zone of growth proceeding in succession from the upper part of the leaf to the lower. The maximum period of growth of the whole leaf is the sum of the maximum periods of all the zones.

The linear monocotyledonous leaves examined in reference to alternations of growth by day and night showed a daily periodicity of growth, the growth diminishing as the intensity of the light diminishes. The maximum of growth corresponds to the greatest intensity of light; the minimum is observed to occur shortly before sunrise. The cause of the daily periodicity of growth is assimilation; as assimilation increases the growth increases; as it diminishes the growth diminishes.

The same daily periods of growth were observed in etiolated linear monocotyledonous leaves in the dark, the external conditions being constant. The periodicity has thus been transmitted.

In the dicotyledonous leaves observed the daily periods were modified, so that after the maximum of growth was reached in the forenoon a retardation took place, and a gradual diminution of the growth till the following morning before sunrise. At daybreak the growth rapidly increases again to reach a maximum in the forenoon. If the intensity of the light is small the maximum is later of occurring than if the light be very intense.

The maximum of the day periods of growth of the dicotyledonous leaf is due to the assimilation. The retardation during the day occurring after the maximum of growth (but not the maximum of light) has been reached, is due to the action of the light.

The third paper occupying the remainder of the part is by Dr. Celakovsky, and is entitled, "Teratological Contributions to the Morphological Import of the Stamens." It is illustrated by three plates. Considerable uncertainty still exists as to the morphological value of the different parts of the stamen, but more especially of the anther. The difficulty does not exist in regard to the pollen-bearing caulomes, but there are still difficulties in those cases where the stamens are modified leaves. Whether the question can be settled by the study of the development alone is a matter of doubt, even after the valuable researches of Warming and Engler on the subject; and it appears likely that the most important results may be expected from the careful study of the numerous abnormalities of stamens so constantly met with. The scientific study of the teratological developments of stamens must therefore be looked upon as of the highest importance, and Celakovsky—already well known by his teratological researches, here describes and figures the changes (phyllody) of the stamens of *Rosa chinensis*, *Dictamnus albus*, and in the double flower of *Camellia japonica*.

There are two important questions to be answered. 1. Are the pollen-sacs mere enlargements of the leaf-substance of the staminal leaf, or are they special developments somewhat like "emergences"? 2. Do these sacs belong to the under side, upper side, or both sides of the leaf; or are there differences of position in different plants?

Cassini and Roeper held that the pollen-sacs were cavities in the leaf-parenchyma, two forming on each side of the leaf, so that the margin of the leaf corresponded to the suture between the sacs. Mohl considered this view only to hold for certain cases, as the Euphorbiaceæ, and found, what Bischoff had already pointed out, that in all examples examined, as in poppy, rose, and nigella, the four pollen sacs were placed on the *upper* side of the leaf, and that the margin of the leaf ran along the two posterior or lower loculaments. Mohl did not consider the sacs as "emergences," and differing morphologically from the true leaf, as he says that the connective represents the central portion of the modified leaf, while the loculaments are the thick swollen lateral halves, which become contracted in length and breadth. Mohl considered that in the plants with extrorse anthers both the loculaments of each anther lobe were developed on the under side of the leaf. Alexander Braun pointed out in 1851 that the anthers were produced by doubling of the lamina (Ueberspreitung). This view was confirmed by Wydler in 1852, who compared the anther to the abnormal double lamina in the leaf of Bignonia.

Sachs considers the anthers to be appendages of the leaf. He compares each loculament in the anther of Cycads and Cupressineæ to Sporangia; the four pollen-sacs in the Metasperms being "emergences" from the upper side of the leaf, those of the Archisperms from the lower side. Braun still further examined the subject and confirmed his original views, namely, that the pollen-sacs do not belong to a simple leaf, but to one with a double lamina, the doubling due to the formation of an "emergence" (in Karl Schimper's, not in Warming and Sachs' sense). The two upper anther sacs belong to the "emergence," the two posterior to the original lamina of the leaf. Celakovsky in the paper now before us departs from the views published by him in *Flora* for 1874, and fully confirms the views of Braun and Wydler.

The second part of vol. xi. contains five papers by Pfitzer, Koch, Reinke, and Reinsch. Dr. Pfitzer's paper is on the rapidity of the current of water in the plants. It contains an elaborate series of researches, the first on the movement of leaves due to the absorption of water by the stem and root; the second by means of solution of lithium. Dr. McNab's experiments are extended and confirmed, but the astonishing rapidity of 22 metres per hour was observed in *Helianthus annus*, the greatest rapidity observed by Dr. McNab being 40 inches per hour in *Primus Lauro-cerasus*. Pfitzer also uses a solution of soluble indigo carmine 4 parts to 1,000, and finds that it is superior to solution of lithium, as it can be detected at once instead of using the spectroscope.

The second paper is by Dr. Ludwig Koch, on the development of the seeds of Orobanchaceæ. The development of the anatropal ovule, with one integument is described, and the development of the embryo. This agrees with the description given by Hanstein, of the embryo of Capsella. The endosperm is formed of divisions of embryo-sac, which contains antipodal vesicles before fertilisation. The third and fourth papers are by Prof. Reinke, both on the development and reproduction of algæ, of the genera Phyllitis, Scytosiphon, Asperococcus, and Bangia, the observations having been made at the Zoological Station at Naples, during the winters of 1875 and 1876.

The last paper is by Reinsch: "Observations on new Saprolegnieæ, on parasites in cells of Desmedicæ, and on the 'Spinous Spheres' in Achyla." A number of new species and genera are described and fully illustrated.

W. R. M'Nab

MOVING DIAGRAMS OF MACHINERY

Patent Working Drawings. By H. and T. C. Batchelor (London: Macmillan and Co.)

ALL who are engaged in the teaching of kinematics and of applied mechanics must often have it brought forcibly before them the difficulty that exists in making even comparatively simple mechanical motions intelligible to students by means of ordinary drawings and diagrams, while the more complex motions and combinations can hardly be treated of at all profitably without the aid of working models, which are very expensive, and take up a great deal of space. Again, inventors and the proprietors of patented mechanical inventions, are often at a loss to explain to unscientific or uninitiated persons the advantages of their systems, and costly working models have to be resorted to in order to avoid the mystification which ordinary mechanical drawings often produce in the minds of those not accustomed to them, or who are not versed in the principles of mechanics.

To supply this recognised need of teachers and others, Messrs. H. and T. C. Batchelor have designed and worked out a most ingenious system which combines the mechanical movements of a model with the flatness and clearness of a diagram. The name "Working Drawings" applied to these diagrams is somewhat misleading, especially to engineers and others accustomed to this term as having a distinct and special meaning, namely, drawings made for and used by the workmen employed upon the construction of machinery to work from. Working drawings are essentially drawings for the workshop, and that is the universal acceptation of the word. The meaning attached to it by Messrs. Batchelor is, however, very different; it is drawings which will work moving diagrams. This sense is, perhaps, more critically correct, but as another meaning is the generally accepted one, we cannot but think that it would have been wise if a name had been given to these diagrams more descriptive of what they are. They are, in fact, moving diagrams or sectional working models of machines, the fixed parts being lithographed as a background upon a firm cardboard mount, and the moving parts being also lithographed on card, but cut out and jointed together by most ingenious mechanical contrivances; the whole being no thicker than a sheet of stout cardboard.

The perfection of the centres upon which the various parts revolve or are pivoted together must be seen to be adequately appreciated, for while these centres allow perfect ease of motion to all the parts, they are absolutely

steady and without the slightest shake. It is this system of centring that constitutes the patent by which Messrs. Batchelor's drawings are protected. The pivots are made entirely of card and paper cut in a most ingenious manner, by which both freedom and steadiness are insured. Nor are the centres the only parts of these drawings interesting for their ingenuity; the contrivance for holding down the sliding parts is equally good. It consists of a band of thin paper passing over the sliding part, and printed exactly like the part it covers, so that it is invisible except on close examination.

The first of these drawings which is before us is a diagram in illustration of the action of the "trunk engine," the characteristic feature of which consists in making the piston-rod hollow and of sufficient internal diameter to allow the connecting-rod to be attached at one end direct to the piston, and to oscillate within the trunk, the other end embracing the crank-pin. By this means the crank shaft can be brought nearer to the cylinder, considerable space thereby being saved, and the alternative system by which direct connection between the piston and crank is effected, viz., the oscillating cylinder, is avoided, with its more complicated valve gear and expensive construction.

The double trunk system represented in the drawing was the invention of the late Mr. John Matthew, who for many years was a partner in the eminent firm of Messrs. John Penn and Sons, and it is the system upon which, almost without exception, the large screw engines of Messrs. Penn are constructed, with which so many of the ships in her Majesty's navy are fitted.

There is nothing to be desired in the execution of the diagram before us, of which the name of Messrs. Maclure and Macdonald is a sufficient guarantee. It is lithographed in white upon a blue ground, and all the parts come out with singular distinctness. We could have wished that, in the choice of an example for illustration, a more modern design of engine had been selected. The eccentric rod, with its lattice bracing, is that employed in the old beam engines, and a trunk engine made to the drawing before us could hardly work, for the crank pin is evidently inserted into one of the spokes of the fly-wheel, and unless projecting to an impossible extent, the trunk could not clear the wheel; this could very easily have been remedied by showing the "throw" of a crank behind the connecting-rod, which would have aided rather than detracted from the clearness of the diagram.

While thus criticising the particular design of engine selected for representation, we can only express admiration of this most ingenious system of illustrating mechanical motions and the action of machines. For educational purposes it will be of the highest value, and there are many of the examples in Reuleaux's masterly work upon the "Kinematics of Machinery,"[1] so ably translated by Prof. Kennedy, to which it might with great advantage be applied.

We feel sure that Messrs. Batchelor's drawings will be a great boon to inventors for explaining their inventions to others; and as supplementary to scientific evidence in disputed patent cases and other litigation, they will be found of value.

C. W. C.

[1] "Theoretische Kinematik."

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Oxygen in the Sun

FROM the time of the discovery by Prof. Draper of the presence of oxygen in the sun down to the present moment I have devoted most of my leisure time to the consideration of the question as to why the oxygen lines should appear *bright* while the metallic lines should appear dark in the solar spectrum. I was led into this inquiry under the firm belief that the new fact made known by Dr. Draper might lead to a modification of existing views of the sun's atmosphere, and it was consequently with the greatest pleasure that I read in last week's NATURE a communication from Dr. Schuster on this subject.

The views which I have arrived at being in my opinion hardly matured enough for publication, I reserve further statement at present, but will so far anticipate as to say that the explanation which I am disposed to maintain necessitates the assumption that oxygen possesses two different spectra—a low temperature band-spectrum and the well-known line-spectrum of high temperatures. This assumption I thought warranted by the behaviour of other non-metals as made known by the researches of many spectroscopists, but more particularly by those of Salet and Lockyer, and endorsed by the low temperature absorption spectra of the metals discovered by Roscoe and Schuster, Lockyer and Roberts. The recent research of Dr. Schuster, however, has now placed this assumption in the position of a fact, and all who have followed recent spectroscopic advancement will recognise the value and importance of this last discovery.

As Dr. Schuster's explanation of the brightness of the oxygen lines differs fundamentally from that which I am inclined to hold, and as he considers this view warranted by the result of his investigation, I will beg permission to make a few brief remarks upon the chief points of difference between us, being convinced that their discussion cannot fail to elicit opinions of interest to all concerned in the progress of solar physics. This ventilation of opinion is the more necessary as views very similar to, if not identical with, those of Dr. Schuster's had occurred to me and had been abandoned for reasons which I will now explain.

According to Dr. Schuster, "the temperature of the sun, at some point intermediate between the photosphere and the reversing layer" is the same as that at which the spectrum of oxygen changes; that is to say, if I rightly interpret these words, *above the photosphere* the temperature is such that we get the line spectrum of oxygen and *above the reversing layer* the temperature is such that we get the band ("compound line") spectrum. This state of affairs would doubtless account for the reversal of the "compound line" spectrum which Dr. Schuster has now shown to be present in the solar spectrum, but I fail to see at present how it is to be reconciled with the *bright* line oxygen spectrum. Let us consider the conditions more closely. All observers agree in placing the reversing layer at the base of the chromosphere—the present hypothesis necessitates a space between the photosphere and the reversing layer—*i.e.*, a space sufficiently extended to contain the incandescent oxygen giving the line spectrum. Neglecting for the present the antagonism between these views, let us assume that such a space exists, and for the sake of simplicity let us also neglect the other elements which may be present. Now it cannot be assumed that the supposed zone is *higher* in temperature than the photosphere—it *might be* of the same temperature, but, being above the photosphere it would more probably be at a *lower* temperature. Let us, however, make the assumption most favourable to Dr. Schuster's view, viz., that the hypothetical zone is of the same temperature as the photosphere. Then we have a zone of oxygen exterior to the photosphere and of the same temperature as this last region, and above the oxygen the cooler reversing layer. Thus the light of the photosphere passes unchanged through the oxygen zone, and we should see no dark lines corresponding to the line-spectrum of this gas.—As a matter of fact, however, the oxygen lines are *bright*—hence it must be at a higher temperature than the photosphere, or we must be looking through an enormous stratum of

it, a stratum thick enough for the radiation of the gas to overpower the fierce glare of the photosphere behind it, and both these views have been shown to be untenable.

December 21 R. MELDOLA

Oxygen in Sea-water

AT p. 267 of the second volume of the "Voyage of the *Challenger*," Sir Wyville Thomson writes:—

"Mr. Buchanan drew the conclusion in explanation of the small amount of oxygen at depths of 300 fathoms and upwards, 'that animal life must be particularly abundant and active at this depth, or at least more abundant than at greater depths.' In other words, that a permanent condition, probably of all conditions the most unfavourable to animal life, is produced and maintained by its excess."

"This is entirely contrary to experience."

The words in inverted commas are part of a sentence in a short report in NATURE (vol. xvi. p. 255), of a paper which I read before the Royal Society of Edinburgh, on the results of the analysis of so many of the samples of air extracted during the cruise, from sea-water of different sources, as I was able to accomplish before my connection with the work of the expedition ceased. I will not encroach on your valuable space by anticipating the discussion of the bearing of my observations and those of others on the question of the greater or less abundance of animal life at different depths in the sea; but as the above quotation, from its fragmentary character, is somewhat misleading, both as to the nature of the belief which I expressed and my grounds for holding it, I must ask you to give place to the concluding sentences of the above report:—

"It is evident from these figures[1] that between 200 and 400 fathoms there is a great consumption of oxygen going on, and, as it is difficult to conceive its being consumed otherwise than by living creatures, the conclusion is forced on us that animal life must be particularly abundant and active at this depth, or, at least, more abundant than at greater depths; for at less depths there is more opportunity for renewal of the oxygen by reason both of the greater proximity to the surface and of the existence of vegetable life. This conclusion was borne out by the numerous experiments made by Mr. Murray with the tow-net at intermediate depths, which went to prove the existence of abundance of animal life down to 400 fathoms, vegetable life never extending to much below 100 fathoms. Below 400 fathoms life is sparingly met with."

It will be seen that the only independent experience which exists, namely, Mr. Murray's observations with the tow-net at different depths, is in favour of the conclusion at which I arrived. J. Y. BUCHANAN

10, Moray Place, Edinburgh, December 13

[1] A table of the mean amounts of oxygen in a hundred parts of oxygen and nitrogen contained in waters from different depths

On some Peculiar Points in the Insect-Fauna of Chili

FOR some years past I have been particularly interested in some points in the entomology of Chili and the extreme southern portion of South America, which, although known to most entomologists who have made special groups their study, have never yet been, so far as I know, even more than casually alluded to in works on geographical distribution, and are ignored in the principal ones. I allude to the occurrence in that part of the world of well-marked palæarctic or nearctic forms not found otherwise in America south of Mexico, and utterly unknown in the southern hemisphere in the Old World.

I have collected a not inconsiderable amount of data concerning this subject, and have the intention of addressing a circular to zoologists and also to botanists, asking for further information.

I will here allude to such familiar genera as *Carabus* amongst beetles and *Argynnis* and *Colias* amongst butterflies. *Carabus* is very abundant in species in the palæarctic region, poor in the nearctic, and reappears (for the whole world) only in Chili. The distribution of *Argynnis* and *Colias* is similar, only that they are about equally abundant in the two northern regions, and of *Colias* it appears probable that a single species occurs in Peru, but this exception only proves the rule.

In the *Trichoptera*, or Caddis-flies, a group of insects in which I am especially interested, there is even a still more striking case. The typical family, *Limnophilidæ*, comprising those insects the larvæ of which manufacture the cases of twigs and straws, so abundant in our ponds and ditches, and which is so rich in species in northern regions, is not, with the exception stated below, known south of Mexico in the New World nor south of the Himalayas in the Old; but I have several species from Chili, Araucania, and the Falkland Isles.

I could already multiply parallel instances, but have said enough to prove my case.

Confessedly I have, at present, only crude theoretical notions on the causes of this anomalous distribution. It might be said that these insects are the remains of a former Antarctic glacial epoch. But if this be so, then we must presuppose the existence of former Arctic and Antarctic faunas similar in details; all other evidence tends, I think, to disprove this. It may truly be said that, owing to the non-existence of large tracts of land towards the south pole at all comparable with those that exist towards the north, we are not in a position to acquire sufficient data, yet we have the continent of Australia and the large islands of New Zealand extending somewhat far south, and they furnish us with no indication whatever of forms parallel with those found in Chili.

It has occurred to me as just possible, that at the conclusion of the northern glacial epoch a few stragglers, instead of wending their way northward, mistook the points of the compass and went southward. But there remains this great difficulty, viz., that, with one possible exception, there are no indications of these forms on the northern portions of the Andes of South America.

I call attention to this subject as one deserving far more consideration than it has hitherto received, and with the idea that, by ventilating it in NATURE, I may receive additional information on a point that greatly interests me.

39, Limes Grove, Lewisham R. MCLACHLAN

Arctic Auroræ

IT will probably interest some of your readers to know that in reply to a communication lately addressed by me to the Admiralty I am informed that Captain Sir George Nares reports that although the auroral glow was observed on several occasions between October 25, 1875, and February 26, 1876, true auroræ were seldom observed, and the displays were so faint and lasted so short a time that the spectroscopic results were not considered worthy of a special report. Although the citron line was seen occasionally, on only two occasions was it well defined, and then for so short a time that no measure could be obtained. A report is preparing with a view to compare the auroral displays with magnetic disturbance, meteorological changes, and other phenomena which will include the few spectroscopic observations obtained. J. RAND CAPRON

Guildown, December 24

Insects and Artificial Flowers

IN a late number of NATURE a short account is given of some experiments recently made by Prof. J. Plateau, of Ghent, as to insects being deceived by artificial flowers. The nature of these experiments is not given, but the result would appear to have been of a somewhat negative character. In connection with the subject the following incident will not, I think, be considered uninteresting. I was coming by one of the lake steamers from Como to Menaggio, in September, 1875, and saw a humming-bird hawk moth, *Macroglossa stellatarum*, fly to some bright-coloured flowers on a lady's hat on deck, and hang, poised over them for a short time, and then fly away. During the process it made one of those short familiar darts off, for a moment, and then returned, after the manner of the moth when disturbed, and it remained long enough to convince me that it had tested the flowers and found them wanting. Another incident comes across my mind while writing this, which, though it does not exactly bear upon the point, yet is of a somewhat kindred nature. I was crossing from Harwich to Antwerp in August of the same year, and as the weather was fine, and the boat crowded, I remained on deck all night. About 4 o'clock in the morning I saw what appeared to be a bird or a bat flying rapidly about the rigging. As I was watching it the funnel of the steamer poured forth a thick column of black smoke, owing to the fresh coaling it had just received. Off went the creature as soon as it perceived the change, or, at all events, as soon as the change took place, and flew for some time in and about the smoke, now darting through it, close to the funnel mouth, and then letting itself be borne along with it, for some distance, as if in sport, looking very strange and weirdlike in the process.

After awhile, as the full daylight broke, it left the smoky region above and came down towards the deck, and I then discovered it to be neither bird nor bat, but a specimen of the death's-head moth, *Sphinx Atropos*, whose flight I then witnessed for the first time. After running the gauntlet of several of the passengers, who tried to catch it with their hats, it settled somewhere on the spars or woodwork of the boat and escaped, perhaps to renew its flight in a similar manner the following day.

Highfield, Gainsborough, December 21 F. M. BURTON

The Selective Discrimination of Insects

MAY I be permitted to remark on Mr. Bridgman's communication in NATURE (vol. xvii. p. 102)? He says he has collected pollen grains of different kinds washed from the thigh of an *Andrena nigro-ænea*, and varying in colour from orange-red to white. The true inquiry as to the discrimination of insects is not as to the colour on distinct kinds of pollen, but their homogeneity in respect of fertilisation.

The remark I made implied, rather than expressed, that bees and butterflies visited only those plants the admixture of the pollens of which induced fertilisation. In this respect and in this only, it appears to me, the investigation of the subject becomes of importance. No fact of natural phenomena is without use and without instruction; there are *no hap-hazards* in nature. If Mr. Bridgman, or other naturalists, can show the admixture of the diverse grains of pollen collected by him from the thighs of the creature named would not induce the fertilisation of the plants from which they were collected, then the discriminatory fact assumed is dispelled and the peculiarity observed by Mr. Forbes and myself, and doubtless by others, becomes of little value. The colour of the pollen grains is of no importance in the inquiry, as observation shows; the discriminatory fact, if it has any importance, is not as to variations in colour, but the collection of the pollen from distinct species of plants the admixture of which *would not induce fertilisation*. If it be proved that the admixture of the collected pollens are only such as induce fertilisation, then a natural phenomenon is disclosed of great importance. This is the fact I imagine Sir John Lubbock meant when he advised the pursuit of the inquiry.

I am still of opinion that it is *odour*, not colour, which is the attractive element. It is so with carrion birds and the blow-flies which collect on the fœtid arum. In phenomena one particular law appears to be repeated in all the natural kingdoms. The same rule is also to be observed in physics. S. B.

Sunbury-on-Thames

OUR ASTRONOMICAL COLUMN

THE TOTAL SOLAR ECLIPSE OF A.D. 418, JULY 19.—Philostorgius, in his "Epitome of Ecclesiastical History," relates that while Theodosius the Second was a youth, on July 19, at the eighth hour of the day, the sun was so greatly eclipsed that the stars were seen, and while the sun was thus hidden there was seen in the sky a light in the form of a cone, "which some ignorant people called a comet"; and he goes on to describe the supposed differences in the appearance of the phenomenon from that of a comet, particularly remarking that it resembled the flame of a torch, subsisting of itself, without any star to serve as a base, and adding particulars of its track and duration. That the object thus singularly discovered during a total, or nearly total, eclipse of the sun, was really a comet as the "ignorant people" supposed, is proved by the records in the Chinese Annals. The eclipse to which reference is made by Philostorgius took place on July 19, A.D. 418. The comet of that year is stated in Pingré's *Cometographie* to have been discovered in the 10th moon, commencing November 15, in which he follows the Jesuit, Couplet, but the account given by Mr. Williams, on the authority of the She Ke and Ma Twan Lin, dates the appearance of the comet on day Kang Tsze of the 5th moon, when it was situate in Ursa Major; on September 15 it was on the confines of Leo and Virgo; "it was bright, and gradually lengthened until it was 100 cubits in length." Philostorgius also refers to the passage of the comet through Ursa Major, and says it continued visible until the end of the autumn.

It may interest some readers to have particulars of the eclipse, during which it is recorded that a large comet was first discovered. The following figures depend upon a very similar system of calculation to that applied to other ancient eclipses, described in this column:—

G.M.T. of Conjunction in R.A. 418, July 18, at 23h. 3m. 17s.

	° ′ ″
Right Ascension	118 30 48
Moon's hourly motion in R.A.	38 34
Sun's ,, ,, ,, ...	2 32
Moon's declination	21 23 10 N.
Sun's ,, ...	21 2 41 N.
Moon's hourly motion in decl.	3 40 S.
Sun's ,, ,, ,, ...	0 28 S.
Moon's horizontal parallax	59 38
Sun's ,, ,, ...	0 9
Moon's true semi-diameter	16 15
Sun's ,, ,, ...	15 48

At Constantinople these elements give a very large eclipse, commencing at 0h. 5m. and ending at 2h. 50m. local mean time, magnitude, 0·95; at a short distance to the south the eclipse would be total.

VARIABLE STARS.—The following are geocentric minima of Algol and S. Cancri during the ensuing two months, so far as they are visible in this country. They are expressed in Greenwich mean time, and are calculated from Prof. Schönfeld's elements:—

ALGOL

	h. m.		h. m.
Dec. 31	12 29	Jan. 28	4 41
Jan. 2	9 18	Feb. 5	19 8
,, 5	6 7	,, 8	15 58
,, 16	17 23	,, 11	12 47
,, 19	14 13	,, 14	9 36
,, 22	11 2	,, 17	6 26
,, 25	7 52	,, 28	17 43

S. CANCRI

	h. m.		h. m.
Dec. 31	10 20	Feb. 7	8 51
Jan. 19	9 35	,, 26	8 7

ASTRONOMICAL PHENOMENA IN 1878.—The principal astronomical occurrence of the next year is the total solar eclipse of July 29, which traverses British Columbia and the United States; the American astronomers will doubtless give a good account of it, and it is reported they are likely to have coadjutors from this side of the Atlantic. There will be a transit of the planet Mercury on May 6, visible in this country to past the time of the nearest approach of centres, and a lunar eclipse on August 12, magnitude 0·6, wholly visible here. Mars will be occulted by the moon on the evening of June 3, and the second-magnitude star σ Sagittarii on the afternoon of October 30; on November 10 the moon traverses the Pleiades. A return of Encke's comet to perihelion also takes place in the summer, but not under favourable circumstances for observation, and the comet of short period detected by Tempel on July 3, 1873, will again arrive at perihelion late in the spring. Saturn's rings disappear on February 6, but reappear on March 1, according to Bessel's elements.

FERTILISATION OF GLOSSOSTIGMA

THE following letter to Mr. Darwin has been forwarded to us by him for publication:—

"Museum, Auckland, October 23, 1877

"My Dear Sir,—I forward to you a copy of a paper on the fertilisation of *Selliera*, one of the *Gooeniaceæ*, which perhaps you may care to glance over. When I wrote it I did not know of your notes on *Leschenaultia*, published in the *Gardener's Chronicle* for 1871. In both plants the pollen is shed before the expansion of the flower, and neatly collected in the indusium, but in *Selliera* the stigma is situated within the indusium, and by its gradual upward growth after the flower expands slowly forces out the pollen, which is then transferred by insects to older

flowers. When mature, the stigma protrudes considerably beyond the indusium. This appears to differ entirely from what takes place in *Leschenaultia*.

"I have recently been much interested with the curious irritability displayed by the stigma of *Glossostigma elatinoides*, one of the Scrophularineæ. The style is dilated towards its apex into a broad spoon-shaped stigma, which, when the flower expands, is closely doubled over the four stamens, entirely concealing them from view. If the front of the bent part of the style is touched it at once springs up, uncovering the stamens, and moves back to the upper lobe of the corolla, to which it becomes closely applied. In this position it remains for a few minutes, and then slowly moves back to the stamens and curves over them as at first. It appears to me that this irritability of the stigma is simply a contrivance to insure cross-fertilisation, for an insect crawling into the flower must inevitably touch the stigma, which would then uncover the stamens. On withdrawing, the insect would be certain to dust itself with pollen, but it would not by this effect the fertilisation of the flower, for the stigma would be then closely applied to the upper lobe of the corolla, entirely out of its way. If the insect were, however, to visit another flower it is evident that it must come into contact with the stigma at its first entrance and would doubtless leave some pollen thereon. The movement of the stigma is remarkably rapid, and its apex must pass through an angle of at least 180°. I have been unable to find a record of a similar case, or of so pronounced a degree of irritability in the stigma of any plant. The movement of the lobes of the stigma in *Mimulus* is much weaker, and is through a much less angle. Yours faithfully,

"T. F. CHEESEMAN

"Charles Darwin Esq., F.R.S."

A TELEPHONIC ALARUM

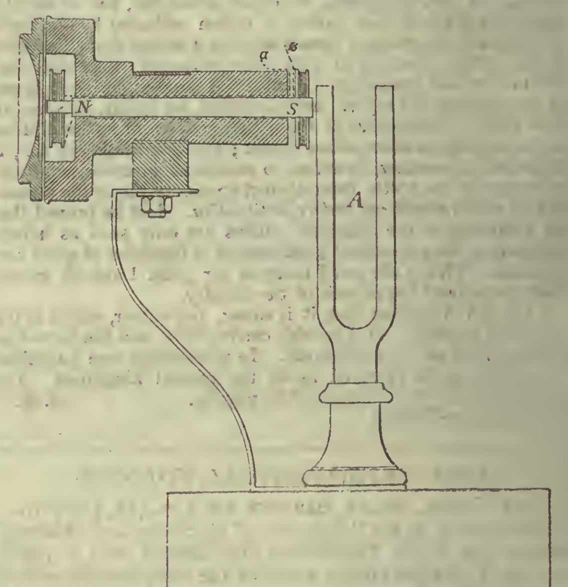
THE speaking of the telephone is admittedly so weak that it can only be caught by keeping the instrument in immediate contact with the ear. Hence there is transmitted through the telephone in its present form no sound which would be intense enough to announce to any one who was in a large room and who did not hold the telephone close to his ear, that a message was about to be sent from the transmitting station. The consequence is that a warning apparatus must be attached to the telephone, so that there may be no fear of missing a projected telephonic conversation.

It is clear that the conducting wire of a telephone can be used to sound a bell as an alarum by means of a current from a galvanic battery, and thereby the defect referred to would be supplied. But the necessary apparatus would considerably raise the price of fitting up a telephone apparatus, and besides, one most important property of the telephone, viz., producing the required electric current automatically, would be partly lost. I have, then, invented another warning apparatus, which, I believe, is quite workable.

Hitherto telephones have been so constructed that only one pole (N in the figure) of the magnet is effective; I now use also the second pole S, by providing it with a coil of wire, which is simply inserted in the circuit behind the first coil. (The dotted lines in the figure will explain this connection; the two ends *a* and *β* are connected with the binding screws fastened to the telephone; from this the circuit goes to the second telephone.) Before this pole of the magnet may be very easily set up a tuning-fork, A, which, with the telephone, is simply fixed on a resonance case, B; this arrangement should be made both at the transmitting and receiving stations, and both forks should be in unison. If now the sending station wish to signal that a conversation is to be begun, the fork of that place will be sounded with a fiddle-bow; the currents thereby induced in the coil are powerful enough to set the fork of the receiving station in such intense vibration that the sound may be distinctly heard in a large room; warned by this signal a person can in the usual way put the telephone to his ear and listen to the words from the transmitting station. And so *vice versâ*.

I have made an experiment in a large room, when about 100 people were present, and all could hear the sounds of the fork, which in the manner described was set in vibration by a second fork in a distant room. The two forks were König Ut_4; lower forks give less clearly heard tones; with higher forks I was unable to make any experiment, since I had not two similar ones at my disposal.

Let me mention two other experiments which I have made. The first is of importance in connection with the question as to how the clang-tints of tones are reproduced through the telephone. In one of the two telephones described substitute for the Ut_4 fork a higher one, and sound this by means of a fiddle-bow, and there will be heard with another inserted telephone of the ordinary construction tones of even 12,000 double vibrations per second, a sign that the variations of the magnetic condition of a magnet perceptibly occur, even when the forces producing these variations change their size 24,000 times in



a second. This result moreover was not to be expected, since, as is known, magnetic polarisation requires time to accomplish. Whether these higher tones are comparatively weaker than the deeper cannot be determined, but probably this is the case.

In another experiment I used the telephone to test the electric vibrations indicated by Helmholtz and others, which are produced by the opening of the primary current of an induction apparatus in the induced coil, when the ends of the latter are connected with the armatures of a condenser. For this purpose I inserted the telephone in the circuit between coil and condenser, and observed the effect when the current in the inducing spiral was opened.

When the ends of the induced spiral were not connected with the condenser, I heard a dull report in the telephone; when, again, these ends were connected with the condenser, this report was accompanied by a shorter, higher sound, whose vibration-number might perhaps be determined by a musical ear; a proof of the existence of the vibrations mentioned in the last case. The observations were made with a telephone, the iron membrane of which was very thin and had a very deep tone.

W. C. RÖNTGEN

THE NEW PARIS TRANSIT CIRCLE[1]

OF the numerous instruments with which Leverrier enriched the Paris Observatory during the twenty years of his direction, the last which he was able to see completely installed was the new transit circle. This instrument was not, like all the others, constructed at the expense of the State; an inscription on the marble pillars that support it informs the visitor that it was presented to the Observatory by the generous munificence of M. Raphaël Bischoffsheim. This is not the only gift of M. Bischoffsheim to astronomy; the Observatory of Lyons is also indebted to him for its fundamental instrument.

The project of erecting a new meridian circle at the observatory goes back to the time of the debate raised before the Academy of Sciences on the subject of the transfer of the observatory to a site outside Paris. Those who would not admit the legitimacy of the complaints made by the adversaries of the present situation of the observatory, were obliged to admit that the great meridian room, constructed in 1830 by Arago, did not offer any of the guarantees necessary to observations of great precision. The thickness of the walls and of the double roof of that room, the small breadth of the openings, the nearness of the observatory buildings, the difference of level between the two faces north and south, must necessarily affect the equilibrium of the neighbouring layers of the atmosphere and hinder them from taking that horizontality which admits of the correction of the observations from the influence of refraction.

Since the astronomer cannot get rid of this troublesome influence, his first business ought then to be to reduce it to conditions in which it may be possible to calculate the effect. Thus what strikes the visitor admitted to the new meridian circle of the Observatory is the small building in which it is placed. In the middle of a green lawn rises a hut made entirely of sheet-iron, the roof formed of two plates which, by sliding upon rollers, may be separated from each other, and leave all the upper part of the building open. The walls are formed of two envelopes of thin iron, between which the air freely circulates, thus maintaining the whole structure at the temperature of the air itself. Large windows may also be opened, and the observer and the instrument be thus placed in the same conditions as if the observations were made in the open air. All these conditions are to avoid as far as possible the disturbances arising from atmospheric refraction, the greatest source of inaccuracy in astronomical observation. The only obstacle which may yet be a hindrance to perfection in the conditions of observation is the presence of those beautiful trees which make the terrace of the observatory a magnificent garden, but which store up the warm air during the day and slowly distribute it during the night. No doubt some day the astronomers will be obliged to sacrifice to the precision of their observations the enjoyment of this beautiful foliage.

The meridian circle is composed, as its name indicates, of two instruments: the meridian telescope, intended, by its association with an astronomical clock, to fix the moment of the passage of a star across the meridian of the place of observation, and the mural circle, which gives the measure of the angular distance of this same star from the pole or the zenith. When, forty years ago, Gambey constructed the two meridian instruments of the Paris Observatory, so justly celebrated and on the model of which those of most other observatories have been designed, he had to reconcile, by prodigies of skill, the lightness resulting from the means of construction then in use, with the rigidity of the parts necessary for precision of observation. It is the alliance of these two almost contradictory qualities which renders so interesting the instruments of that celebrated artist and especially his machine for dividing the circles, which the Baron Séguier has restored in the galleries of the Conservatoire. But there resulted from this at first the necessity of separating the measure of the two co-ordinates of the stars—the instant of the meridian passage and the polar distance. There also resulted the necessity which Gambey was under to fix on his mural circle of two metres in diameter, a telescope altogether insufficient in optical power.

A simple glance at the great meridian circle of the observatory, the western equatorial, the great telescope, the new instrument of M. Bischoffsheim, all from the workshop of the great mechanician, M. Eichens, shows the revolution which has been effected in the processes of construction. In place of instruments formed of pieces of sheet brass connected by simple screws or even soldered together, we have the bodies of the telescope of cast-iron bolted on axes of cast-iron and steel, strong and elegant in appearance; circles of bronze cast in a single piece and protected against all deformation by numerous cross-bars. It is the art of the engineer applied to the construction of astronomical instruments, with the power given by the choice of metals and the thickness of pieces, and the precision which the employment of engineering tools secures.

This revolution was begun in England about 1847 by the Astronomer-Royal, Sir George Airy. In 1863, M. Leverrier successfully installed a meridian circle greater still than that of Greenwich, and intended, like it, for the observation of the small planets. But these gigantic instruments, veritable siege-guns of long range, since they reach the farthest depths of the heavens, want, simply on account of their weight, one essential quality—they are not reversible. Whatever be the rigidity of the pieces, the instrument is subject, in each successive position, to flexions necessarily unequal, which the astronomer must investigate and measure in order to correct his observations. But this investigation and this measurement can only be made by turning round the instrument. It will be understood, in fact, that the apparatus, directed successively to the same point of the sky, first with one of its faces up, then the same face below, gives, if it is really perfectly rigid but elastic, two results differing equally from the truth, one *minus* and the other *plus*, so that the mean of the two observations gives the exact position of the star. It is this which may be expected from the new meridian circle of M. Bischoffsheim. Fig. 1 represents the telescope upon its car, which serves to raise it above its pillars and to turn it right round by a movement of rotation around a vertical axis.

Since 1852 M. Brunner has constructed small portable instruments answering to these conditions. Improved by his sons, by M. Rigaud, and by M. Eichens, these meridian circles are now only used in geodesic expeditions. In 1868 M. Eichens constructed for the observatory of Lima a reversible meridian circle, the telescope of which was 2·30 m. in length, and the object-glass 20 cm. in free opening. It is this model, successively improved, which has become, in the hands of the able constructor, the meridian circle of Marseilles (1876), and the circle given by M. Bischoffsheim (1877). The object-glass of the first was made by Léon Foucault, the two others are by M. Ad. Martin. The new observatory of Lyons, in the establishment of which M. André took an active part energetically sustained by the Administration, will soon possess a similar meridian circle, a little smaller (telescope of 2 m., object-glass of 14 cm. aperture, by M. Praczmowzki), the expense of which is borne by M. Bischoffsheim.

The illustrations which we give then show the perfected model meridian circle employed in observatories for the determination of the celestial co-ordinates of the stars. To be able to understand the use of the various parts of the instrument, it will suffice to describe a complete observation of a star.

Some minutes before the passage of the star across the meridian, the astronomer gives to the telescope such

[1] From an article in *La Nature* by M. C. Wolf.

an inclination that the star, carried on by the daily movement, will cross the field of the instrument. For this purpose the interior circles fixed on the axis of the telescope carry a rough scale which may be seen by means of a pointer telescope fixed on the east wall. A clamp which clasps the edge of this circle serves to fix the instrument. The observer then places himself on the observing chair in the position indicated on Fig. 2. The star soon appears, enters the field of view on the west and proceeds towards the east side. With the star the observer sees in the field of view a network of spider threads stretched vertically and traversed by a horizontal thread. Listening to the beats of the clock, he notes the second and the fraction of a second at which the star passes under each of the vertical threads; the mean of these times is the precise moment of the passage across the middle thread. At this same moment he slightly displaces the telescope by a

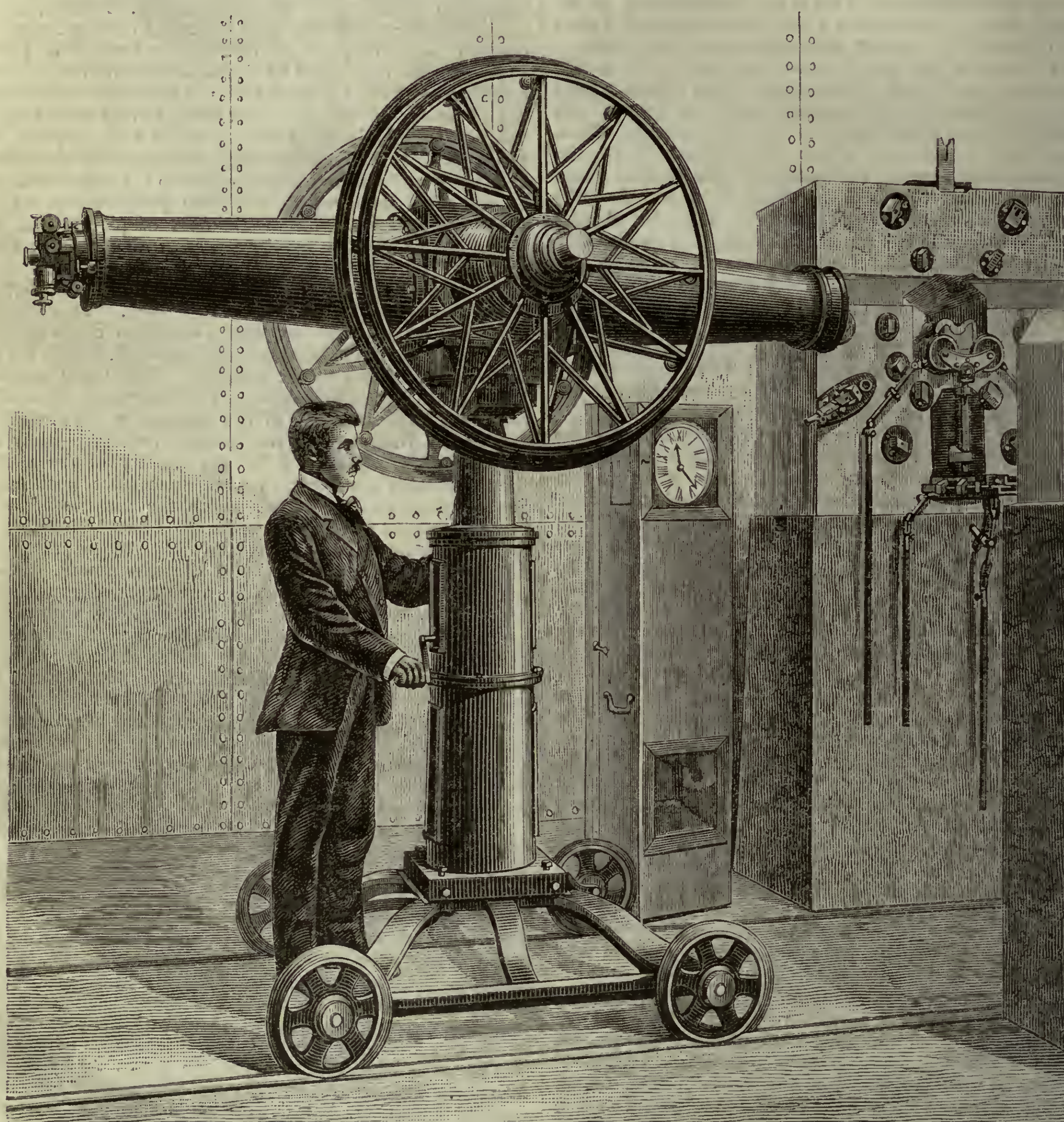


FIG. 1.—Reversing Apparatus.

movement given to the clamp and brings the star under the horizontal thread. The direction of a line determined by the crossing of this thread and the middle vertical one and the optical centre of the object-glass is that along which the star is seen at the moment of its passage across the middle thread. To fix this direction it is necessary to connect it with two points of an absolute fixity. For this purpose the telescope is provided with a circle of a metre in diameter, the limb of which is very finely and very exactly divided; this turns with the telescope in front of six microscopes permanently fixed to the east pillar. M. Eichens has adopted for these microscopes the arrangement devised by Sir George Airy for the meridian circle of Greenwich. The tube of each of these is formed by the side of a hole pierced in the block of marble which forms the upper part of the pillar; the positions of these microscopes is then permanently fixed to that of the wall,

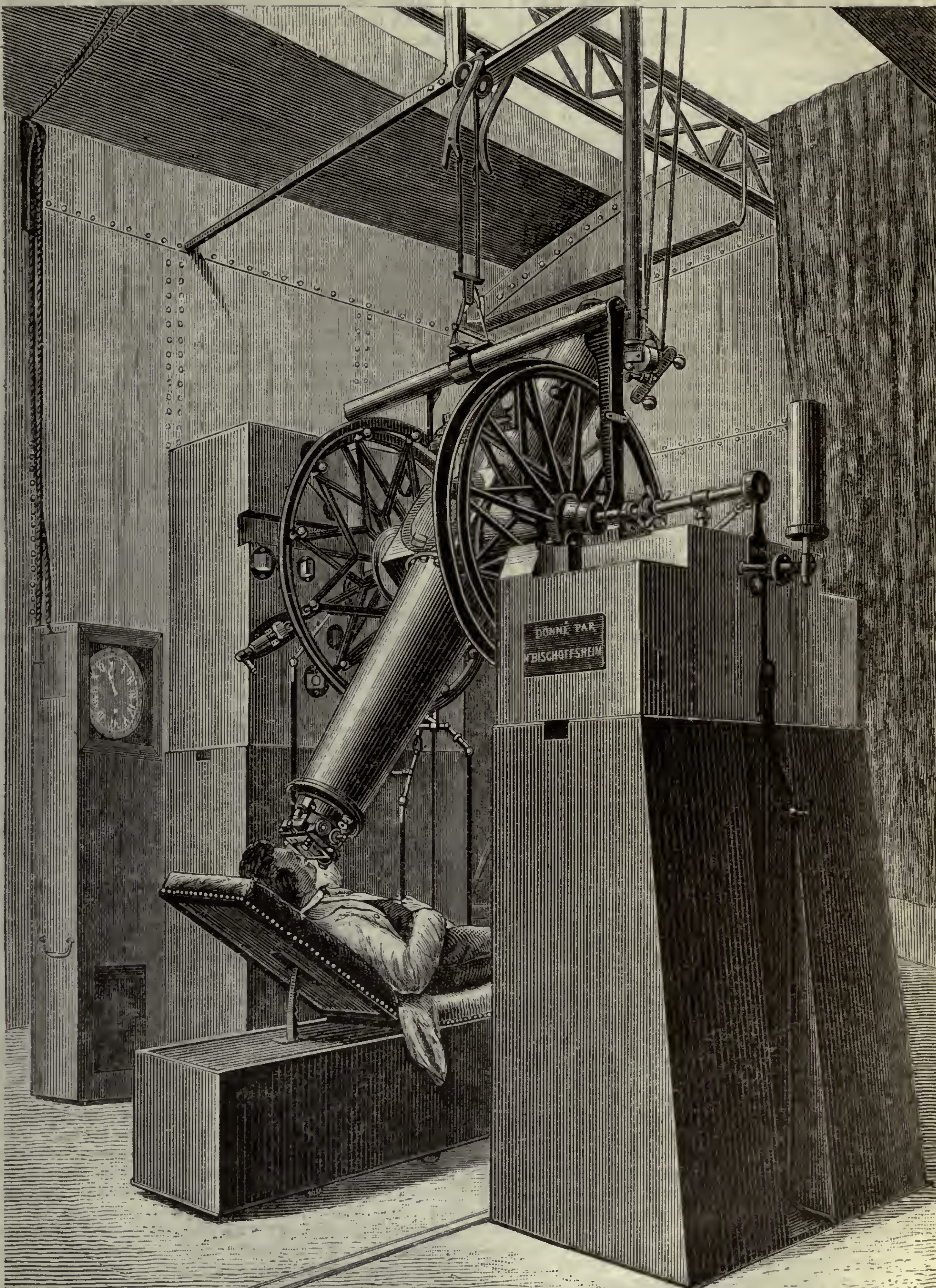


Fig. 2.—Taking an Observation

and can only change by a displacement of the wall itself. Other orifices admit to the circle the light of a lamp and enable the divisions to be read. These are drawn at every five minutes of the circle, which then bears 4,320 equidistant marks; each microscope is provided with a micrometer which enables the tenths of a second of arc to be observed.

If now, by observations of the pole star at its upper and lower transits, the observer determines in the same way the direction of the telescope looking to the pole, the angle comprised between that direction and that of the telescope directed to the star will give the polar distance of the star. If by means of a mercury bath he determines the direction of the telescope when its optical axis is vertical, he will ascertain in the same way the distance of the star from the zenith.

These observations may be made in the two positions which the telescope takes before and after being turned round. This is why it carries two cast-iron circles roughly graduated and two brass circles finely graduated on silver, which on the reversal of the instrument are substituted for each other before the pointer-telescope and the fixed microscopes. The arrangement of these circles insures a perfect symmetry to the instrument, an essential condition if we wish to prevent irregular deformations.

But these operations will only give the co-ordinates of the star if they are made with an instrument set in the meridian of the place. It is necessary then that the telescope should turn round a horizontal axis, that it should be perpendicular to that axis, and that the plane which it describes in turning should pass through the pole of the earth. A level, which the illustration represents resting by two forks upon the pivots of the telescope, but which during the observations is raised by means of a crane fixed to the ceiling, serves to measure and correct the inclination of the axis of rotation. By turning it upon a long support the perpendicularity of the optical axis on the axis of the pivots can be assured. Two supports are to be constructed, one on the north, the other on the south; the latter only has been made. Finally the astronomical observation of the pole star indicate if the last of the three conditions is fulfilled.

A word on the illumination of the system of cross wires visible in the eye-piece. During the day they stand out on the clear background of the sky; at night the same effect is obtained by means of a ray of light proceeding from a gas-lamp fixed on the west pillar, the rays of which are sent towards the eye-piece by a small prism fixed in the middle of the telescope. A screen with a variable opening, or cat's-eye, permits the intensity of the light to be proportioned to the brightness of the star observed. Finally, for very weak stars a very simple mechanical arrangement suppresses all light in the field, and brings it to bear on the wires, which appear as luminous lines on a background absolutely dark.

The long illness of M. Leverrier did not permit him to push on, so actively as he would have wished, the preliminary investigations of this beautiful instrument, among which we must mention one, long and difficult —the divisions of the two circles. It will, without doubt, be facilitated by this circumstance, that, traced by means of the dividing machine constructed by M. Eichens, the lines present a regularity and a finish altogether favourable to precision.

The astronomers of the observatory will hold it a point of honour to take advantage as soon as possible of the magnificent apparatus which they owe to the generosity of M. Bischoffsheim.

FETICHISM IN ANIMALS

MR. HERBERT SPENCER, in his recently published work on the "Principles of Sociology," treats of the above subject. He says: "I believe M. Comte expressed the opinion that fetichistic conceptions are formed by the higher animals. Holding, as I have given reasons for doing, that fetichism is not original but derived, I cannot, of course, coincide in this view. Nevertheless, I think the behaviour of intelligent animals elucidates the genesis of it. I have myself witnessed, in dogs, two illustrative cases." One of these cases consisted in a large dog, which, while playing with a stick, accidentally thrust one end of it against his palate, when, "giving a yelp, he dropped the stick, rushed to a distance from it, and betrayed a consternation which was particularly laughable in so ferocious-looking a creature. Only after cautious approaches and much hesitation was he induced again to lay hold of the stick. This behaviour showed very clearly the fact that the stick, while displaying none but the properties he was familiar with, was not regarded by him as an active agent, but that when it suddenly inflicted a pain in a way never before experienced from an inanimate object, he was led for the moment to class it with animate objects, and to regard it as capable of again doing him injury. Similarly in the mind of the primitive man, knowing scarcely more of natural causation than a dog, the anomalous behaviour of an object previously classed as inanimate, suggests animation. The idea of voluntary action is made nascent; and there arises a tendency to regard the object with alarm, lest it should act in some other unexpected and perhaps mischievous way. The vague notion of animation thus aroused will obviously become a more definite notion, as fast as development of the ghost-theory furnishes a specific agency to which the anomalous behaviour can be ascribed."

The other case observed by Mr. Spencer was that of an intelligent retriever. Being by her duties as a retriever led to associate the fetching of game with the pleasure of the person to whom she brought it, this had become in her mind an act of propitiation; and so, "after wagging her tail and grinning, she would perform this act of propitiation as nearly as practicable in the absence of a dead bird. Seeking about, she would pick up a dead leaf or other small object, and would bring it with renewed manifestations of friendliness. Some kindred state of mind it is which, I believe, prompts the savage to certain fetichistic observances of an anomalous kind."

These observations remind me of several experiments which I made some years ago on this subject, and which are perhaps worth publishing. I was led to make the experiments by reading the instance given in the "Descent of Man," of the large dog which Mr. Darwin observed to bark at a parasol as it was moved along a lawn by the wind—so presenting the appearance of animation. The dog on which I experimented was a Skye terrier—a remarkably intelligent animal, whose psychological faculties have already formed the subject of several communications to this and other periodicals.[1] As all my experiments yielded the same results I will only mention one. The terrier in question, like many other dogs, used to play with dry bones by tossing them in the air, throwing them to a distance, and generally giving them the appearance of animation, in order to give himself the ideal pleasure of worrying them. On one occasion, therefore, I tied a long and fine thread to a dry bone and gave him the latter to play with. After he had tossed it about for a short time I took an opportunity when it had fallen at a distance from him and while he was following it up, of gently drawing it away from him by means of the long and invisible thread. Instantly his whole demeanour changed. The bone which he had previously pretended to be alive now began to look as if it really were alive, and his astonishment knew no bounds. He first approached it with nervous caution, as Mr. Spencer describes, but as the slow receding motion continued, and

[1] See especially an article on "Conscience in Animals," in *Quarterly Journal of Science* for April, 1876.

he became quite certain that the movement could not be accounted for by any residuum of the force which he had himself communicated, his astonishment developed into dread, and he ran to conceal himself under some articles of furniture, there to behold at a distance the "uncanny" spectacle of a dry bone coming to life.

Now in this, and in all my other experiments, I have no doubt that the behaviour of the terrier arose from his *sense of the mysterious*, for he was of a highly pugnacious disposition, and never hesitated to fight an animal of any size or ferocity; but apparent symptoms of spontaneity in an inanimate object which he knew so well, gave rise to feelings of awe and horror which quite enervated him. And that there was nothing *fetichistic* in these feelings may be safely concluded if we reflect, with Mr. Spencer, that the dog's knowledge of causation, for all immediate purposes, being quite as correct and no less stereotyped than is that of "primitive man," when an object of a class which he knew from uniform past experience to be inanimate suddenly began to move, he must have felt the same oppressive and alarming sense of the mysterious which uncultured persons feel under similar circumstances. But further, in the case of this terrier we are not left with *à priori* inferences alone to settle this point, for another experiment proved that the sense of the mysterious was in this animal sufficiently strong of itself to account for his behaviour. Taking him into a carpeted room I blew a soap-bubble, and by means of a fitful draught made it intermittently glide along the floor. He became at once intensely interested, but seemed unable to decide whether or not the filmy object was alive. At first he was very cautious and followed it only at a distance, but as I encouraged him to examine the bubble more closely, he approached it with ears erect and tail down, evidently with much misgiving; and the moment it happened to move he again retreated. After a time, however, during which I always kept at least one bubble on the carpet, he began to gain more courage, and the scientific spirit overcoming his sense of the mysterious, he eventually became bold enough slowly to approach one of the bubbles and nervously to touch it with his paw. The bubble, of course, immediately vanished; and I certainly never saw astonishment more strongly depicted. On then blowing another bubble, I could not persuade him to approach it for a good while; but at last he came and carefully extended his paw as before with the same result. But after this second trial nothing would induce him again to approach a bubble, and on pressing him he ran out of the room, which no coaxing would persuade him to re-enter.

One other example will suffice to show how strongly developed was the sense of the mysterious in this animal. When alone with him in a room I once purposely tried the effect on him of making a series of horrible grimaces. At first he thought I was only making fun; but as I persistently disregarded his caresses and whining while I continued unnaturally to distort my features, he became alarmed and slunk away under some furniture, shivering like a frightened child. He remained in this condition till some other member of the family happened to enter the room, when he emerged from his hiding-place in great joy at seeing me again in my right mind. In this experiment, of course, I refrained from making any sounds or gesticulations, lest he might think I was angry. His actions, therefore, can only be explained by his horrified surprise at my apparently irrational behaviour—*i.e.*, by the violation of his ideas of uniformity in matters psychological. It must be added, however, that I have tried the same experiment on less intelligent and less sensitive terriers with no other effect than causing them to bark at me.

I will only add that I believe the sense of the mysterious to be the cause of the dread which many animals show of *thunder*. I am led to think this, because I once had a setter which never heard thunder till he was eighteen months old, and on then first hearing it I thought he was about to die of fright, as I have seen other animals do under various circumstances. And so strong was the impression which his extreme terror left behind, that whenever afterwards he heard the boom of distant artillery practice, mistaking it for thunder, he became a pitiable object to look at, and, if out shooting, would immediately bolt home—or, if at a great distance from home, would endeavour to bury himself. After having heard real thunder on two or three subsequent occasions, his dread of the distant cannons became greater than ever; so that eventually, though he keenly enjoyed sport, nothing would induce him to leave his kennel, lest the practice might begin when he was at a distance from home. But the keeper, who had a large experience in the training of dogs, assured me that if I allowed this one to be taken to the battery, in order that he might learn the true cause of the thunder-like noise, he would again become serviceable in the field. The animal, however, died before the experiment was made. GEORGE J. ROMANES

RUHMKORFF

WE regret to record the sudden death on December 20, at Paris, of Henry Daniel Ruhmkorff, whose name is so closely connected with the history of magneto-electricity. He was born in Hanover, Germany, in 1803, and but little is known of his early life. In 1819 he wandered to Paris, and obtained a position as porter in the laboratory of Prof. Charles Chevalier, at that time one of the leading French physicists. Here he displayed a remarkable fondness for electrical apparatus, as well as ingenuity in its arrangement, and was enabled shortly after to start a modest manufactory of physical apparatus. Through the efforts of Chevalier and the excellence of the work performed, the business was rapidly extended. In 1844 Ruhmkorff brought out his first invention, a convenient thermo-electric battery. Soon after he turned his attention to magneto-electricity, especially the production of the induced currents, discovered by Faraday in 1832. A long series of experiments resulted in the appearance, in 1851, of the famous "Ruhmkorff coil," with its later modifications, the most important piece of apparatus in this branch of physics. With this powerful adjunct the electrician was enabled to obtain sparks 18 inches in length, pierce thick plates of glass, and carry out a vast variety of experiments. The invention was rewarded by a decoration and medal at the Exhibition of 1855, while in 1858 it received the first prize of 50,000 francs at the French Exhibition of Electrical Apparatus. Since then the manufacture of the coils and of electrical machines in general has assumed enormous dimensions, and the leading physicists of Europe are well acquainted with the dingy little bureau in the Rue Champollion, near the University. Personally M. Ruhmkorff was of a quiet, dignified appearance, and despite the disadvantages of his early life, he enjoyed the friendship of the leading Parisian *savants*, and was an honoured member of the French Physical Society. M. Jamin delivered an address over the grave, in which he stated that Ruhmkorff died almost a poor man, because he had spent all his earnings on behalf of science and in works of benevolence.

LIQUEFACTION OF OXYGEN

THE number of the permanent gases is rapidly diminishing. We have had occasion recently to refer to M. Cailletet's successful attempts to compress nitric oxide, N_2O_2, methyl hydride, CH_4, and acetylene, C_2H_2, to the liquid form. The list of non-compressible gases was thus reduced to three, viz., hydrogen, nitrogen, and oxygen. Within the past week M. Raoul Pictet has succeeded in obtaining the last-mentioned gas in the liquid state, an event which is certainly one of the most

novel and interesting in the chemical progress of the expiring year.

The *Journal de Genève* of December 23 gives the following account of the experiments :—

One of the most interesting physical experiments of our time has just been made at Geneva with rare success in the laboratory of the Society for the Manufacture of Physical Instruments. M. Raoul Pictet has succeeded in obtaining, by means of ingeniously combined apparatus, the liquefaction of oxygen gas. The following is the process by which the curious result was obtained :—

By a double circulation of sulphurous acid and carbonic acid, the latter gas is liquefied at a temperature of 65° of cold, under a pressure of from four to six atmospheres. The liquefied carbonic acid is conducted into a tube four metres long; two combined pumps produce a barometric vacuum over the acid which is solidified in consequence of the difference of pressure. Into the interior of this first tube containing solidified carbonic acid is passed a tube of a slightly less diameter, in which circulates a current of oxygen produced in a generator containing chlorate of potash and the form of which is that of a large shell thick enough to prevent all danger of explosion. The pressure may thus be carried to 800 atmospheres.

Yesterday morning (December 22), all the apparatus being arranged as described, and under a pressure which did not exceed 300 atmospheres, a liquid jet of oxygen issued from the extremity of the tube, at the moment when this compressed and refrigerated gas passed from that high pressure to the pressure of the atmosphere.

The great scientific interest of this experiment is that it demonstrates experimentally the truth of the mechanical theory of heat, by establishing that all gases are vapours capable of passing through the three states—solid, liquid, and gaseous. Only twenty days ago M. Cailletet, as we have said, succeeded in liquefying the bioxide of nitrogen, under a pressure of 146 atmospheres and at a temperature of 11° of cold. After the experiment of M. Raoul Pictet there remain not more than two elemental gases which have hitherto escaped the attempt at liquefaction—hydrogen and nitrogen.

The experiment above described was to be repeated on Monday and subsequent days, with some slight changes in the processes and the arrangement of the apparatus.

NOTES

SOME interesting experiments with the telephone have been made by Mr. W. H. Preece between Dublin and Holyhead through the submarine cable. Conversation was freely maintained and songs were sung on each side and heard and appreciated on the other. The articulation was excellent, but muffled, as though the speakers spoke through respirators. This is what might have been expected from the static induction of the cable. It is the longest actual cable yet spoken through, its length being sixty-seven miles.

AT their last sitting the enlarged Council of the Paris Observatory were occupied in considering the question of the position of French meteorology. M. Dumesnil, the representative of the minister, was obliged to silence some members of the minority who were assailing the character of some of the physicists having the control of the Observatory and the transmission of the warnings to the sea-ports. A large majority rendering justice to the ingenuity displayed and to the highly scientific nature of the warnings, passed a vote recommending the administration not to alter the present condition of things at the Observatory.

DR. CARLO GHINOZZI, Professor of Medical Clinic at the Istituto Superiore of Florence, for many years colleague and afterwards successor of Prof. Bufalini, died on Saturday, the 15th instant, at the age of 66 years.

IN Bonn a committee has been formed consisting of leading citizens and Professors Bauerband, Kekulé, and Proschel, of the University, for the purpose of erecting a monument to the late Prof. Jacob Noeggerath, whose death last September we briefly alluded to at the time. Prof. Noeggerath was born in Bonn October 10, 1788, and since the foundation of the university in 1818 had been connected with it as Professor of Mineralogy. As a successful teacher of the natural sciences he acquired an unusually widespread fame, and the majority of the present Prussian mining officials pursued their studies under his direction. His general scientific researches touch on a number of interesting geological questions, such as the formation of basalt, &c.; but his chief efforts were directed to an exhaustive study of the mineralogy and geology of Rhenish Westphalia, the results of which are to be seen in the magnificent mineralogical collection at Bonn, and the rapid development of the mining interests in this district. As a favourite writer of popular works on scientific subjects, he contributed in no small degree to the general taste for this class of literature now prevalent in Germany.

THE expedition sent out by the Dutch Geographical Society for the exploration of Sumatra has met with a severe check by the sudden death of its leader, M. Schouw Landvort. His extensive knowledge, indomitable perseverance, and great powers of endurance, fitted him eminently for the position, these qualitie being notably evidenced by the bold journey across the middle of the island, through hitherto unknown regions, in the company of natives only, which we had occasion lately to chronicle.

AT the meeting of the Council of the Zoological Society on Wednesday last week, the president, the Marquis of Tweeddale, proposed that the silver medal of the Society should be awarded to Mr. Robert Hudson, F.R.S., in acknowledgment of the valuable services he had rendered to the Society for the fifty years that he had been a Fellow thereof. The motion was carried unanimously at the full meeting of the Council.

THE organisation of public instruction in France is undergoing an exceedingly beneficial change. A decree, published in the *Journal Officiel* of December 17, establishes a representative Council of Public Instruction under the title of "Comité Consultatif." The committee is divided into three different sections corresponding to the three divisions of public instruction in France, primary, secondary (grammar schools), and superior (universities). Each section is to appoint its president and secretary. The three sections in general session are to be presided over by the minister. Some of the members are appointed by the minister to serve during a period of five years, others are members *ex officio*. The minister cannot elect any who are not members of the teaching body or of the Institut. The directors of the administration of primary, secondary, or superior instruction are *ex officio* members of their respective sections. They meet yearly at a certain fixed period. The opinion of the committee is not binding, but it must be taken on a number of matters, such as bills which are to be presented to Parliament, modification of programmes, &c. Another decree appoints the members of the three committees. Among these are many names well-known to science, as MM. Laboulaye, Würtz, Claude Bernard, Vulpian, Gavarret, Chevreul, Faye, Berthelot, Milne-Edwards, Puiseux, and Desains.

THE following are the probable arrangements for the Friday Evening Meetings at the Royal Institution, before Easter, 1878:—January 25, Prof. Huxley, F.R.S., "William Harvey;" February 1, Wm. Henry Preece, C.E., "The Telephone;" February 8, Matthew Arnold, "Equality;" February 15, P. L. Sclater, F.R.S., "Zoological Distribution and some of its Difficulties;" February 22, Prof. Roscoe, F.R.S.; March 1, Richard Liebreich, M.D., "The Deterioration of Oil Paintings;" March 8, Prof. Goldwin Smith, "The Influence of

Geographical Circumstances on Political Character;" March 15, Lord Rayleigh, F.R.S.; March 22, Prof. Tyndall, F.R.S.; March 29, Prof. Dewar, F.R.S.; April 5, Sir John Lubbock, Bart., M.P., F.R.S.; April 12, Sir Joseph Dalton Hooker, C.B., Pres. R.S., "The Distribution of Plants in North America."

PROF. BARFF begins his juvenile lectures at the Society of Arts next Wednesday. His subject is "Coal and its Components."

VOLCANIC eruptions are threatening Iceland again. The last number of the *Skuld*, published in Eskifjödur, states that on the evening previous an unprecedented heat was suddenly felt, so strong that the inhabitants thought themselves in the vicinity of a vast conflagration. The phenomenon was followed by alternate gusts of rain and showers of volcanic ashes accompanied by subterranean rumblings.

THE German Government has lately named a new steamer after the well-known meteorologist, Prof. Dove, of Berlin, in recognition of the advantages accruing to navigation from his many observations and discoveries.

THE Italian Geographical Society has received news from Signori Martini and Cecchi, who have penetrated into Shoa. There is no intelligence of the Marquess Antinori and the engineer Chiarini, whose fate causes grave anxiety.

THE Geographical Society of Paris held a banquet last Saturday to commemorate the fifty-seventh anniversary of its foundation. Among the toasts which were given we must notice that of Mr. Gordon Bennett, the enterprising director of the *New York Herald*, who originated Stanley's fruitful mission, and the King of the Belgians, by MM. Levasseur and de Lesseps.

NEW halls of exhibition for antiquities have been opened in the Louvre. An interesting anthropological exhibition will be opened on January 15 at the Palais de l'Industrie. It will be confined to the discoveries made in South America by the several scientific missionaries sent to that region by the French government. The exhibition will be open only till March 1.

WE have received from Messrs. De la Rue and Co. some specimens of their exquisitely-printed Indelible Diaries, Pocket Diaries, Memorandum Books, and Calendars for the coming year. Our readers have doubtless already supplied themselves with one or other of these. If not, the following statement will recommend the Pocket Diary to every lover of science:—We not only find everything that one finds generally in such a pocket companion, but, under the careful editorship of Mr. Godward, the amateur astronomer is supplied with information as to astronomical phenomena, including the times of rising, southing, and setting of the five principal planets, and the illuminated discs of Venus and Mars, and occultations visible at Greenwich. The physiographer finds meteorological averages of mean temperature, rainfall, and barometer, hints as to weather forecasts, and the magnetic elements. Physical data are not forgotten, and the conversion of metric measures into British inches and centigrade readings into Fahrenheit are given. The geographer and statistician have also facts stored up for them which will certainly be often referred to in the course of the 8,000 odd hours which make up the year. One thing, and one thing only, we miss—the old three-page article and exquisite steel engraving which brought home to everybody the latest thing of mark in the progress of the sciences of observation.

WE learn with pleasure in perusing the last pamphlets sent to us by Capt. Howgate on his intended Polar Colony, that the use of small pilot balloons has been recommended to Mr. Sherman, the meteorologist of the preliminary *Florence* expedition. The method practised by M. de Fonvielle in the beginning of 1877 at Secretan's workshop for ascertaining the altitude of clouds and the direction of the winds by throwing ballonets into the air, has been improved upon in America and will be used regularly in arctic work. This success has led MM. de Fonvielle and Secretan to prepare instructions for the above purposes, in the hope of extending the use of these ballonets to the bringing of news from ships in danger or expeditions severed from the civilised world either by sandy wastes or icy solitudes. A number of examples cited in recent works on ballooning may be regarded as an indication that the old mode of throwing bottles into the sea may be replaced by a new method equally simple and having at least a thousand more chances of success.

CAPT. HOWGATE's scheme for Polar colonisation has been brought before the Council of the Paris Geographical Society, and it is expected that a resolution favourable to the contemplated expedition will be adopted in time to be sent to America before Congress has come to a final decision on that important object.

AN interesting discussion arose at the last meeting of the Anthropological Institute, on the contents of the small oval pits which have been discovered in the neighbourhood of some of the shafts at Cissbury. The president, Mr. John Evans, pointed out marks on the bone of a small ruminant, probably a roebuck, which indicated that it had been used in the process of weaving. A carding-comb, a terra-cotta bead, large enough to serve as a spindle-whorl, and a loom-weight of chalk were found in the same pit. Lord Rosehill mentioned that chalk weights were also met with in Mr. Tindale's pit at Cissbury, and some were now in his museum. Mr. Park Harrison was of opinion that the little pits were graves, but they appeared to have been disturbed at a remoter period and used for more than one interment. The potsherds found in them were of various dates, some being of a type more common on the Continent than in this country.

WE notice the appearance of the first two of the three divisions of the *Jahresbericht für Chemie* for 1876, which completes the report of physical, inorganic, organic, vegetable, and physiological chemistry, leaving the analytical, technical, mineralogical, and geological portions for the closing number. Prof. Fittica, of Marburg, is still editor-in-chief, and he is assisted by C. Böttinger, C. Hell, H. Klinger, A. Laubenheimer, E. Ludwig, A. Naumann, F. Nies, H. Salkowski, Z. H. Skraup, K. Zöppritz, G. Schultz, and W. Weyl, the latter two replacing K. Birnbaum and A. Michaelis in the editorial *corps* of the preceding year. The publication of the *Jahresbericht* has been much more prompt since the appearance of Prof. Staedel's *Jahresbericht für die reine Chemie* in 1873, which although confined exclusively to pure chemistry, renders a tolerably complete report for each year in the following September.

THE two last numbers of the *Isvestia* of the Russian Geographical Society contain a very interesting account, by Dr. Wojeikoff, of his travels in Japan, made during the summer of last year. Besides a vivid description of the country visited, and of its inhabitants, the reader will find in these papers many interesting data as to the physical characteristics of the land, with many determinations of heights, the climate, the products, &c. Two separate papers are devoted, one to the exterior trade of Japan, and the other to the population and its dependence upon agriculture, as compared with other countries.

THE Moscow Society of Friends of Natural Science has undertaken various anthropological researches for the exhibition which will take place at Moscow in 1879. One of them was made in the Ryazan government by M. Néfédoff, who has already discovered and excavated ten unknown and very interesting *koorganes* (mounds) in Kasimov district. He has found there

eleven human skeletons with many ornaments, some of them in bronze, representing snakes, heads of various animals, &c.; and a comparison of the Ryazan skulls and ornaments with those excavated in the Moscow and Meriaks *koorganes*, proves that they belong to quite a different people. Altogether the discovery promises to be of great importance. Another gentleman sent by the same society, M. Bensengr is busily engaged in making anthropological measurements and ethnographical descriptions of the Ryazan Tartars.

AT the meeting of the St. Petersburg Society of Naturalists on December 9, M. Polyakoff—returned from a journey to Western Siberia, the Altai, and Alatan Mountains—read a report on the interesting question as to the state of Central Asia during the glacial period. After having described the boulder-clays, boulders, and morainic deposits he met with during his journey, as well as the present characters of the flora and fauna of the country, he concluded in favour of a complete glaciation of Central Asia during the last ice-period.

WE notice a valuable Russian work, just published by M. Mushketoff, "Materials for a Knowledge of the Geology and of the Mines of the Zlatoust Mine District in Southern Ural." It is the result of careful study, contains many new and valuable data, and is accompanied by an elaborate geological map.

AT the last meeting of the Russian Geographical Society on December 8, Prof. Ujfalvy, of the Paris High School of Eastern Languages, who was sent by the French Government on an anthropological mission to Central Asia, made a very interesting communication on his work in the Russian provinces of Orenburg, Fergana, and Turkistan. After a careful study of the Bashkirs, he arrived at the conclusion that this people are the original stock of the Madjars; that the Mescheryacks are intermediate between Bashkirs and Ostyacks, and that the Tepteri are true Tartars. The conclusions arrived at as to the various peoples of Turkistan are more complicated and could not be briefly stated; but the learned professor has collected many important data, and has obtained valuable photographs, collections of old coins from Turkistan, of stone implements from Siberia, &c.—At the same meeting M. Minaieff referred to the work he has compiled, by order of the society, on the tracts of Central Asia occupying the upper parts of the Amu-daria. The work is divided into three parts: geographical, ethnographical, and linguistic, the former being the richest, and sums up all we know at present about those lands.

COL. GORDON has lately entered into a contract with Messrs. Yarrow and Co., of Poplar, for four steel steamers of small draught. He intends exploring the Albert Nyanza and the rivers flowing into it. The steamers are to be carried as far as possible by water, and are to be composed of several portable pieces of about 200 lbs. each, to be put together on arrival at their destination. Col. Gordon and his party are reported to be in good health.

SINCE the beginning of last year a new scientific journal has appeared at Christiania (Cammermeyer) under the title *Archiv for Mathematik og Naturvidenskab*. It is edited by Herrer Sophus Lie, Jakob Worm Müller, and G. O. Sars. The journal is published in four yearly parts which form a volume of about 500 pages. We have received the first seven parts, and may congratulate the editors and publishers on the decided step of progress which the appearance of this journal evidently marks in the history of Norwegian science. Amongst a number of mathematical papers by Herr Sophus Lie, and others of minor interest, there are some interesting geological treatises by Herr Karl Pettersen, viz., on the orography of Norway, on the geology of the Salten fjord, on the giant's cave near the Lavangen fjord in the neighbourhood of Sandvort, and on the fjords of Northern Norway. Herr S. A. Sexe has contributed two papers on some old coast-lines and on the direction of the winds in the so-called "stille Belt." Herr Amund Helland is the author of a treatise on the ice-filled fjords of Northern Greenland, and of an elaborate account of the varying quantities of chlorine present in the sea-water of the German Ocean, the Atlantic, and Davis' Straits. Herr G. O. Sars contributes an interesting note on the scientific expeditions in the Atlantic during 1876, and some detailed researches on the invertebrate fauna of the Mediterranean (with plates.) Herr J. Worm Müller gives some notes on Malassez's method of estimating the number of red corpuscles in blood as well as on the relation between the number of red corpuscles and the colouring power of blood. Of the remaining papers we note—a metallurgical paper by E. Münster; on the influence of the eccentricity of the orbits of heavenly bodies upon the quantity of heat they receive from the sun, by H. Geelmuyden; and two zoological notes, one by J. Koren and D. C. Danielssen, the other by Herman Friele.

THE additions to the Zoological Society's Gardens during the past week include a Greater Sulphur-Crested Cockatoo (*Cacatua galerita*) from Australia, presented by Miss Rosetta Cohen; a Grey-breasted Parrakeet (*Bolborhynchus monachus*) from Monte Video, presented by Mr. Alex. F. Baillie; a Mocassin Snake (*Tropidonotus fasciatus*), born in the Gardens.

CERTAIN MOVEMENTS OF RADIOMETERS[1]

NEARLY two years ago Mr. Crookes was so good as to present me with two of his beautiful radiometers of different constructions, the discs of one being made of pith, and those of the other of roasted mica, in each case blackened with lampblack on one face. With these I was enabled to make some experiments, having relation to their apparently anomalous movements under certain circumstances, which were very interesting to myself, although the facts are only such as have already presented themselves to Mr. Crookes, either in the actual form in which I witnessed them, or in one closely analogous, and have mostly been described by him. Although it will be necessary for me to describe the actual experiments, which have all been repeated over and over again so as to make sure of the results, I do not bring forward the facts as new. My object is rather to endeavour to co-ordinate them, and point to the conclusions to which they appear to lead.

I do not pretend that these conclusions are established; I am well aware that they need to be further confronted with observation; but as I have not leisure to engage in a series of experiments which would demand the expenditure of a good deal of time, and have lately been urged by a friend to publish my views, I venture to lay them before the Royal Society, in hopes that they may be of some use, even if only in the way of stimulating inquiry.

In describing my experiments I will designate that direction of rotation in which the white face precedes as positive, and the reverse as negative. It will be remembered that, under ordinary circumstances, radiation towards either radiometer produces positive rotation.

1. If a glass tumbler be heated to the temperature of boiling water, and inverted over the mica radiometer, there is little or no *immediate* motion of the fly, but quickly a *negative* rotation sets in, feeble at first, but rapidly becoming lively, and presently dying away.

2. If after the fly has come to rest the hot tumbler be removed, a *positive* rotation soon sets in, which becomes pretty lively, and then gradually dies away as the apparatus cools.

3. If the tumbler be heated to a somewhat higher temperature, on first inverting it over the radiometer there is a *slight positive* rotation, commencing with the promptitude usual in the case of a feeble luminous radiation, but quickly succeeded by the negative rotation already described. If the tumbler be heated still more highly, the initial positive rotation is stronger, and lasts longer, and the subsequent negative rotation is tardy and feeble.

4. If the pith radiometer be treated as in § 1, the result is the same, with the remarkable difference that the rotation is positive instead of negative; it is also less lively.

[1] Paper read at the Royal Society, December 20, by Prof. G. G. Stokes, Sec. R.S.

5. But if the tumbler be removed when the fly has come to rest, it remains at rest, or nearly so.

6. If the tumbler be more strongly heated, positive rotation begins as promptly as with light. In this case the tumbler must not be left long over the radiometer, for fear the vacuum should be spoiled by the evolution of gas from the pith.

7. If the tumbler be heated by holding it over the spout of a kettle from which steam is issuing, and held there till the condensation of water has approximately ceased, and be then inverted over the pith radiometer, the bulb is immediately bedewed, and a *negative* rotation is almost immediately set up, though sometimes, just at the very first moment, there is a trace of positive rotation. The negative rotation is lively, but not lasting; and after fifteen seconds or so, is exchanged for a positive rotation, which is not lively, but lasts longer.

8. If the tumbler be lifted when the negative rotation has ceased, and the dewed surface be strongly blown upon, a lively but brief positive rotation is set up.

9. To produce positive rotation by blowing it is not *essential* that the bulb be wet. If it be merely warm, and the circumstances are such that the fly is at rest for the moment, or nearly so, blowing produces positive rotation, though much less strongly than when the bulb is wet.

10. If the tumbler be heated as in § 7, and inverted over the mica radiometer, the rotation is positive, as when the tumbler is dry.

11. If the tumbler or a cup be smoked inside (to facilitate radiation), heated to a little beyond the temperature of boiling water, and inverted over the pith radiometer, a positive rotation is produced; and if, when this has ceased, which takes place in a couple of minutes or so, the heated vessel be removed, a negative, though not lively, rotation is produced as the apparatus cools.

12. These results do not seem difficult to co-ordinate so far as to reduce them to their proximate cause.

As regards the small quantity, if any, of heat radiated directly across the glass of the bulb, the action of which was experimentally distinguishable by its promptitude, both radiometers behaved in the ordinary way.

13. As regards the mica radiometer, when the bulb gets heated and radiates towards the fly the fly is impelled in the negative direction *as if* the white pearly mica were black and the lampblack were white. And there is nothing opposed to what we know in supposing that such is *really* their relative order of darkness as regards the heat of low refrangibility absorbed and radiated by the glass; for the researches of Melloni and others have shown that lampblack is, if not absolutely white, at any rate very far from black as regards heat of low refrangibility. On the other hand, glass and mica are both silicates, not so very dissimilar in chemical composition, and it would not therefore be very wonderful, but rather the reverse, if there were a general similarity in their mode of absorption of radiant heat, so that the heat most freely radiated by glass and accordingly abounding in the radiation from *thin* glass such as that of the bulb, were greedily absorbed by mica. The explanation of the reversal of the action when heat and cold were interchanged is too well known to require mention.

14. With the pith radiometer, when the bulb as a whole is heated, and radiates towards the fly, the impulse is positive, though less strong than in the case of the mica (§ 4); and when the bulb as a whole is cooler than the fly the impulse is negative (§ 11).

But to explain all the phenomena we must dissect the total radiation from or towards the bulb. When I first noticed the negative rotation produced by a heated wet tumbler, I was disposed to attribute it to radiation from the water, which possibly the glass of the bulb might be thin enough to let pass; but when I found that hot water in a glass vessel outside, even though the glass of it were thin, produced no sensible effect, and that blowing on the heated bulb when it was dry produced a similar effect to blowing on it when dewed, though of much less amount, I perceived that the moisture acted, not by direct radiation from it, and in consequence of a difference of quality between the radiations from glass and water, but by causing a rapid *superficial* heating of the bulb; and, similarly, the blowing on the dewed surface acted by causing a rapid superficial cooling. When the dry tumbler radiates to the bulb, the radiation is absorbed at various depths; the absorption is most copious, it is true, at the outer strata, but still the change of temperature is not by any means so much confined to the immediate surface as when we have to deal with the latent heat of vapour condensed on it, or obtained from it by rapid evaporation.

Hence, thin as is the glass of the bulb (about 0·02 in. thick), we must still, in imagination, divide it into an outer and inner stratum, and examine the effects of these separately. The heat radiated by either stratum depends only on its temperature, but the radiation from the outer, on its way to the fly, is sifted by passing through the inner, and the portion for which glass is most excessively opaque is in great part stopped. It appears from the observed results that the residue acts decidedly negatively, while when the bulb is pretty uniformly heated there is positive action. We may infer that if it were possible to heat the inner stratum alone it would manifest a very decided positive action.

15. In the struggle between the opposing actions of the outer and inner strata we see the explanation of the strange behaviour of the pith radiometer. In the experiment of § 7 the outer stratum at first shows its negative action, but quickly the inner also gets heated, partly by conduction from the outer, partly by direct radiation from the tumbler, and then the inner prevails. In the experiment of § 5 the whole bulb cools, partly by radiation, partly by convection, while the fly remains warmer; and the slightly greater coolness of the outer than of the inner stratum makes up for the superiority of the inner when the two are equally cool, so that the antagonistic actions nearly balance, and slight causes, such as greater or less agitation of the air, suffice to make the balance incline one way or other. That the inner stratum *would* prevail if the two were about equally cooled may be inferred from the behaviour of the radiometer when the bulb is pretty uniformly heated (§§ 4, 11), or shown more directly by cooling the bulb with snow, when a negative rotation may be obtained.

16. The complete definition of a radiation would involve the expression of the intensity of each component of it as a function of some quantity serving to define the quality of the component, such as its refractive index in a standard medium, or its wave-length, or the squared reciprocal of the wave-length.[1] The experimental determination of the character, as thus defined, of a radiation consisting of invisible heat-rays is beset with difficulties, at least in the case of heat of extremely low refrangibility; and in general we can do little more than speak in a rough way of the radiation as being of such or such a kind. It is obvious that the behaviour of radiometers by itself alone affords no indication of the refrangibilities of the kinds of heat with which we have to deal; nevertheless, by combining what we know of the behaviour of bodies in respect to radiations in general (especially luminous radiations, which are the most easily studied) with what we observe as to the motions of radiometers, we may arrive at some probable conclusions.

17. We may evidently *conceive* a series of ethereal vibrations of any periodic time, however great, to be incident on a homogeneous medium such as glass, and inquire in what manner the rate of absorption would change with the period; though whether we can actually *produce* ethereal vibrations of a very long period is another question, seeing that we can only act on the ether by the intervention of matter, and are limited to such periods of vibration as matter can assume when vibrating molecularly, in a manner communicable to the ether, and not as a continuous mass, in the manner of the vibrations which produce sound. We may inquire whether, on continually increasing the period of vibration, the glass (or other medium) would ultimately become and remain very opaque, or whether, after passing through a range of opacity, it would become transparent again, on still further increasing the period of the incident vibrations.

18. This is a question the experimental answer to which, as it seems to me, could only be given, in so far as it could be given at all, as a result of a long series of experiments, of a kind that Melloni has barely touched on. A variety of considerations, which I could not explain in short compass, lead me to regard the second alternative as the more probable, namely, that, on increasing the periodic time, homogeneous substances in general (perhaps even metals, though this is doubtful) become at last transparent, or at least comparatively so. The limit of opacity, in all probability, varies from one substance to another; and the lower it is, the lower would be the lowest refrangibility of the radiation which the same substance is capable of emitting.

19. In what immediately follows I shall suppose accordingly that glass is strongly absorbing through a certain range of low

[1] A map of the spectrum, constructed with the squared reciprocals of the wave-lengths for abscissæ, would be referred to a natural standard, no less than that of Angström, which is constructed according to wave-lengths; while it would have the great advantage of admitting of ready comparison with refraction spectra, the kind almost always used.

refrangibility, on *both* sides of which it gradually becomes transparent again.[1] Imagine a spectrum containing radiations of all refrangibilities with which we have to deal; let portions of this spectrum on the two sides of the region of powerful absorption for glass be called *wings* of that region, and let left to right be the order of increasing refrangibility. Then the spectrum of the radiation from a thin plate of glass, if it could be observed, would be seen to occupy the region of chief absorbing (and therefore emitting) power and its wings. The spectrum of the radiation from the outer stratum of the bulb of the pith radiometer, after transmission through the inner, would consist of two wings, with a blank, or nearly blank, space between; it would resemble, in fact, a widened bright spectral line, with a dark band of reversal in its middle, save that, instead of being confined to extremely narrow limits of refrangibility, the central space and its wings would be of wide extent. It follows from the experiments that, in the complete radiation from glass, the portions of the spectrum called the wings together act negatively, the portion between positively. It does not, of course, follow that each wing acts negatively, but only that the balance of the two is negative. When the tumbler is heated a little over 212° there is a slight positive action from radiation which passes directly through the bulb. The circumstances lead us to regard this as an extension of the right wing; for it comes from a depth, measured from the inner surface of the bulb in glass, *i.e.*, not counting the intervening air, somewhat greater than the thickness of the wall of the bulb; and we know that the more a solid body is heated, the higher, as a rule, does the refrangibility of the radiation which it emits extend, and the greater the proportion of rays of high to those of low refrangibility. It is simplest, therefore, to suppose that the action of the right wing, like that of the space between the wings, is positive, and that the observed negative action in the experiment of § 7 is due to the excess of negative action of the left wing over positive action of the right. In the mica radiometer the experiments indicate no such difference of action in the different layers of the bulb as in the case of the pith radiometer. Hence taking, in accordance with what now appears to be made out to be the theory of the motion of the radiometer, the direction in which the fly is impelled as an indication which is the warmer of the two faces of the discs, and that again as an indication which is the darker with respect to the radiation to which it is exposed, we arrive at the following results as regards the order of darkness of the substances for the three regions into which the spectrum of the incident radiation has been supposed to be divided, the name of the lighter substance being in each case placed above that of the darker :—

	Left wing.	Region of intense absorption by glass.	Right wing.
From pith radiometer ...	Lampblack.	Pith.	Pith.
	Pith.	Lampblack.	Lampblack.
From mica radiometer ...	Lampblack.	Lampblack.	Mica.
	Mica.	Mica.	Lampblack.

Hence, on descending in refrangibility, the order of darkness of the two substances o either pair is at first the same as for the visible spectrum, and at last the opposite; and the reversal of the order takes place sooner with mica and lampblack than with pith and lampblack. The order falls in very well with that of the chemical complexity of the three substances.

20. The whole subject of the behaviour of bodies with respect to radiant heat of the lowest degrees of refrangibility seems to me to need a thorough experimental investigation. The investigation, however, is one involving considerable difficulty. We can do little towards classifying the rays with which we are working unless we can form a pure spectrum. A refraction spectrum is the most convenient; but the only substance known which would be approximately suitable for forming the prism, lens, &c., required for such a spectrum, and for confining liquids, is rock-salt, of which it is extremely difficult to procure perfectly limpid specimens of any size; and even rock-salt itself, as Prof. Balfour Stewart has shown, is defective in transparency for certain kinds of radiant heat. Then, again, the only suitable measuring-instrument for such researches, the thermopile, demands a thorough examination with reference to the coating to be employed for absorbing the incident radiation. Hitherto lampblack has been used almost exclusively for the purpose; and it is commonly assumed, in accordance with certain of Melloni's results, that lamplack absorbs equally heat-rays of all kinds. But the experiments by which Melloni established the partial diathermancy of lampblack prove that rays exist for the absorption of which that substance is unsuitable.

[1] It may be noticed that this supposition, which, as appearing the more probable, is adopted for clearness of conception, is not essentially involved in the explanation that follows; which would hardly be changed if the "left wing" were not terminated on the left.

On calling on Mr. Crookes after the above was written, I was surprised to find that all his mica radiometers behaved towards a heated glass shade in the opposite way to that he had given me, going round positively instead of negatively. Mr. Crookes showed me and gave me a specimen of the kind of mica he employs as eminently convenient for manipulation. It is found naturally in a condition resembling artificially roasted mica. It is not, however, quite so opaque for transmitted light, nor of quite such a pearly whiteness for reflected light as that which has been artificially roasted at a high temperature. The mica radiometer that Mr. Crookes first gave me, which I will call M_1, was, Mr. Gimingham told me, the only one they had made with roasted mica.

Mr. Crookes was so kind as to give me, for comparative experiment, a mica radiometer, which I will call M_2, made from the natural foliated mica. It revolves a good deal more quickly than M_1 under the influence of light; it also gets more quickly under way, indicating that the mica is thinner. When covered with a hot glass it revolves positively, as already remarked; there is, however, but little negative rotation when the glass is removed.

The difference in the thickness and condition of the mica sufficiently explains the difference of behaviour of M_1 and M_2. Any radiant heat incident on the white face that reaches the middle of the mica, whether it afterwards is absorbed by the mica or reaches and is absorbed by the lampblack, tends to heat the second or blackened face more than the first, and therefore conspires with the heat incident on the lampblack, and absorbed by it, to produce positive rotation; and the smaller thickness and less fine foliation of the natural mica are favourable to the transmission of radiant heat to such a depth.

P.S.—It might be supposed at first sight that the change of rotation from negative to positive (in § 7) was due, not to a change in the conditions of absorption, but to the circumstance that the inner surface of the bulb had become warm by conduction, so as to be warmer than the surfaces of the fly instead of colder. For we now know that the "repulsion resulting from radiation," as in some way or other it undoubtedly does result, is an indirect effect, in which radiation acts only through the alterations it occasions in the superficial temperatures of the solids in contact with the rarefied gas; and it might be supposed that when the inner surface of the bulb passed from colder than the fly to warmer, the direction of rotation would, on that account alone, be reversed. This, however, is not so. If bulb and fly are at a common temperature, and the instrument is protected from radiation, the fly remains at rest whether the common temperature be high or low. If a small portion of the total surface in contact with the rarefied gas be warmed by any means, repulsion takes place, through the intervention of the rarefied gas, between the warmed surface and the opposed surfaces, if not too distant; if it be cooled, the result is attraction. It does not matter whether the surface at the exceptional temperature belong to the fly or the bulb. The former takes place in the ordinary case of a radiometer exposed to radiation, the latter in that of a radiometer at a uniform temperature and protected from radiation when a small portion of the bulb is warmed or cooled, in which case the part at the exceptional temperature repels or attracts the disc irrespectively of its colour or the nature of its coating.[1] Suppose now that the fly is being warmed by radiation from without, the bulb being cool, at least at its inner surface. Let A, B be the two kinds of faces of the discs, and suppose A to be the better absorber of the total radiation. Then A will be the warmer, and therefore will be more strongly repelled than B. Suppose now that the bulb is heated till its inner surface becomes warmer than the fly. Then the fly will still be receiving heat by radiation, to some extent also by communication from the gas; but this will be the same for both faces. Hence if A be still the better absorber of the two (A, B), A will be the warmer, and being less below the tem-

[1] Theoretically there would be a minute difference of temperature, produced, other circumstances being alike, by the difference in the absorbing or emitting power of the two faces of a disc, as regards the radiation which is the difference between the radiations from or towards the affected portion of the bulb and the same portion at the normal temperature. But this, and the repulsion or attraction corresponding to it, would be only a small quantity of the second order, the main effect being deemed one of the first order.

perature of the interior surface of the fly will be less attracted, or, which is the same, more repelled. Hence, whether the inner surface of the bulb be cooler or hotter than the fly, a reversal in the direction of rotation while the fly is being heated, indicates a reversal in the order of absorbing power of the two faces, and that, again, shows that the order is different for different components of the total radiation, and that the ratio of the intensity of those components has been changed.

It is perhaps hardly necessary to observe that the radiometers mentioned in this paper are of the usual form—that is to say, that their arms are symmetrical, so far as *figure* is concerned, with respect to a vertical plane passing through the point of support. Accordingly the rotation which is attained, for instance, with a radiometer with concave disks of aluminium, alike as to material on both faces (of which kind, again, I owe a beautiful specimen to Mr. Crookes's kindness), has not been referred to. This rotation, depending on the more favourable presentation to the bulb of the outer (and therefore nearer and more efficient) portions of the fly on the convex than on the concave side, has nothing to do with the one isolated subject to which the present paper relates, namely, the elucidation of the peculiar behaviour in certain cases of certain kinds of radiometers, by a consideration of the heterogeneous character of the total heat-radiation.

(*To be continued.*)

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

LEEDS.—By the liberality of the Worshipful the Drapers' Company, the Council of the Yorkshire College are prepared to appoint an instructor in coal mining at the stipend of 100*l.* per annum and half the students' fees. A portion only of the instructor's time will be required. The fuller conditions and duties of the office may be learned from the secretary. Applications and testimonials must be received on or before January 18.

LEXINGTON, U.S.—In connection with the Centennial, efforts have been made in the United States to raise an endowment fund for Washington and Lee University, at Lexington, Va. The institution dates from colonial times, and was endowed, while it was still only an academy, by Washington and other soldiers of the Revolution. Among other recent benefactors of the University is Mr. L. J. McCormick, of Chicago, who has offered to give his magnificent telescope, made by Alvan Clark, of Cambridge, U.S., at a cost of 50,000 dols., provided the institution would raise the necessary funds to equip and maintain it. The trustees have not yet been able to do anything towards the acceptance of this proposal. It would be a great misfortune if the conditions could not be complied with, and we hope that the suggestion that the ladies in various parts of the States should take the matter up will be complied with; there is no doubt if they make up their minds to success they will succeed.

BERLIN.—The great Prussian university is closely competing now with the Leipsic University in point of attendance. In the calendar which has just appeared we notice that the number of matriculated students during the present winter amounts to 2,839, an increase of 600 on the summer semester. They are divided among the faculties as follows: theological 168, legal 1,163, medical 345, philosophical 1,163. There are 210 foreigners in the list, including 42 from America. Besides these matriculated students, there are 2,200 other persons in attendance on the lectures, belonging to the various technical and art schools of the city. The corps of instructors numbers 210, nearly half of whom are in the philosophical faculty.

BONN.—The winter attendance at the University is 859, an increase of sixty-two on the preceding semester. The philosophical faculty includes 375, the legal 219, the medical 126, the Catholic theological, 89, and the Evangelical, 50.

SOCIETIES AND ACADEMIES

LONDON

Linnean Society, December 6.—Prof. Allman, president, in the chair.—Messrs. J. N. Fitch, J. S. Gamble, F. S. Piggott, A. B. Stewart, and Prof. Macoun were elected Fellows.—Mr. Thiselton Dyer exhibited portions of the "Nam-mu" tree, which grows in Yunnan, 25°–26° N. lat. The Chinese nobility greatly prize its wood for building purposes and for making coffins, and enormous columns in tombs of the Ming dynasty, 300 years old, are still extant. Supposed to be teak, it probably rather belongs to the Lauraceæ, the leaves closely resembling those of *Phœbe pallida.* Mr. Dyer also exhibited a seed of *Entada scandens*, and another of an anonaceous plant (*Cyathocalyx Maingayi ?*) found in the cæcum of *Rhinoceros sumatrensis* from Chittagong, and dissected at the Zoological Gardens, Regent's Park; and he likewise showed fruits of *Oncocarpus vitiensis* from the crop of a fruit-pigeon (*Carpophaga latrans*).—Attention was afterwards called by Mr. Dyer to the fruit-head of an Indian *Pandanus* made into a brush, the fibrous tissue of the drupes forming the bristles, and this instrument was said to be used to scrape cloth, like our teazle (*Dipsacus*).—Flowers and foliage of Cinchona (*C. calisaya*, vars., *Josephiana* and *Anglica*) grown in the garden of Mr. J. Elliot, at Tottenham, were exhibited by that gentleman, whose researches among the quinine-bearing trees are already well known and appreciated.—Mr. Moggridge read a note on the occurrence at Wallis Down, a heath near Bournemouth, of *Dabeocia polifolia.*—A paper on certain organs of the Cidaridæ was communicated by Mr. Chas. Stewart, who illustrated, amongst others, the subjoined points of his recent investigations. Among the sea-urchins the families Diadematidæ, Echinometridæ, and Echinidæ, have long been known to possess external branchiæ; but the existence of such in the Cidaridæ has been denied by Müller, though insisted on by Alex. Agassiz. Mr. Stewart finds in *Dorocidaris papillata* five organs corresponding to branchiæ, but situated internally. The water bathing these interior gills finds ingress and egress by a crevice near the "compasses," the peculiar mechanism of the teeth and jaws producing the temporary opening in question. As respects the pedicellariæ of Cidaridæ, where the jaw ends in a terminal hollow fang, there is an additional orifice to that at the tip, besides two glands in the vicinity; he suggests this to be a poison apparatus, comparable to the falces of the spider, and poison sac and tooth of venomous serpents.—The Secretary read a paper by Dr. I. Bayley Balfour, "Observations on the genus *Pandanus*." Few families of plants present more difficulty in their elucidation than the Pandanaceæ; this by variability of species, difficulty of procuring the male flower, with little character in the leaves, while the fruit loses its distinctive features in drying. The Screw-pines had attracted the notice of the early voyagers, but their descriptions are confused. To Rumphius we owe the name Pandanus, though his account and figures are poor compared with Reede's of a century before. Linnæus, though indicating a plant under the name *Bromelia sylvestris*, omitted the genus *Pandanus*, a want supplied by his son. Afterwards, as species increased, many new genera were unnecessarily introduced, which Dr. Balfour is now inclined to reject; even Brongniart's New Caledonia genera do not claim acceptance. *Pandanus* runs over a great breadth of longitude, viz., from east tropical Africa through the Mascarene Islands, India, Indian Archipelago, and Australia, to the Sandwich Islands. The East Archipelago and the Mascarenes are centres whose species do not commingle. There succeeds in this paper other facts and an extensive list of names and references to all the Pandani known.—The substance was given of a report on a small collection of insects obtained by Dr. J. C. Ploëm, in Java, with description of a new species of *Hoplia*, by Chas. O. Waterhouse, of the British Museum.—The Secretary read a communication by Dr. J. Stirton, viz., "Notes on the Rev. Mr. Crombie's paper on the Lichens of the *Challenger* Expedition," and another note by Dr. R. C. A. Prior, relative to the migration of wild geese, purported to have passed from North America to the African coast.

Physical Society, December 15.—Prof. G. C. Foster, president, in the chair.—The following candidates were elected Members of the Society:—W. E. Ayrton, J. M. Cameron, J. W. Clark, J. E. Judson, B.A., H. N. Moseley, M.A., F.R.S., Lord Rayleigh, M.A., F.R.S., W. N. Stocker, M.A., and H. T. Wood.—Mr. C. W. Cooke read for the author, Prof. S. P. Thompson, a paper on permanent Plateau films, and exhibited the process of their formation. After a brief enumeration of the various attempts made by Plateau himself, Schwartz, Mach, Rottier, and others, most of which are described in the work of Plateau, the author described his own experiment on the subject. As the result of these he concludes that the best films are obtained by using a mixture of 46 per cent. of pure amber-coloured resin, and 54 of Canada balsam, which should be heated to from 93° to 95° C. The frames for forming the films are made of brass wire 0·3 mm. in diameter, and when thicker wire is employed they are found to be irregular in consequence of the retention of heat by the metal. The films are obtained by simply introducing these frames into the heated

mixture, and they harden almost immediately on exposure to the air; but better results are obtained by slow drying in an air bath heated up to 80° C., and allowed to cool. In proof of the toughness of the films it was mentioned that a flat circular film 4 cm. in diameter, had supported a 50-gramme brass weight at its centre.—Mr. Sedley Taylor then exhibited some experiments in illustration of a paper on the colours exhibited by vibrating liquid films which he has recently communicated to the Royal Society.—Dr. Guthrie exhibited a simple lecture illustration of the action of the telephone. Two similar coils of wire are placed one on the end of a bar magnet, and the other on a soft iron core. A tin disc about three inches in diameter is suspended by two threads almost in contact with one end of this latter, and when a similar disc is brought, at regular intervals, against the end of the magnet which is provided with the coil, a distinct movement of the first-named disc is observed which can be easily increased by properly timing the movement of the inducing disc.

Anthropological Institute, November 27.—Mr. John Evans, D.C.L., F.R.S., president, in the chair.—The election of five new members was announced.—Major-Gen. A. Lane Fox, F.R.S., exhibited various objects from Istria and Scinde.—The Director read some notes on Socotra, by Capt. Hunter, R.N., in which some of Lieut. Wellstead's statements about that island were criticised.—A paper on the Záparos, by Mr. A. Simpson, was then read, in which many interesting observations of these tribes of "Equador" were recorded. Their wonderful tracking powers, abstention from heavy meats, such as tapir and peccari, curious mode of training hunting-dogs, were described. Their enjoyment in the destruction of life, human or animal, with the exception of the alligator, which they will not touch themselves, was very marked. The Napos do not resemble them in this respect. The Záparos are very disunited, and wander about in separate hordes, the worst of which are the Supinus. Courtship is sometimes carried on by a silent invitation by the suitor to his elect to cook his food. If rejected, he tries elsewhere. The Záparos are described as of a happy and cheerful disposition, very superstitious, believing in an evil spirit, and very poor and almost nude.—A paper on the Malayo Polynesians, by Rev. S. J. Whitmee, was then read, in which the author noted the high social position of women in the Samoan group, as compared with their place among the *black* Polynesians. The existence of hereditary ranks and titles among the *brown* Polynesians seems to the author to indicate a former higher condition. The author referred to the difficulties experienced by missionaries in obtaining the true versions of the native poems and myths, and noted the custom of preserving the myths in poetry as well as prose, the two versions acting one as a check on the other, and so preserving the correctness one of the other. In the discussion, Major-Gen. A. Lane Fox, Mr. Blackmore, Mr. Hyde Clarke, and others, took part.

Entomological Society, December 5.—J. W. Dunning, vice-president, in the chair.—Mr. W. L. Distant exhibited specimens of the rare species of Hemiptera-Heteroptera, *Tetroxia beauvoisii*, and *Oncocephalus subspinosus*, from the West Coast of Africa.—Mr. F. Smith exhibited a fine series of *Macropis labiata*, male and female, captured near Norwich by Mr. I. B. Bridgman.—Mr. Smith also exhibited a specimen of *Rophites quinquespinosus*, a genus and species new to the British Hymenoptera, captured near Hastings by the Rev. E. H. Bloomfield.—Mr. Meldola exhibited three fine photographic enlargements of micro-photographs (two being of parts of insects) taken by Mr. Edward Viles, of Pendryl Hall, Wolverhampton. The photographs, which had been exhibited at the recent exhibition of the Photographic Society were 30 x 24 in., while the original negatives were 3 in. square.—Mr. Meldola likewise performed an acoustical experiment illustrating the action of the stridulating apparatus in the *Phasma* (*Pterinoxylus*), an account of which had been given to the society by Mr. Wood Mason at the last meeting.—Mr. Wood Mason made further remarks on the structure of the stridulating organ in scorpions.—Mr. F. Smith mentioned a case of stridulation occurring in a British species of *Curculionidæ* (*Acalles*).—Mr. Dunning called attention to a striking case of mimicry recorded by Mr. Neville Goodman in the *Proceedings* of the Cambridge Philosophical Society for February, 1877, the mimic being a species of *Laphria*, and the model, the well-known Egyptian hornet, *Vespa orientalis*.—Mr. F. Smith read a paper containing descriptions of new species of hymenopterous insects from New Zealand, collected by Prof. Hutton at Otago.—Mr. A. G. Butler communicated a paper on the Lepidoptera of the Amazons, collected by Dr. James W. H. Trail during the years 1873 to 1875.—Dr. Sharp communicated the following papers:—Descriptions of eight new species and a new genus of Cossonides, from New Zealand, and descriptions of some new species and a new genus of Rhyncophorous Coleoptera, from the Hawaiian Islands.

MANCHESTER

Literary and Philosophical Society, October 16.—Mr. E. W. Binney, F.R.S., president, in the chair.—The President exhibited to the meeting some coal-measure plants and other organic remains from Spain. From the character of the fossil organic remains and the nature of the strata he was led to believe that the coal-field of Puertollano was of the same geological age as our true English coal-measures.—Mr. M. P. Pattison Muir, F.R.S.E., exhibited and gave a description of a modified form of Hofmann's apparatus for determining vapour densities.—Note on an edible clay from New Zealand, by M. M. Pattison Muir, F.R.S.E. The author lately received from Mr. R. E. Day, a small specimen of a clay which is greedily eaten by the sheep in a certain district in New Zealand. The clay was brought by Mr. Day from Simon's Pass Station, Mackenzie Country, South Island. It there forms a range of low bare hills: the sheep (merino sheep) eat very considerable quantities of the clay without appearing to be any the worse for it. It is supposed by the shepherds that the clay must contain salt, and that it is to supply the deficiency of this article of food that the sheep resort to the earth. The analysis shows that very probably the shepherds are right:—Silica, 61·25; alumina, 17·97; ferric oxide, 5·72; calcium oxide, 1·91; magnesium oxide, 0·87; alkalies (as chlorides), 3·69; organic matter, 1·77; water, 7·31 = 100·49.—On the decomposition of calcium sulphate by alkaline chlorides; a contribution to agricultural chemistry, by M. M. Pattison Muir, F.R.S.E.—On some thionates, by H. Baker, student in the Owens College. Communicated by Prof. C. Schorlemmer, F.R.S.

VIENNA

Imperial Academy of Sciences, October 18.—On the chemical nature of peptone and its relation to albumen, by M. Herth.—On the addition of prussic acid to urea, and on the action of trichloric lactic acid on urea, by M. Cech.—Transformation of cyanamide into ammelide, by MM. Cech and Dehmel.—New discoveries on the negative heliotropism of above-ground parts of plants, by M. Wiesner.—On Fraunhofer's rings, Quetelet's stripes, and allied phenomena, by M. Exner.

October 25.—On the connection of *n* straight lines in the plane, and on properties of the triangle and two propositions of Steiner therewith connected, by M. Kantor.—On the structure and the growth of some forms of mould-fungus, by M. Hasloch.—On the development of the small pollen-plants of *Colchicum autumnale* L., by M. Tomaschek.—On the secular acceleration of the mean motion of the moon, by M. v. Littrow.

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THURSDAY, JANUARY 3, 1878

THE LAST OF THE GASES

THE year 1877 will ever be memorable in the history of scientific progress, its close having been marked by a brilliant series of researches which have ended in an absolute demonstration of the fact that molecular cohesion is a property of all bodies without any exception whatever.

This magnificent work divides itself into two stages, which we shall refer to separately: first the liquefaction of oxygen, and then, following close upon this, the liquefaction of hydrogen, nitrogen, and atmospheric air.

In the liquefaction of oxygen, which we announced last week as having been accomplished by M. Pictet of Geneva, we have not only an instance of the long time we may have to wait, and of the great difficulties which have to be overcome, before a theoretical conclusion is changed into a concrete fact—something definite acquired to science; but also another instance of a double discovery, showing that along all the great lines of thought opened up by modern investigation and modern methods, students of science are marching at least two abreast.

It appears that as early as December 2 M. Cailletet had succeeded in liquefying oxygen and carbonic oxide at a pressure of 300 atmospheres and at a temperature of $-29°$ C. This result was not communicated to the Academy at once, but was consigned to a sealed packet on account of M. Cailletet being then a candidate for a seat in the Section of Mineralogy. Hence, then, the question of priority has been raised, but it is certain that in the future the work will be credited to both, on the ground that the researches of each were absolutely independent, both pursuing the same object, creating methods and instruments of great complexity. We regret, therefore, that M. Jamin, at the sitting of the Academy to which we have referred, seemed to strain the claims of M. Cailletet by stating that to obtain the gas non-transparent was the same as to obtain it liquefied. We are beginning to know enough of the various states of vapour now not to hazard such an assertion as this. This remark, however, rather anticipates matters, and indeed, as we shall show afterwards, M. Cailletet need not himself be very careful of the question of priority—even if it were ever worth caring for except to keep other people honest.

Owing to the double discovery and the curious incident to which we have referred, the meeting of the Academy on the 24th ult. was a very lively one, as not only was the sealed packet and a subsequent communication from M. Cailletet read, but M. Pictet had sent a long letter to M. Dumas giving full details of his arrangements. MM. Dumas, H. St. Claire Deville, Jamin, Regnault and Berthelot all took part in the discussion, the former admirably putting the work in its proper place by the following quotation from Lavoisier :—

"... Considérons un moment ce qui arriverait aux différentes substances qui composent le globe, si la température en était brusquement changée. Supposons, par exemple, que la terre se trouvât transportée tout à coup dans une région beaucoup plus chaude du système solaire, dans une région, par exemple, où la chaleur habituelle serait fort supérieure à celle de l'eau bouillante ; bientôt l'eau, tous les liquides susceptibles de se vaporiser à des degrés voisins de l'eau bouillante, et plusieurs substances metalliques même, entreraient en expansion et se transformeraient en fluides aériformes, qui deviendraient parties de l'atmosphère.

"Par un effet contraire, si la terre se trouvait tout à coup placée dans des régions très froides, par exemple de Jupiter et de Saturne, l'eau qui forme aujourd'hui nos fleuves et nos mers, et probablement le plus grand nombre de liquides que nous connaissons, se transformeraient en montagnes solides.

"L'air dans cette supposition, ou du moins une partie des substances aériformes qui le composent, cesserait, sans doute, d'exister dans l'état de fluide invisible, faute d'un degré de chaleur suffisant; il reviendrait donc à l'état de liquidité, et ce changement produirait de nouveaux liquides dont nous n'avons aucune idée."

When Faraday in the year 1823 (at the age of 31) began the researches indicated in the last paragraph quoted by M. Dumas, and first liquefied chlorine and then several other gases, he had no idea that he had been anticipated, as he had been, by Monge and Clouet, who condensed sulphurous acid before the year 1800, and by Northmore, who liquefied chlorine in 1805. If the great experimenter were among us now how delighted he would be to see one of the greatest ironmasters of France employing the enormous resources at his disposal at Châtillon-sur-Seine, and a descendant of the Pictet, the firm friend of his great friend De la Rive (who was the first to whom he communicated his liquefaction of chlorine), thus engaged in carrying on the work which he made his own.

The methods employed by MM. Pictet and Cailletet are quite distinct and are the result of many years' preparatory study, as testified by M. H. St. Claire Deville and M. Regnault. It is difficult to know which to admire most, the scientific perfection of Pictet's method or the wonderful simplicity of Cailletet's. It is quite certain that the one employed by the latter will find frequent use in future experiments. We may briefly refer to both these methods.

M. Cailletet's apparatus has already been briefly alluded to in these columns. It consists essentially of a massive steel cylinder with two openings; through one hydraulic pressure is communicated. A small tube passes through the other, the sides of which are strong enough to withstand a pressure of several hundred atmospheres, and which can be inclosed in a freezing mixture. It opens within the cylinder into a second smaller cylinder serving as a reservoir for the gas to be compressed. The remainder of the space in the large cylinder is occupied by mercury. M. Cailletet's process consists in compressing a gas into the small tube, and then by suddenly placing it in communication with the outer air, producing such a degree of cold by the sudden distention of the confined gas that a large portion of it is condensed, a process perfectly analagous to that used to prepare solid carbonic acid by the rapid evaporation of the liquefied gas.

In M. Cailletet's experiment with oxygen it was brought to a temperature of $-29°$ C. by the employment of sulphurous acid and a pressure of 300 atmospheres; the gas was still a gas. But when allowed to expand suddenly, which, according to Poisson's formula, brings it down to 200 degrees below its starting-point, a cloud was at once formed. The same result has since been obtained without

the employment of sulphurous acid, by giving the gas time to cool after compression. M. Cailletet has not yet obtained, at all events, so far as we yet know, oxygen in a liquid form, as M. Pictet has done; on being separated from its enormous pressure it has merely put on the appearance of a cloud.

M. Pictet's arrangements are more elaborate. He uses four vacuum- and force-pumps, similar to those which were recently exhibited in the Loan Collection of Scientific Apparatus for making ice, driven by an engine of 15-horse power. Two of these are employed in procuring a reduction of temperature in a tube about four feet long containing sulphurous acid. This is done in the following way: the vacuum pump withdraws the vapour from above the surface of the liquid sulphurous acid in the tube, which, like all the others subsequently to be mentioned, is slightly inclined so as to give the maximum of evaporating surface. The force-pump then compresses this vapour, and sends it into a separate reservoir, where it is again cooled and liquefied; the freshly-formed liquid is allowed to return under control to the tube first referred to, so that a complete circulation is maintained. With the pumps at full work there is a nearly perfect vacuum over the liquid and the temperature falls to $-65°$ or $-70°$ C.

M. Pictet uses this sulphurous acid as a cold-water jacket, as we shall see. It is used to cool the carbonic acid after compression, as water is used to cool the sulphurous acid after compression.

This is managed as follows:—In the tube thus filled with liquid sulphurous acid at a temperature of $-60°$ C. there is another central one of the same length but naturally of smaller diameter. This central tube M. Pictet fills with liquid carbonic acid at a pressure of four or six atmospheres. This is then let into another tube four metres long and four centimetres in diameter. When thus filled the liquid is next reduced to the solid form and a temperature of $-140°$ C., the extraction of heat being effected as before by the pump, which extracts three litres of gas per stroke and makes 100 strokes a minute.

Now it is the turn of the oxygen.

Just as the tube containing carbonic acid was placed in the tube containing sulphurous acid, so is a tube containing oxygen inserted in the long tube containing the now solidified carbonic acid. This tube is five metres long, fourteen millimetres in exterior diameter, and only four in interior diameter—the glass is very thick. The whole surface of this tube, except the ends which project beyond the ends of the carbonic acid tube, is surrounded by the frozen carbonic acid.

One end of this tube is connected with a strong shell containing chlorate of potash, the other end is furnished with a stop-cock.

When the tube was as cold as its surroundings, heat was applied to the chlorate, and a pressure of 500 atmospheres was registered; this descended to 320. The stop-cock was then opened, and a liquid shot out with such violence that none could be secured, though we shall hear of this soon.

Pieces of lighted wood held in this stream spontaneously inflamed with tremendous violence.

In this way, then, has oxygen been liquefied at last. But this result has no sooner filled us with surprise than it has been completely eclipsed. On the last day of December, a week after the meeting of the Academy to which we have referred, M. Cailletet performed a series of experiments in the laboratory of the École Normale at Paris, in the presence of Berthelot, Boussingault, St. Claire Deville, Mascart, and other leading French chemists and physicists, using the same method as that formerly employed for oxygen and he then and there liquefied hydrogen, nitrogen, and air!

M. Cailletet first introduced pure nitrogen gas into the apparatus. Under a pressure of 200 atmospheres the tube was opened, and a number of drops of liquid nitrogen were formed. Hydrogen was next experimented with, and this, the lightest and most difficult of all gases, was reduced to the form of a mist at 280 atmospheres. The degree of cold attained by the sudden release of these compressed gases is scarcely conceivable. The physicists present at the experiment estimated it at $-300°$ C.

Although oxygen and nitrogen had both been liquefied, it was deemed of interest to carry out the process with air, and the apparatus was filled with the latter, carefully dried and freed from carbonic acid. The experiment yielded the same result. On opening the tube a stream of *liquid air* issued from it resembling the fine jets forced from our modern perfume bottles.

These more recent results are all the more surprising as, at an earlier stage, hydrogen, at a pressure of 300 atmospheres, has shown no signs of giving way.

These brilliant and important results, though, as we have said, they give us no new idea on the constitution of matter, open out a magnificent vista for future experiment. First, we shall doubtless be able to study solid oxygen, hydrogen, and air, and if MM. Pictet and Cailletet succeed in this there will then be the history to write of the changes of molecular state, probably accompanied by changes of colour, through which these elemental substances pass in their new transformations.

There is a distinct lesson to be learnt from the sources whence these startling *tours de force* have originated. The means at the command of both MM. Cailletet and Pictet arise from the industrial requirements of these gentlemen, one for making iron, the other for making ice.

Why then in England, the land of practical science, have we not more men like MM. Cailletet and Pictet to utilise for purposes of research the vast means at their disposal, or at all events to allow others to use them?

It is also clear that to cope with modern requirements our laboratories must no longer contain merely an antiquated air-pump, a Leyden jar, and a few bottles, as many of them do. The professor should be in charge of a work- instead of an old curiosity-shop, and the scale of his operations must be large if he is to march with the times—times which, with the liquefaction of the most refractory gases, mark an epoch in the history of science.

HUXLEY'S PHYSIOGRAPHY

Physiography: an Introduction to the Study of Nature. By T. H. Huxley, F.R.S. (London: Macmillan and Co., 1877.)

AMONG educational works which are calculated to afford real assistance to the teacher in his all-important labours, we may recognise two distinct classes. One

of these includes the "text-books," which should aim at presenting only the accurate and well-proportioned outlines of a system of instruction, leaving it to the teacher himself so to fill in these outlines with explanation and illustration, as to cause the new facts and reasonings to produce the most vivid and abiding impressions upon the minds of his pupils. But inasmuch as the attainment of such a result demands much practical skill and educational tact—a skill and tact which are by no means easy of acquirement—the necessity and value of another class of works becomes manifest. This second class of educational works comprises such as aim at instructing the teacher how best to perform his difficult task; which exemplify the work of explanation, illustrate the art of illustration, and show how the dry bones of barren facts may, by clear arrangement and logical connection, be compacted into a body of real knowledge, and this body by being infused with the earnest intelligence of the teacher, may be quickened into active and fruitful life in the minds of the scholars.

It is to this latter very important class of educational works that we should be inclined to refer the book before us, and we cannot therefore regard the designation of it as a "manual for students," which is borne upon its cover—one for which we suspect that the author is not himself responsible—as either happy or judicious. That some instruction in the physical laws of that universe in which we are placed ought to form a recognised part of our system of elementary education has been again and again maintained and strongly insisted upon by scientific men, and by none more persistently or more urgently than by the author of the present work. When we reflect on the fact that to the man who has learnt to recognise, obey and apply these laws, Nature reveals herself as a helpful and bountiful mother, ever ready to aid him n his industry, his arts, and his commerce; while to him who ignores or violates these laws she is known only as a terribly relentless and avenging goddess, ever thwarting his most earnest endeavours, and scourging him with plagues, pestilences, and famines—it is hard to realise how slowly the necessity for this instruction in natural knowledge has forced itself upon the minds of those who are responsible for the scheme of elementary education adopted in this country. But society—the machinery of which is every day becoming more complicated and more susceptible to those painful consequences which follow from the infringement of the laws of nature—will doubtless in the end demand, as indeed it has a right to do, that every unit in her organisation should be fitted so to play his part, as to avoid the danger to himself and others which the neglect or violation of natural laws invariably entails.

Almost every demand that the principles of physical science should be taught in our elementary schools, has been met with the objection that our knowledge of nature and her laws has in recent years grown to such an extent, and ramified into so vast a number of channels as to make any attempt to teach it to the young quite hopeless. As well might we point to the number of volumes in the library of the British Museum, and declare that their existence demonstrates the uselessness of teaching the art of reading. No one, of course, would desire that an epitome all the sciences should be taught to children; but what is demanded is that the methods of modern scientific thought should be made familiar to every mind, that a few leading and necessary truths should be taught concerning the world in which we live and the laws which control its potent forces (seeing that upon our knowledge or ignorance of these depends much of our happiness and success or our misery and failure in the adventure of life), and that, last but not least, the minds of all young people should be conducted within the threshold of the temple of natural knowledge, so that any among them that may be endowed with the necessary capabilities may learn there to dedicate themselves to the pursuit of science.

How can this elementary instruction in science be best imparted to the young? This is the important question which Prof. Huxley applies himself to answer in the work before us; and he accomplishes his object much better by means of example than he could by any amount of discussion of the general principles of the art of teaching. On several other occasions the author has indicated the importance of making a knowledge of the more striking phenomena of nature, those with which we come into contact in our every-day life, and which exercise the greatest influence on our daily occupations and experiences, the starting-point of our introduction to the world of scientific thought; and it is to this vestibule of the temple of natural science that he applies the name of "Physiography."

The author of the present work of course recognises that first principle of good teaching which consists in fastening at first on facts and ideas which are known and familiar, and from thence leading the minds of the student by a succession of steps, no one of which shall present any serious difficulties, up to those more unfamiliar observations and those less obvious deductions from them, which if presented in the first instance might startle and repel rather than attract the scholar. We must ask the reader himself to trace in the work before us how, setting out from the most striking and easily observed facts about the River Thames, Prof. Huxley shows his admirable skill in teaching by leading his readers through a series of reasonings couched in simple and untechnical, but always accurate and elegant, language, up to the grandest truths in physics, biology, geology, and astronomy; how, throughout, happy analogies and telling illustrations make the path of the scholar, light, easy, and pleasant; and how in all this nothing of the exactness and dignity of science is sacrificed to a desire to say those fine or funny things which are too often supposed to convert a prosy book into a "popular" one.

The teacher who takes these easy lessons in elementary science and simply repeats them to his scholars can scarcely fail to communicate some sound and useful instruction to them. But every competent and judicious teacher will prize Prof. Huxley's book rather as a model than as a "crib"—and this is the light in which the author, we are persuaded, would desire that his work should be regarded by them. It is as easy, for example, to make the Mersey, the Severn, the Forth, or the Clyde the starting point of our studies of nature, as the Thames, and in Manchester, Bristol, Edinburgh, or Glasgow respectively, it is far better to do so; nor will any well-instructed teacher find the smallest difficulty in thus adapting his lessons to his

auditory. To such teachers as have never studied or thought on scientific questions themselves, our advice would be to content themselves with placing Prof. Huxley's book in their school-libraries, and not to run the risk of spoiling its teachings by filtering them through their own minds.

We have spoken at such length on the value of this work to the teacher, as to leave but little space for reference to its interest to the general reader, yet this is by no means small; to those who seek an "introduction to the study of nature," which shall be at the same time both sound and readable, exact and untechnical, we most heartily commend the work before us.

We are informed in the preface that the idea of this work has long been entertained, and its plan and methods frequently revolved in the mind of the author. It is probable that not a little of its present excellence is due to this slow maturation of its plan, assisted, as we learn that its development has been, by its embodiment in two successive courses of lectures—on the shorthand notes of one of which the present book is based. In seeking for an editor to relieve him of the more trying labour of book-making, Prof. Huxley has been fortunate in securing the services of Mr. Rudler, whose knowledge of a great number of branches of science is combined with much literary skill. To this cause we may attribute the small number of inaccuracies in either fact or expression which a careful perusal of the work has revealed. Such as do occur may be easily remedied in the new edition, which we have no doubt will soon be called for.

In concluding this notice we cannot refrain from congratulating its author upon the production of the work, and at the same time of assuring him that among all the labours he has undergone, and the sacrifices he has made on behalf of elementary education in this country, none is likely to produce more valuable and more enduring fruit than this much-needed model of the art of teaching the fundamental truths of natural science, the appearance of which at the present time we cannot but regard as being most opportune.

J. W. J.

OUR BOOK SHELF

Myths and Marvels of Astronomy. By Richard A. Proctor. (London: Chatto and Windus, 1878.)

THE author observes in his preface that "the chief charm of astronomy with many does not reside in the wonders revealed to us by the science, but in the lore and legends connected with its history, the strange fancies with which in old times it has been associated, the half-forgotten myths to which it has given birth," and further remarking that in our own times myths and fancies, startling inventions and paradoxes have also found place, he has framed the present volume to meet the tastes of the class of readers which he believes to be specially interested in such matters.

In a work confessedly written with this object in view, perhaps it will hardly be expected that there can be much to require notice in a scientific journal. An important point will be accuracy of detail, and in this respect, except in two or three cases, we remark little to which exception can be taken. Amongst other subjects, the author enters upon "the religion" and the mysteries of the Great Pyramid, "Suns in flames," the rings of Saturn, comets as portents, the notorious lunar-hoax of 1835-36, and the origin of the constellation-figures. He is unlucky in his notice of the first discovery of the famous star of Tycho Brahe in 1572, reproducing from Sir John Herschel's "Outlines," the story of Tycho's attention having been first directed to the object on the evening of November 11, by seeing "a group of country people gazing at a star which he was sure did not exist an hour before." This story is as much a myth as anything in the volume before us, as will be evident to the reader who consults the account of his first observation and of the observations of others given by Tycho himself, and it is strange that the statement which has misled Mr. Proctor should have been continued in the various editions of Sir John Herschel's "Astronomy" since the year 1833. The account given in the chapter "On some Astronomical Myths" of the actual position of the intra-Mercurial planet question is too incomplete to enable the reader to form a competent judgment thereupon, though it may leave him under the impression that there is something mythical about it. Mr. Proctor appears to reject "the idea of wilful deception" on the part of astronomical observers—in which case the mere expression of disbelief in the existence of an intra-Mercurial body or bodies does not assist explanation of recorded observations, more particularly where motion has been remarked. There are a few numerical errors in the volume, as in the note on p. 235, where it is stated that the comet of the August meteors has "a period of at least 150 years;" so long a period would be irreconcilable with the observations, and the very complete investigation by Prof. Oppolzer assigns 121½ years as the most probable length of the revolution. But as already stated there is general accuracy of detail, and the volume will doubtless be found acceptable to the particular class of readers for whom it has been prepared.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Electrical Experiment

THE inclosed letter gives an account of an experiment in which an electric current appears to be produced by the direct action of gravity, a result which, if clearly established, would be new and of considerable scientific interest.

In trying to repeat the experiment yesterday I observed a considerable deflexion of the galvanometer in the direction described by Mr. Pirani, but as this deflexion seemed to occur some seconds *after* the inversion of the tube, I examined the tube and found a small bubble of air working its way up through the solution, and as soon as it came to the top of the tube the deflexion occurred.

I have not yet had time to repeat the experiment without the bubble, but I mention this to show that care must be taken to secure that the electrolyte is homogeneous, and that it does not contain anything which will either sink to the bottom of the tube or float to the top, so as to act alternately on the two electrodes.

The fact that the deflexion continued for some time after the tube was placed horizontally seems to indicate the possibility of something which was shifted from end to end when the tube was inverted, but remained where it was when the tube was only laid on its side.

J. CLERK MAXWELL

Cavendish Laboratory, Cambridge, December 28, 1877

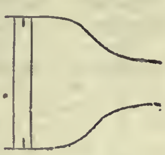
"University of Melbourne, Oct. 30, 1877

"MY DEAR SIR,—On page 317 of vol. i. of your 'Electricity and Magnetism' it is pointed out that a greater electromotive force is required to produce a given current between zinc electrodes in a solution of sulphate of zinc when zinc is carried upwards than when it is carried downwards.

"I am not aware that it has been noticed that by the same

reasoning as that by which the induction of currents is deduced from the force exerted between a circuit and a magnet and the existence of contact electromotive force from the Peltier effect, it follows that a current should exist if two zinc electrodes connected by a wire are immersed in a solution of sulphate of zinc, the direction of the current being (in the solution) from the upper to the lower electrode.

"I tested this a few days ago, using a glass tube eighteen inches long, filled with a saturated solution of sulphate of copper and closed by copper caps with wires attached.



"On connecting the wires with a very delicate Thomson's astatic galvanometer belonging to Prof. Halford, a very considerable deflection was produced (200 divisions) when the tube was held vertically, the direction of the deflection being reversed when the tube was reversed.

"If the tube, after being held vertically, was placed in a horizontal position, the deflection diminished, but several minutes elapsed before the index came to zero, which it eventually did. I cannot explain the time taken. I am now preparing to test the actual loss of weight of the upper electrode.

"I have the honour to be, Sir,
"Your obedient servant,
"F. J. PIRANI,
"Lecturer on Natural Philosophy and Logic,
University of Melbourne.

"P.S.—If the phenomenon has not been noticed before I shall be obliged if you will kindly communicate it to NATURE.
"F. J. P."

The Telephone

I HAVE been much interested in the communication by Dr. Röntgen on a telephonic alarum. During the past six or seven weeks, in investigating the phenomena of the telephone, chiefly as to the suggestions they offer regarding the mechanism of nervous transmission, I have frequently shown to friends the striking experiment described by Dr. Röntgen, and, amongst others, to Sir William Thomson. It has succeeded with Ut_2, Ut_3, and with numerous forks up to Ut_5, but, as stated by Dr. Röntgen, the best result was obtained with Ut_4. With those below this pitch the tone was feeble, whilst with those above it it was transient, in consequence of the difficulty of keeping the small fork going. With Ut_2, worked continuously by an electro-magnet, another fork of the same pitch sounded loudly and steadily. I have also been engaged in some endeavours to record on a moving surface the vibrations of the plate. These have been so successful as to show that it is only a question of delicate adjustment. In endeavouring to utilise one telephone by making several friends listen at once, I have found that by fixing the metal disc to a thin membrane over a small cavity filled with air, like a Koenig's capsule, and having a number of flexible leaden tubes connected with it, an ear placed at the end of each tube will hear distinctly. JOHN G. MCKENDRICK

Physiological Laboratory, University of Glasgow,
December 31, 1877

The Radiometer and its Lessons

PROF. OSBORNE REYNOLDS (vol. xviii. p. 121) appears to have done himself less than justice in the extracts he has sent you from his earlier papers, as representing his published views on the action of residual gas in radiometers. For the extracts do not suffice to constitute an explanation of this action, whereas the papers from which he makes the extracts contained what, if true, might have been an explanation of the action of residual gas, along with much else that is admittedly erroneous; and although those papers (the only ones published before mine) conclude with Prof. Reynolds's own expression of opinion that residual gas is not the cause of the force observed by Mr. Crookes.

He quotes three paragraphs. In two of these he recited the fundamental principle in the kinetic theory of gases which he sought to apply. To obtain an explanation of the phenomenon from this principle according to the method pursued by Prof. Reynolds, it was necessary for him (*a*) to establish a law connecting an excess of force perpendicular to the disc with a flow of heat in radiometers, and (*b*) to indicate agencies which could occasion a sufficient flow of heat. He quotes the passage in which he announced the result of his, as I believe, unsuccessful attempt to accomplish the former of these, but he omits the equally necessary passage in which he dealt with the latter. It will be found at page 407 of the *Proceedings* of the Royal Society, vol. xxii., and is couched in the following terms:—"It must be remembered that ϵ [which measures the outflow of heat] depends on the rate at which cold particles will come up to the hot surface, which is very slow when it depends only on the diffusion of the particles of the gas *inter se*, and the diffusion of the heat among them. It will be much increased by convection currents." If this passage, as was requisite, had been added to the extracts made by Prof. Reynolds, it would have brought his recent account of the views he had announced into conformity with my account of them.

In connection with this subject it should be observed that Prof. Osborne Reynolds has in express terms excluded from his explanation that which I believe to be the real agency which brings a sufficient supply of cold molecules up to the hot surface, for he states, in his letter to NATURE (vol. xvii., p. 27), that "it is incompatible with his explanation that the increase resulting from rarefaction in the mean length of the path of the gaseous molecules would favour the action." Now the polarisation of the gas depends on the ratio which this mean length bears to the interval between heater and cooler.

I cannot find anywhere in Prof. Osborne Reynolds's writings an explanation of the thing to be explained, viz., that the stress in a Crookes's layer is different in one direction from what it is at right angles to that direction. Let v be the component of the momenta of the molecules striking a square unit of the heater in the unit of time, resolved perpendicularly towards the heater; and let u be the corresponding normal component of their momenta from the heater, when they are thrown off. Then $u + v$ is the pressure on the heater. Now if u and v could result respectively from *unpolarised* motions in the gas, the momentum resolved parallel to the heater would be $\frac{1}{2}u + \frac{1}{2}v$ from left to right, with an equal momentum from right to left. Adding these we find $u + v$ the pressure of the gas parallel to the heater. This is equal to the normal pressure, and, therefore, under these circumstances, there would be no Crookes's force whatever. It is only when we take the polarisation of the gas into account that the momenta resolved parallel to the heater become different from $\frac{1}{2}u$ and $\frac{1}{2}v$.

Prof. Osborne Reynolds says that my views are at variance with results arrived at by Clausius and other discoverers in this branch of physics. I do not myself value appeals to authority in matters of science. But it so happens that here again it appears to be Prof. Reynolds who makes the mistake. Clausius, in his great memoir on the conduction of heat by gases, published in 1862 (*Phil. Mag.*, vol. xxiii. p. 529), warns his readers against the very error into which Prof. Reynolds seems to fall, and points out that there "are obvious limits" beyond which the laws he had discovered for the conduction of heat do not prevail, one of which limits is that the gas "must not be so expanded that the mean length of excursion of the molecules becomes so great that its higher powers cannot be neglected." Now it is just to this excepted case, to the Sprengel vacua experimented on by Mr. Crookes, that Prof. Osborne Reynolds applies the laws of conduction, and he then objects to my theory that it does not agree with the laws so misapplied. The phenomenon of Crookes's stress appears to come into existence precisely in Clausius's excepted case, viz., so soon as the ratio which the mean length of excursion of the molecules bears to the interval between heater and cooler, is such, that when multiplied by a function of the temperatures of the heater and cooler, its square is of appreciable magnitude in Clausius's equations. This may be experimentally secured either by placing the heater and cooler very close together, as in experiments upon spheroidal drops, or by excessively attenuating the gas so as to lengthen the free paths of the molecules sufficiently, as in radiometers. G. JOHNSTONE STONEY

Dublin, December 20

POSTSCRIPT, December 22.—I have just seen Prof. Schuster's letter (NATURE, vol. xvii. p. 143). Dr. Schuster will pardon me if I say that he has adopted a scarcely legitimate course in introducing into a discussion on priority his present reminiscence of one of the conversations about the radiometer which he held with his friend, Prof. Osborne Reynolds, two and a half years ago. The language in which he reports it is foreign to Prof. Reynolds's style of composition, so that we may conclude we are dealing with Dr. Schuster's words, and the words which occurred to him after he had read much else on the subject. No

judicially-minded person would attach much weight even to a report of his own, drawn up under such circumstances, and all judicially-minded persons will regret its introduction here. Prof. Osborne Reynolds's reasoning proceeds on the hypothesis that the gas is not polarised. The only real question here is, Is Prof. Schuster prepared to maintain that this reasoning is correct?

Prof. Schuster, in reporting his reminiscences, first recites a kinetic principle which is quite consistent with there being as much force sideways as perpendicularly to the disc, and which therefore contains no explanation of the phenomenon; and when he comes to the first essential point, viz., that which requires him to show that "an increased pressure on the cold side of the vanes of a radiometer will not counterbalance the force acting on the blackened sides," all that he has to say on the subject is that "he does not think that such is the case!" This is the essential thing to be *proved* before the explanation can be accepted, and he recites experiments which show that it is essential.

Prof. Schuster concludes this part of his letter with the admission that "he does not see how [on his theory] an increase in the force can take place" as the exhaustion proceeds. So much the worse for the theory, since experiment indicates that such an increase in the force does take place. In proof of this I may allege, in addition to Mr. Crookes's experiments, several series of experiments made by Mr. Moss, one of the most judicially-minded, patient, and dexterous manipulators I have met with. The experiments were made with the apparatus described in a communication from him and myself, published last spring in the *Proceedings* of the Royal Society, and the effect of the convection current was with extreme care excluded in two ways—by placing the swinging disc where the influence of the convection current on it before and behind was balanced, and by observing the motions that arose before the convection current had time to reach the disc. Both methods concurred in showing that, as in Mr. Crookes's experiments, the force on the disc uniformly increased with increasing attenuation of the gas up to the limit to which we pushed the exhaustion. Mr. Crookes has shown that beyond that limit it begins to decrease. Prof. Schuster will do a real service to science if he will devote his great skill for some months to repeating these and other concurrent experiments, and either confirm them or point out why they should be set aside.

Prof. Schuster thinks that "any theory of the radiometer which makes the action depend on the comparatively large [? small] ratio of the mean free path to the dimensions of the vessel, must necessarily be wrong." Has not Prof. Schuster here overlooked the minuteness of the phenomenon which has to be accounted for? Spheroidal drops of less than a millimetre diameter are easily formed of several light liquids. The Crookes's stress which supports these is an excess of vertical stress over horizontal stress in the supporting layer of polarised gas, amounting to about the 12- or 15-thousandth part of the whole stress. This compares favourably with the minute ratio to which Prof. Schuster refers.

I will not at present enter on that part of Prof. Schuster's letter in which he criticises my published views on penetration, as he refers me to the researches of Messrs. Kundt and Warburg, which I have not yet seen. G. JOHNSTONE STONEY

Glaciation of Orkney

LAST spring Prof. Geikie informed me of a correspondence which was going on in the columns of NATURE as to the question whether the Orkney Islands bore evidence of having been glaciated. It was with much surprise that I heard that there could be any question on this point at all, but I refrained from submitting my opinion to the public—unhesitating though that opinion was—on account of my being then just about starting for my native county, and thus having an opportunity of very specially directing my attention anew to the matter. As the observations I then made *without exception* tended to confirm me in what really required no confirmation, I think I may now come forward as one who has for long known those islands, and who has made a very special geognostic survey of them, during many years. And I would first say, as regards the question, "whether Orkney does or does not give proof of having been covered by a great ice-sheet?" that I believe that no one who has educated his eye—not by looking at pictures in books, but among the rocks themselves—to the *apprehension* and recognition of the hill-contours of an ice-scalped country, would hesitate to declare Orkney to be such. Let such a one take his stand, at a sufficient altitude, anywhere along the north coast of Sutherland, with a scratched and polished boss under his feet, rolling up into rounded hillocks on every side, and sweep his eye from the two Ben Griams over to Hoy, and he could not but exclaim, "*There* is a country which has suffered sore."

In having to differ from Mr. Laing, I join issue with him on two points—boulders and foreign stones, and boulder-clay. I have also to corroborate Prof. Geikie as to glaciation near Stromness; for I, during last summer, saw to the immediate north-west of Stromness a surface of gneiss, say ten feet by three, most unmistakably glaciated—both scratches and polishing being shown.

Now as regards "boulders and foreign stones." Mr. Laing will find—I wonder at his not knowing of it—about 100 yards to the west of the House of Saval, in Sanday, one of the finest boulders in Scotland. This boulder, of great size, consists of hornblendic gneiss; for long I was unable to identify it with any variety of the hornblendic gneiss of Sutherland; but this very year's work enables me to say that it is *very similar* to that of a locality near Durness. In all probability, however, its parent rock lay east, not west.

Another boulder I have heard of, but not seen; it was described to me under the name of the "Eagle Stone"; it lies upon the side of a hill in Westray, near Pierowall, and is said to be peculiar as a loose stone, both on account of its toppling position, its being different from any rock in Orkney, and from there being no rock near it.

As to there being "foreign stones" in Orkney, I shall only say that I have at present in my collection polished jaspers, picked up in rolled masses in Orkney; and that fragments of broken agates are found not unfrequently, on the hill tops and sides, in Hoy. These are quotations, *ex grege*.

Mr. Laing's very precise observations on the *clay beds*—let us call them—do call for special investigation.

If the conclusions arrived at by other observers are found to coincide with his—while they could not affect the ultimate decision as regards the ice-clad country—they certainly would strike these clay beds out of the category of boulder-clays. But, sir, I have seen these clays, and I did not see what Mr. Laing saw; and what I did observe leads me to doubt the correctness of his conclusions. For I found it to be a notable circumstance as regards these Orkney clay-beds that they are very markedly *clay*-beds; *i.e.*, that the amount of clay relatively to that of the stoney matter therein is very much greater than that of most boulder-clays.

Now this is a fact which saps the very foundation of Mr. Laing's observation—an abnormally argillaceous clay bed being the result of the disintegration of a normally siliceous sandstone is difficult to conceive. Nay more, although the cement of certain of the Orkney beds is silicate of alumina, forming the blue argillaceous flag, it is an unquestionable fact, that these flags do not disintegrate by the action of the weather. Even the Picts knew that when they built their Broughs thereof. Silicate of alumina is not affected by carbonated waters.

Upon—nearly all along—the west shore of Shapinsha there are cliffs—sea-cliffs of these clay beds, which lie *betwixt the rocks*, or the last visible rock, *and the sea*; that last rock is a red ferruginous loose-grained sandstone, with little or no cement, what there is being micaceous; the clay beds are ochre yellow. The disintegration of this rock never could have yielded these clay beds.

But Mr. Laing may argue that they resulted from the decay of an overlying argillaceous bed. The argument will not stand. Firstly, because the dip is the wrong way; the rock dips at a high angle to the east; the clay *slightly* caps it, and stands as a bank between its escarpment and the sea. Secondly, because a friable yellow freestone, destitute of argillaceous cement, should overlie the red beds. Thirdly, because on the other side of the bay where the argillaceous flags do appear they are quite permanent. Ice might *grind them up*—the "weather" does not rot them down. But here no clay beds are seen.

Finally, sir, I would request my talented countryman—whom I have great pleasure in breaking a lance with in this field—to consider how or why it is that these clay beds are found only *on one side* of the long depression which runs up the centre of the islands? M. FORSTER HEDDLE

University, St. Andrews, December 19, 1877

Northern Affinities of Chilian Insects

I THINK I may be allowed to express my surprise at Mr. McLachlan's statement that this subject has never yet been

"even more than casually alluded to in works on geographical distribution," and is "*ignored in the principal ones;*" when I have devoted no less than six pages of my book on "The Geographical Distribution of Animals" (vol. ii. pp. 42–48) to a discussion of the main facts—quite as much as could be properly given to it in a general work. It is, however, well worthy of a detailed study, which I am very glad is being undertaken by so competent an entomologist. I hope Mr. McLachlan will endeavour to obtain collections of coleoptera and other orders of insects from the higher tropical Andes, where, I feel confident, some northern forms will also be found.

ALFRED R. WALLACE

Mr. Crookes and Eva Fay

A FEW words from myself seem to be called for by the recent letter of Mr. Crookes in reply to Prof. Carpenter, published in your journal. As far as I am concerned, the breach of etiquette complained of can only apply to my obtaining the publication of the letter Mr. Crookes addressed to me in the *Banner of Light*. The subsequent *facsimile* that appeared I am not in any way responsible for.

The part I took in the matter is very simple, and may be briefly explained as follows:—On Mrs. Fay's return from England to this country the genuineness of her mediumship was very much doubted, and was the subject of controversy not only in the spiritual journals, but in other papers as well. Having, whilst in England, satisfied myself that the manifestations were real, I defended her to the best of my ability, and on seeing it stated in the *Boston Herald* that Mr. Crookes had withdrawn his confidence in her, I thought it prudent to write to that gentleman, mentioning my reason for so doing. In due time I received a courteous reply, which I at once took to the *Banner* office, never dreaming that Mr. Crookes could have any possible objection to its publication after the articles he had himself published on the subject in the English journals. Months elapsed, when one day to my surprise I met with the *facsimile* letter in the *New York Daily Graphic*. On mentioning the subject to the editor of the *Banner* he also expressed surprise, and stated his inability to account for the publication of the *facsimile*. He at first was of opinion that I had taken the letter away and mislaid it; but on searching, the document was subsequently found in the office. Hereupon both Mr. Colby and myself wrote to the editor of the *Graphic*, requesting him to state how he obtained possession of the original letter, so as to get the *facsimile* prepared; but neither of us received a reply. I then got a gentleman residing in New York to call on the *Graphic* editor on the subject, and was informed that the said editor declined to say how he obtained possession of the letter. Thus the matter stands, and is as inexplicable today as it was at the time it happened.

I entirely exonerate the editor of the *Banner* and his associates from any complicity in the matter, and I trust Mr. Crookes, after this explanation, will see that his imputation against American honour is wholly unfounded.

The publication of the letter in the *Banner* I alone am answerable for; and as I explained in my letter to Mr. Crookes that my object was to meet a statement in a public journal, I of course thought that he must have felt that the reply he forwarded would in all probability be made public use of.

Boston, U.S.A., December 7, 1877 ROBERT COOPER

P.S.—Mr. Crookes errs in speaking of me as "a Boston gentleman." I am an Englishman temporarily located here.—R. C.

Philadelphia Diploma

IN NATURE, vol. xvii. p. 153, it is stated that "A 'Dr.' Harmuth, in Berlin, who received his diploma from Philadelphia, was lately sentenced to pay 300 marks for using the prefix publicly." It is but just to so old and respectable a university as that of Philadelphia to point out that "Dr." Harmuth's diploma could not have been genuine. So-called "Philadelphia degrees" of all sorts are sold by agents, but they have no connection with the University of Philadelphia, nor have they, at present, any connection with the city, though the author of this scandalous imposition once lived there and carried on a disreputable practice as a quack doctor. The public should still be on their guard against Bogus degrees, for diplomas purporting to issue from several American and German universities are still to be had, in some cases on examination *in absentia* and payment of the fee, in others by a money payment only.

C. M. INGLEBY

Valentines, December 26, 1877

Royal Dublin Society

IN justice to myself I beg to state that my function as editor of the Natural Science papers in the "Scientific Proceedings of the Royal Dublin Society" begins only with Part 2 of that journal, and that I had no knowledge whatever of the material contained in Part 1 until it had been printed and circulated. By publishing this I shall be greatly obliged.

ALEX. MACALISTER

Anatomical Museum, Trinity College, Dublin

The Meteor of November 23

I HAVE just seen Capt. Tupman's letter in NATURE (vol. xvii. p. 114). I can give a fairly accurate estimate of the direction of the meteor from Llandudno at the time it burst. Sitting in a lighted room my eye was attracted by a bright bar of light across the hearth-rug similar in shape to a gap in the Venetian blind caused by a broken tape. The light slowly faded out in about the same place, which was easily remembered. I listened intently for a report for perhaps about a minute, gave it up, and then heard what was somewhat like the report of a ship's gun at a short distance.

It was easy afterwards to estimate the direction of the light as two points west of (true) north, and thirty-five degrees above the horizon.

I regret that the time between the fading of the light and the report I can only guess very roughly. It may have been about two minutes.

T. S. PETTY

51, Boundary Road, N.W.

THE SUN'S MAGNETIC ACTION AT THE PRESENT TIME

PERHAPS no result in magnetism has excited so much interest as that which has connected the varying diurnal oscillation of the magnetic needle, and the frequency of the aurora polaris, with the spotted area of the sun's surface, in a common cycle of ten and a half years. Various investigations have been undertaken in order to determine whether other phenomena could not be found which would take a place in this chain.

That the movements of the magnets and the corruscations of the aurora are due to the cause which produces the immense chasms in the sun's envelopes there can be little doubt; but we know nothing of the mode in which the sun acts on our earth to produce these effects, and we have reason to believe that this ignorance has prevented us hitherto from tracing to the same cause atmospheric variations which have been attributed altogether to the solar heating action.

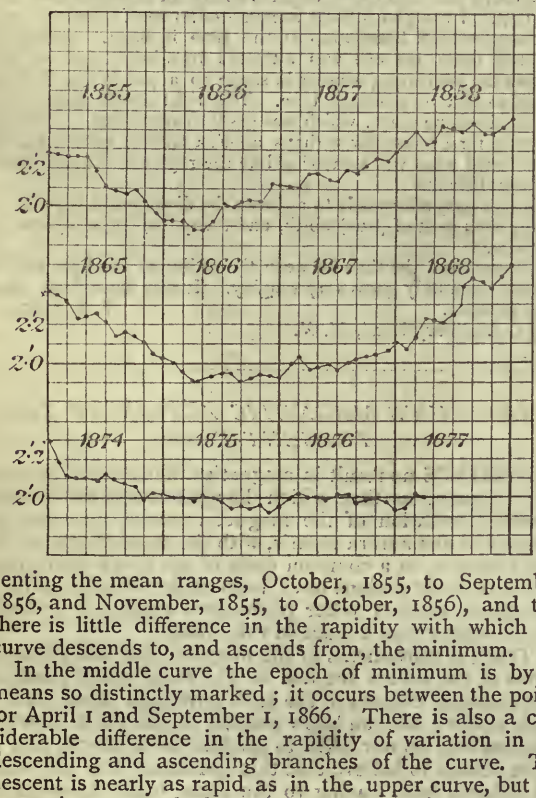
Any facts, then, as to what the sun is doing at the present time with the earth's magnetism will not be without value, whether we regard the facts alone, or as connected with their hypothetical relations to atmospheric phenomena. It should always be remembered, however, that the variations of magnetic oscillations in the decennial period, shown at any one station on the earth's surface, agree generally with those shown over the whole globe, while the meteorological phenomena are so much affected by conditions of position that it is difficult to distinguish what is due to local and what to cosmic causes.

It is well known to those who have studied this subject, that the interval from the time when the sun has fewest, till that when he has most, spots has been less than that from the maximum to the minimum; and that the same fact has been observed in the case of the magnetic oscillations. The way in which the changes of the latter occur near the times of successive minima has not, how-

ever, been studied. The accompanying diagram will show this for the last three minima.

If we suppose that the mean diurnal movement of the magnetic needle is determined for each month, we obtain the amount of the oscillation or range; the mean of the ranges thus found for twelve successive months is represented by a point in the curves; thus the last point in the lowest curve represents the mean of the ranges for the twelve months, October, 1876, to September, 1877 (corresponding to April 1, 1877), as shown by the observations made in the Trevandrum Observatory (nearly on the magnetic equator). The point immediately preceding represents the mean range for the twelve months, September, 1876, to August, 1877, and so on for the other points.

If these curves are examined, it will be seen, that in the upper one the minimum is very clearly marked by two points corresponding to April 1 and May 1, 1856 (repre-



senting the mean ranges, October, 1855, to September, 1856, and November, 1855, to October, 1856), and that there is little difference in the rapidity with which the curve descends to, and ascends from, the minimum.

In the middle curve the epoch of minimum is by no means so distinctly marked; it occurs between the points for April 1 and September 1, 1866. There is also a considerable difference in the rapidity of variation in the descending and ascending branches of the curve. The descent is nearly as rapid as in the upper curve, but the ascent is very much slower.

In the lower curve, the lowest point is that for December 1, 1875, but it is even now, with points for a year and a half later, difficult to say whether this is the minimum or not, the point for January 1, 1877, being only 0'·02 (two-hundredths of a minute of arc) higher. In this curve the change of range in the diurnal oscillation is quite insignificant from November 1, 1874, to April 1, 1877, including the ranges from May 1, 1874, to September 30, 1877, an interval of three years and five months. If this result is confirmed by other observations, as I believe will be the case, no such constant state of the sun's magnetic action will have been observed since the last years of the eighteenth century.

The observations of sun-spots, even if they give as accurate a measure of the intensity of the cause as that obtained from the movements of our magnets, cannot be observed with the same continuity, nor be measured with the same precision; but I have little doubt they will confirm generally the result shown in the last curve, as they have in preceding cases.

With regard to the aurora borealis, the appearances seem to have been very rare during the last two winters. In the report by Capt. Sir G. Nares, on the Arctic expedition, he says that in the winter of 1875-76, "Light flashes of aurora were occasionally seen on various bearings, but most frequently passing through the zenith; none were of sufficient brilliancy to call for notice. The phenomena may be said to have been insignificant in the extreme, and, as far as we could discover, were totally unconnected with any magnetic or electric disturbance" (NATURE, vol. xv. p. 35).

In the twelve months including September, 1843, and August, 1844, including the epoch of minimum disturbance and of auroral frequency, I observed in the south of Scotland (in lat. 55° 35′) thirty appearances of the aurora, and from September, 1844, till the end of 1845, fifty-nine appearances were observed at this single station.[1] Making every allowance for the continuous watch over the magnetic instruments at the Makerstoun Observatory during these years, the difference between Capt. Sir G. Nares' result, in so high latitude, in 1875–76, and that for the south of Scotland, is very distinct. I ought to add, with reference to the apparent want of connection of the faint auroral appearances with the magnetic disturbance noticed by Sir G. Nares, that several of the auroræ observed by me were of the very faintest kind, mere "traces," as I have termed them, which I could never have remarked had I not been warned by *very slight* magnetic irregularities to examine the sky with the greatest attention. Again, in no case have I seen the faintest trace of an aurora without finding at the same time a corresponding irregularity in the movement of the force or declination magnet.

I am unacquainted with any observations of the aurora made in the British Isles during the last two winters;[2] I believe that no scientific institution exists in this country which makes the look-out for aurora throughout the night a definite portion of its work, and that all our knowledge of this phenomenon appears to be left to the chances of some one being out, at the hour of a display, sufficiently bright to attract his attention who will take the trouble to communicate his observation to a public journal.

JOHN ALLAN BROUN

P.S.—I have to thank Mr. A. Buchan for kindly furnishing me with a note of the auroras seen at the stations of the Scottish Meteorological Society during the year 1876. These amounted to forty-two in number, twenty-six in the first half and sixteen in the second half of the year. The greater part were seen in the most northerly stations, including the Orkney, Shetland, and Farö Islands; nine only having been seen south of the Forth. I cannot, however, compare the total result from the hundred stations of the Society with that from the single southerly station of Makerstoun in 1844, since much depends on the nature of the watch kept in each case. It is, however, gratifying to find that so much attention is given at the stations of that highly useful scientific body, the Scottish Meteorological Society, to the observation of this phenomenon.

December 31, 1877

[1] "General Results of the Makerstoun Observations," p. lxxv., *Trans. Roy. Soc. Edin.*, Part 2, vol. xix.

[2] I do not omit Mr. Kinahan's account of "auroric lights," which he saw so frequently in the winter of 1876-77, and which he considered a species of aurora borealis (NATURE, vol. xv. p. 334), as I think there must have been some mistake as to the nature of those lights. He says they were "very common and brilliant during 'the dark days' of December, a few hours before dawn (about five o'clock)." The aurora borealis is very rarely seen at five A.M. in this country. In the two years, 1844 and 1845, during which the aurora was sought for at Makerstoun every hour of the night, it was observed on seventy-seven nights on an average of nearly three hours each night, but it was seen only twice so early, and that with a bright or brilliant aurora which remained during five hours on the first occasion, and from six P.M. to six A.M. on the second. I cannot say, also, that I have ever seen parts of the phenomenon described by Mr. Kinahan, and I had hoped that some other observer in Ireland would have confirmed his observations, which if exact, would be most important, especially as made so frequently at the epoch of minimum.

THE "CHALLENGER" IN THE ATLANTIC[1]

II.

IT still seems but the other day when every zoologist believed with Edward Forbes that not very far below the surface of the sea there existed a region where life was unknown, or where at the most, if it existed it showed but a few sparks, which only served "to mark its lingering presence;" and yet even when Forbes was writing thus, Sir John Ross had brought up from some 800 fathoms deep in Baffin's Bay, "a beautiful *Caput medusæ*," and the present president[2] of the Royal Society had written (August 31, 1845), "It is probable that animal life exists at a very great depth—in the ocean." "On one occasion, off Victoria Land, between the parallels of 71° and 78° S.L., the dredge was repeatedly employed, once with great success at 380 fathoms," and "on another occasion the sounding-line brought up distinct traces of animal life from a depth of 550 fathoms." The history, however, of the subject, is to be found recorded in Sir Wyville Thomson's "Depths of the Sea," and we only here refer to it to remind the reader how completely changed are the general ideas on this subject; and we learn without surprise that "the most prominent and remarkable biological result of the *Challenger's* voyage is the final establishment of the fact that the distribution of living [animal] beings has *no depth limit*, but that animals of all the marine invertebrate classes, and probably fishes also, exist over the whole of the flora of the ocean;" but although life is thus universally extended, probably the number of species as well as of individuals diminishes after a certain depth is reached: This distribution of animal life depends in a marked degree either upon the nature of the sea-bottom or upon the conditions which modify the nature of that bottom. The fauna at great depths was found to be remarkably uniform, and the distribution area seemed to depend mainly on the maintenance of a tolerably uniform temperature. It is curious to note that the families which are peculiarly characteristic of the abyssal fauna, contain a larger number of species and individuals, and these are larger and more fully developed in the Antarctic Ocean, than they are in the Atlantic and the North Pacific.

Though the task of determining the various animal forms procured will occupy a number of specialists for several years, still we have several glimpses of the riches of the ocean fauna in these two volumes. Among these the pretty Hexactinellid sponges, the stalked crinoids, and the echinoids seem to hold foremost places. The stalked crinoids with their lily-like forms are the most remarkable of these, not only on account of their extreme rarity, but also on account of the special interest of their relation to many well-known fossil forms. Of one of these fine forms we give the accompanying illustration (Fig. 3). It was dredged from a depth of about 400 fathoms, near the Island of San Miguel. It belongs to the genus *Pentacrinus*, and has been called after Capt. Maclear, R.N., the commander of the *Challenger*. The lily-shaped head is about 3½ inches in height, and the stalk may have been several inches longer. The scientific description of such a form must necessarily be very technical, and not easily to be understood by the general reader, who, however, cannot fail to get a correct idea of its general form and

FIG. 3.—*Pentacrinus maclearanus*, Wyville Thomson. Slightly enlarged.

[1] "The Voyage of the *Challenger*. The Atlantic: a Preliminary Account of the General Results of the Exploring Voyage of H.M.S. *Challenger* during the Year 1873 and the Early Part of the Year 1876." By Sir C. Wyville Thomson, Knt., LL.D., F.R.SS. L. and E., &c., Regius Professor of Natural History in the University of Edinburgh, and Director of the Civilian Scientific Staff of the *Challenger* Exploring Expedition. Two volumes. Published by Authority of the Lords Commissioners of the Admiralty. (London: Macmillan and Co., 1877.) Continued from p. 148.

[2] Sir Joseph Hooker, C.B.

appearance from the illustration. The special volume in which the whole group of these lily-like starfish will be described is, we understand, to be from the pen of Sir Wyville Thomson.

Though the zoological treasures obtained by dredging were often very great, yet sometimes this often prolonged operation ended in utter disappointment; for example:—The vessel was on her way from Bahia to the Cape, when, on October 2, "we saw our first albatross sailing round the ship with that majestic careless flight which has been our admiration and wonder ever since; rising and sinking, and soaring over us in all weathers, utterly regardless of the motion of the ship, and without the slightest apparent effort. I have often watched these glorious birds for hours from the bridge, and notwithstanding all we know or think we know about the mechanics of flight, to the last I felt inclined to protest that for so heavy a bird to support itself motionless in the air, and perform its vigorous evolutions without a perceptible movement of the wings, was simply impossible by any mechanical means of which we have the least conception.

"On the 3rd we sounded in 2,350 fathoms with a bottom of red mud, still due apparently in a great degree to the South American rivers, and a bottom temperature of 0°·8 C. The trawl was lowered, and on heaving in, it came up apparently with a heavy weight, the accumulators being stretched to the utmost. It was a long and weary wind-in, on account of the continued strain; at length it came close to the surface, and we could see the distended net through the water; when, just as it was leaving the water, and so greatly increasing its weight, the swivel between the dredge-rope and the chain gave way, and the trawl with its unknown burden quietly sank out of sight. It was a cruel disappointment—every one was on the bridge, and curiosity was wound up to the highest pitch; some vowed that they saw resting on the beam of the vanishing trawl the white hand of the mermaiden for whom we had watched so long in vain; but I think it is more likely that the trawl had got bagged with the large sea-slugs which occur in some of these deep dredgings in large quantity, and have more than once burst the trawl net."

FIG. 4.—*Claaodactyla crocea*, LESSON. Stanley Harbour, Falkland Islands. Natural size.

Among the interesting creatures met with living, not in the depths of the sea, but in this instance living amid the fronds of one of the larger algæ, was a Holothuroid, of which we have the following account:—

"The weather while we were at the Falklands was generally cold and boisterous, and boat-work was consequently uncomfortable and frequently impracticable, except in the shallow water within the harbour; we had, however, two or three days' dredging in the pinnace, and made a pretty fair account of the submarine inhabitants of our immediate neighbourhood. *Macrocystis pyrifera*, the huge tangle of the Southern Seas, is very abundant in Stanley Harbour, anchored in about ten fathoms, the long fronds stretching for many yards along the surface and swaying to and fro with the tide. Adhering to the fronds of macrocystis there were great numbers of an elegant little cucumber-shaped sea-slug (*Cladodactyla crocea*, Lesson, sp.), from 80 to 100 mm. in length by 30 mm. in width at the widest part, and of a bright saffron-yellow colour. The mouth and excretory opening are terminal; ten long, delicate, branched oral tentacles, more resembling in form and attitude those of *Ocnus* than those of the typical *Cucumariæ*, surround the mouth; the perisom is thin and semi-transparent, and the muscular bands, the radial vessels, and even the internal viscera can be plainly seen through it. The three anterior ambulacral vessels are approximated, and on these the tentacular feet are numerous and well developed, with a sucking-disc supported by a round cribriform calcareous plate, or more frequently by several wedge-shaped radiating plates arranged in the form of a rosette; and these three ambulacra form together, at all events in the female, a special ambulatory surface.

"The two ambulacral vessels of the 'bivium' are also approximated along the back, and thus the two interambulacral spaces on the sides of the animal, between the external trivial ambulacra and the ambulacra of the bivium, are considerably wider than the other three; consequently, in a transverse section, the ambulacral vessels do not correspond with the angles of a regular pentagon, but with those of an irregular figure in which three angles are approximated beneath and two above. In the female the tentacular feet of the dorsal (bivial) ambulacra are very short; they are provided with sucking-discs, but the calcareous support of the suckers is very rudimentary, and the tubular processes are not apparently fitted for locomotion. In the males there is not so great a difference in character between the ambulacra of the trivium and those of the bivium; but the tentacles of the latter seem to be less fully developed in both sexes, and I have never happened to see an individual of either sex progressing upon, or adhering by, the water-feet of the dorsal canals.

"In a very large proportion of the females which I examined, young were closely packed in two continuous fringes adhering to the water-feet of the dorsal ambulacra. The young were in all the later stages of growth, and of all sizes from 5 up to 40 mm. in length; but all the young attached to one female appeared to be nearly of the same age and size. Some of the mothers with older families had a most grotesque appearance—their bodies entirely hidden by the couple of rows, of a dozen or so each, of yellow vesicles, like ripe yellow plums ranged along their backs, each surmounted by its expanded crown of oral tentacles; in the figure the young are represented about half-grown. All the young I examined were miniatures of their parents; the only marked difference was that in the young the ambulacra of the bivium were quite rudimentary—they were externally represented only by bands of a somewhat darker orange than the rest of the surface, and by lines of low papillæ in the young of larger growth; the radial vessels could be well seen through the transparent body-wall; the young attached themselves by the tentacular feet of the trivial ambulacra, which are early and fully developed.

"We were too late at the Falklands (January 23) to see the process of the attachment of the young in their nursery, even if we could have arranged to keep specimens alive under observation. There can be little doubt that, according to the analogy of the class, the eggs are impregnated either in the ovarial tube or immediately after their extrusion, that the first developmental stages are run through rapidly, and that the young are passed back from the ovarial opening, which is at the side of the mouth, along the dorsal ambulacra, and arranged in their places by the automatic action of the ambulacral tentacles themselves."

One other illustration we take, this time from an animal living in the surface water, though it sinks, when dead, to the bottom of the sea (Fig. 5).

"*Hastigerina murrayi* is very widely distributed on the surface of warm seas, more abundant, however, and of larger size in the Pacific than in the Atlantic. The shell consists of a series of eight or nine rapidly enlarging inflated chambers coiled symmetrically on a plane; the shell-wall is extremely thin, perfectly hyaline, and rather closely perforated with large and obvious pores. It is beset with a comparatively small number of very large and long spines. The proximal portion of each spine is formed of three laminæ, delicately serrated along their outer edges, and their inner edges united together. The spines, when they come near the point of junction with the shell, are contracted to a narrow cylindrical neck, which is attached to the shell by a slightly expanded conical base. The distal portion of the spine loses its three diverging laminæ, and becomes flexible and thread-like. The sarcode is of a rich orange colour from included highly-cooured oil-globules.

"On one occasion in the Pacific, when Mr. Murray was out in a boat in a dead calm collecting surface creatures, he took gently up in a spoon a little globular gelatinous mass with a red centre, and transferred it to a tube. This globule gave us our first and last chance of seeing what a pelagic foraminifer really is when in its full beauty. When placed under the microscope it proved to be a *Hastigerina* in a condition wholly different from anything which we had yet seen. The spines, which were mostly unbroken, owing to its mode of capture, were enormously long, about fifteen times the diameter of the shell in length; the sarcode, loaded with its yellow oil-cells, was almost all outside the shell, and beyond the fringe of yellow sarcode the space between the spines, to a distance of about twice the diameter of the shell all

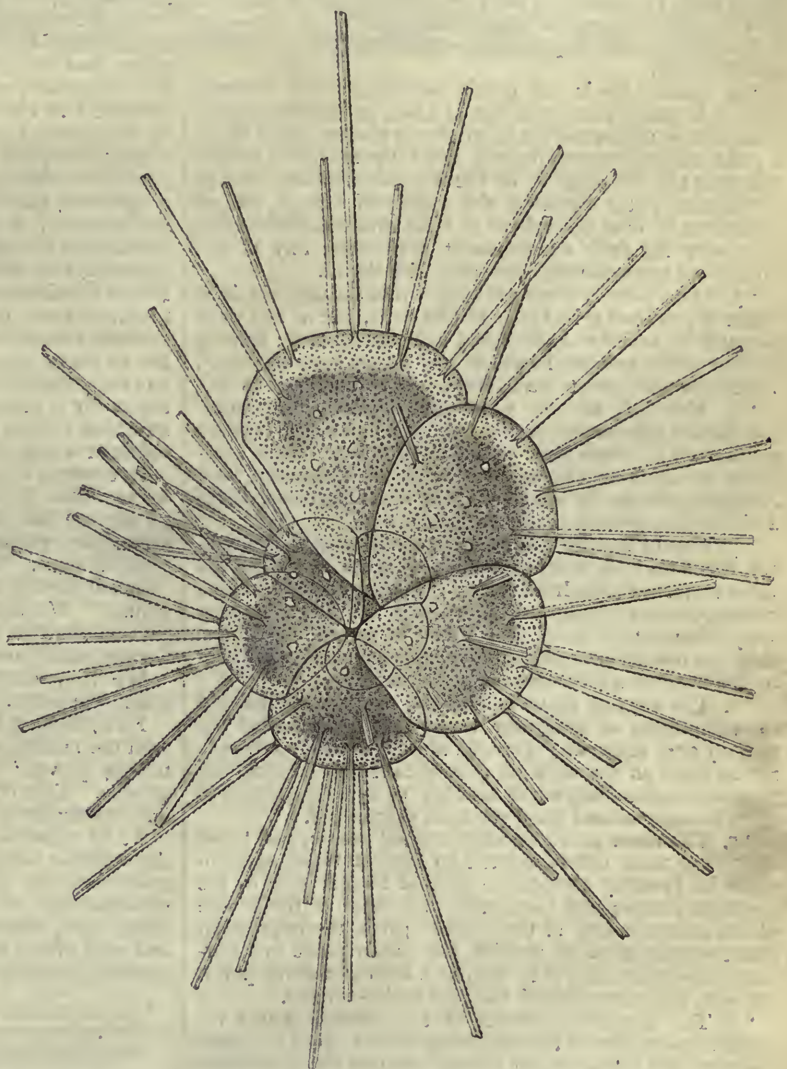


Fig. 5.—*Hastigerina murrayi*, Wyville Thomson. From the surface. Fifty times the natural size.

round, was completely filled up with delicate *bullæ*, like those which we see in some of the Radiolarians, as if the most perfectly transparent portion of the sarcode had been blown out into a delicate froth of bubbles of uniform size. Along the spines fine double threads of transparent sarcode, loaded with minute granules, coursed up one side and down the other, while between the spines independent thread-like pseudopodia ran out, some of them perfectly free, and others anastomosing with one another or joining the sarcodic sheaths of the spines, but all showing the characteristic flowing movement of living protoplasm."

It would be easy to extend our notice on the animal forms alluded to, but our space forbids. It is curious that no vegetable life seems to have been met with in depths below 100 fathoms. "No plants live, so far as we know, at great depths in the sea; and it is in all probability essentially inconsistent with their nature and mode of nutrition that they should do so." But parasitic alga have been detected in some of the deep-sea corals, and we are a little surprised to see the position of the diatoms queried; surely their plant affinities cannot now be discussed, and without these little plants we fancy some of the plant-eating deep-sea forms of animal life would be badly off. Holothuroids are especially fond of them.

The following general conclusions are arrived at:—

"1. Animal life is present on the bottom of the ocean at all depths.

"2. Animal life is not nearly so abundant at extreme as it is at more moderate depths; but, as well-developed members of all the marine invertebrate classes occur at all depths, this appears to depend more upon certain causes affecting the composition of the bottom deposits, and of the bottom water involving the supply of oxygen, and of carbonate of lime, phosphate of lime, and other materials necessary for their development, than upon any of the conditions immediately connected with depth.

"3. There is every reason to believe that the fauna of deep water is confined principally to two belts, one at and near the surface, and the other on and near the bottom; leaving an intermediate zone in which the larger animal forms, vertebrate and invertebrate, are nearly or entirely absent.

"4. Although all the principal marine invertebrate groups are represented in the abyssal fauna, the relative proportion in which they occur is peculiar. Thus Mollusca in all their classes, Brachyourous Crustacea, and Annelida, are on the whole scarce; while Echinodermata and Porifera greatly preponderate.

"5. Depths beyond 500 fathoms are inhabited throughout the world by a fauna which presents generally the same features throughout; deep-sea genera have usually a cosmopolitan extension, while species are either universally distributed, or, if they differ in remote localities, they are markedly representative, that is to say, they bear to one another a close genetic relation.

"6. The abyssal fauna is certainly more nearly related than the fauna of shallower water to the faunæ of the tertiary and secondary periods, although this relation is not so close as we were at first inclined to expect, and only a comparatively small number of types supposed to have become extinct have yet been discovered.

"7. The most characteristic abyssal forms, and those which are most nearly related to extinct types, seem to occur in greatest abundance and of largest size in the southern ocean; and the general character of the faunæ of the Atlantic and of the Pacific gives the impression that the migration of species has taken place in a northerly direction, that is to say, in a direction corresponding with the movement of the cold under-current.

"8. The general character of the abyssal fauna resembles most that of the shallower water of high northern and southern latitudes, no doubt because the conditions of temperature, on which the distribution of animals mainly depends, are nearly similar."

These volumes form a distinct contribution to Science, and will certainly be welcomed by the scientific worker; and their interest to the general reader, who can pass over the few technical descriptions of the new forms, will be scarcely at all less.

THE MODERN TELESCOPE[1]

III.

We know that both with object-glasses and reflectors a certain amount of light is lost by imperfect reflection in the one case, and by reflection from the surfaces and absorption in the other; and in reflectors we have generally two reflections instead of one. This loss is to the distinct disadvantage of the reflector, and it has been stated by authorities on the subject, that, light for light, if we use a reflector, we must make the aperture twice as large as that of a refractor in order to make up for the loss of light due to reflection. But Dr. Robinson thinks that this is an extreme estimate; and with reference to the four-foot reflector now in operation at Melbourne, and of which mention has already been made, he considers that a refractor of 33·73 inches aperture would be probably something like its equivalent if the glass were perfectly transparent, which is not the case.

On the assumption, therefore, that no light is lost in transmission through the object-glass, Dr. Robinson estimates that the apertures of a refractor and a reflector of the Newtonian construction must bear the relation to each other of 1 to 1·42. In small refractors the light absorbed by the glass is small, and therefore this ratio holds approximately good, but we see from the example just quoted how more nearly equal the ratio becomes on an increase of aperture, until at a certain limit the refractor, aperture for aperture, is surpassed by its rival, supposing Dr. Robinson's estimate to be correct. But with specula of silvered glass the reflective power is much higher than that of speculum metal; the silvered glass being estimated to reflect about 90 per cent.[1] of the incident light, while speculum metal is estimated to reflect about 63 per cent.; but be these figures correct or not, the silvered surface has undoubtedly the greater reflective power; and, according to Sir J. Herschel, a reflector of the Newtonian construction utilises about seven-eighths of the light that a refractor would do.

In treating of the question of the future of the telescope, we are liable to encroach on the domain of opinion, and go beyond the facts vouched for by evidence, but there are certain guiding principles which are well worthy of consideration. These have lately been discussed by Mr. Howard Grubb in a paper "On Great Telescopes of the Future." We shall take up his points *seriatim*, premising that in the two classes of telescopes, refractors and reflectors, each possesses some advantages over the other.

We may conveniently consider first the advantages which the refractor has over the reflector.

First, there is less loss of light with the former than with the latter, *as a rule*, hence for equal "space-penetrating power" the aperture of the reflector must be greater. This condition gives us a greater column of air and consequently greater atmospheric disturbance.

"The refractor having a tube closed at both ends, and the reflector being open at the upper end, the condition of air-currents is quite different in the two cases, to the disadvantage of the reflector, for in it the upper end being open, there is nothing to prevent currents of hot and cold air up and down the tube, and in and out of the aperture, and for this reason great advantage has been

[1] Continued from p. 127.

[1] Sir John Herschel, in his work on the telescope, gives the following table of reflective powers:—

After transmission through one surface of glass not in contact with any other surface	0·957
After transmission through one common surface of two glasses cemented together	1·000
After reflection on polished speculum metal at a perpendicular incidence	0·632
After reflection on polished speculum metal at 45° obliquity...	0·690
After reflection on pure polished silver at a perpendicular incidence	0·905
After reflection on pure polished silver at 45° obliquity ...	0·910
After reflection on glass (external) at a perpendicular incidence	0·043

The effective light in reflectors (irrespective of the eye-pieces) is as follows:—

Herschelian (Lord Rosse's speculum metal)	A. 0·632
Newtonian (both mirrors ditto)	B. 0·436
Do (small mirror or glass prism)...	C. 0·632
Gregorian or Cassegrain	D. 0·399
The same telescopes, all the metallic reflections being from pure silver	A. 0·905 B. 0·824 C. 0·905 D. 0·819

found in ventilating the tubes, *i.e.* making it of some open-work construction, in order that the air may pass through and across and remove currents of differing temperatures. This difficulty is not felt with refractors; but, curious to say, in the largest refractor at present in existence (the Washington 26-inch), Prof. Newcomb informs me that considerable inconvenience is felt sometimes from the outside of the object-glass cooling down more quickly in the evening than the inside, which produces a decided effect on the spherical aberration, and injures temporarily the otherwise fine definition. He consequently recommends the use of lattice or ventilated tubes for very large refractors. If this be found necessary, this advantage of the refractor vanishes."

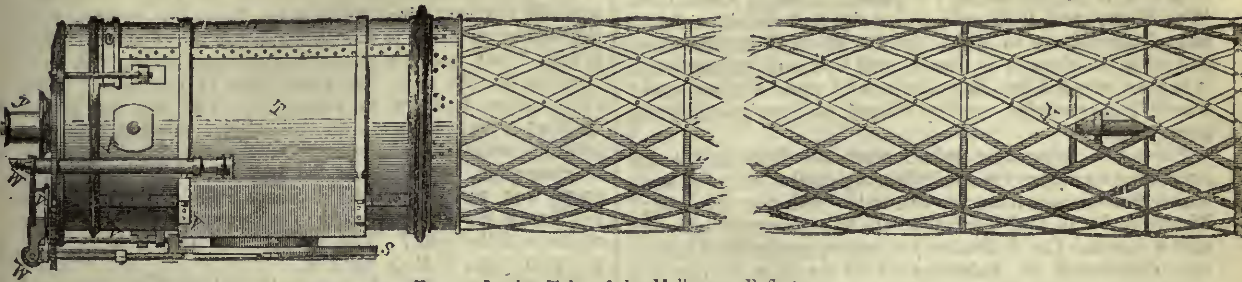


FIG 9.—Lattice Tube of the Melbourne Reflector.

But there is another nice point concerning this larger aperture which has to be considered.

We may set out with observing that the light-grasping power of the reflector varies as the square of the aperture multiplied by a certain fraction representing the proportion of the amount of reflected light to that of the total incident rays. On the other hand the power of the refractor varies as the square of the aperture multiplied by a certain fraction representing the proportion of transmitted light to that of the total incident rays. Now in the case of the reflector the reflecting power of each unit of surface is constant whatever be the size of the mirror, but in that of the refractor *the transmitting power decreases with the thickness of the glass*, rendered requisite by increased size. Although for small apertures the transmitting power of the refractor is greater than the reflecting power of the reflector, still it is obvious that on increasing the size a stage must be at last reached when the two rivals become equal to each other. This limit has been estimated by Dr. Robinson to be 35·435 inches, a size not yet reached by our opticians by some ten inches, but object-glasses are increasing inch by inch, and it would be rash to say that this size cannot be reached within perhaps the lifetime of our present workers. However this may be we can say with safety that up to the present limit of size produced, refractors have the advantage in light-grasping power, and it is also a question whether with increase of thickness in the glass there will not be such an increase in the purity of material and polish as to keep the loss by transmission at its present value. Any one who has a Tully and a Cooke object-glass, by placing them side by side on a clean sheet of paper, will be able to see how our modern opticians have already reduced the loss by transmission.

The next point worthy of attention is the question of permanence of optical qualities. Here the refractor undoubtedly has the advantage. It is true that the flint glass of some object-glasses, chiefly those produced in Germany, gets attacked by a sort of tarnish, still that is not the case generally, while on the other hand, metallic mirrors often become considerably dimmed after a few months of use, the air of a town seeming to be fatal to them, and although repolishing is not a matter of any great difficulty in the hands of the maker, still it is a serious drawback to be obliged to return mirrors for this purpose. There are, however, some exceptions to this, for there are many small mirrors in existence whose polish is good after many years of continuous use, just as on the other hand there are many object-glasses whose polish has suffered in a few years, but these are exceptions to the rule. The same remarks apply to the silvered glass reflectors, for although the silvering of small mirrors is not a difficult process, the matter becomes exceedingly difficult with large surfaces, and indeed at present large discs of glass, say of four or six feet diameter, can rarely be produced. If, however, a process should be discovered of manufacturing these discs satisfactorily and of silvering them, there are objections to them on the grounds of the bad conductivity of glass, whereby changes of temperature alter the curvature, and there is also a great tendency for dew to be deposited on the surface.

With regard to the general suitability for observatory work this depends upon the kind of work required, whether for measuring positions, as in the case of the transit instrument, where permanency of mounting is of great importance, or for physical astronomy, when a steady image for a time only is required. For the first purpose the refractor has decidedly the advantage, as the object-glass can be fixed very nearly immovably in its cell, whereas its rival must of necessity, at least with present appliances, have a small, yet in comparison considerable, motion.

The difficulty of mounting mirrors, even of large size, has now been got over very perfectly. This difficulty does not occur in the mounting of object-glasses of sizes at present in use, but when we come to deal with lenses of some thirty inches diameter, the present simple method will in all probability be found insufficient, but we anticipate that one will be adopted which will allow the permanent position of the object-glass to be retained.

J. NORMAN LOCKYER

(*To be continued.*)

OUR ASTRONOMICAL COLUMN

THE COMET OF 1106.—In Mr. Williams's account of the object observed by the Chinese in this year, and called a comet by Ma Twan Lin, we find the following note:—"This appears to have been a large meteor, as it seems to have been seen for a short time only." It is probable that the author had not compared Pingré's description of the motion of the comet, which was certainly observed in Europe early in the year, or he would have seen that in all likelihood, notwithstanding Ma Twan Lin's account reads as if it referred to a temporary phenomenon, the Chinese really observed the bright comet recorded by the European historians. We are told that in the fifth year of the epoch Tsung Ning, on day Woo Seuh of the first moon (1106, Feb. 10) a

comet appeared in the west; it was like a great Pei Kow (a kind of measure). The luminous envelope was scattered; it appeared like a broken-up star. It was sixty cubits in length and three cubits in breadth. Its direction was to the north-east; it passed through Kwei, Lew, Wei, Maou, and Peih, which are sidereal divisions determined according to Biot by the stars β Andromedæ, β Arietis, α Muscæ, η Tauri, and ϵ Tauri respectively. "It then entered into the clouds, and was no more seen." Gaubil's manuscript, used by Pingré, assigns precisely the same course.

European historians relate that on February 4 (or according to others on the following day) a star was seen which was distant from the sun only "a foot and a half;" Matthew Paris and Matthew of Westminster call this star a comet. On February 7 a comet, properly so called, was discovered in Palestine in "that part of the sky where the sun sets in winter," its ray had "the whiteness of snow," and extended to the commencement of the sign Gemini, below the constellation Orion. As Pingré points out the comet must at this time have had a south latitude, and, considering the sun's position, could not be less advanced than 10° or 12° of Pisces to have been seen in the evening after sunset. The comet subsequently passed by west to north-west, the tail directed to that part of the sky between the north and the east; the comet was visible until the middle of the night, and "shone during twenty-five days in the same manner at the same hour;" as one writer states, it had a real motion from west to east. The length of the comet's appearance is variously given; an eye-witness says that the most piercing sight could hardly distinguish it after fifty days, and a manuscript consulted by Pingré, in the Bibliothèque de Sainte-Geneviève, of the thirteenth century at latest, mentions fifty-six days for the duration of visibility.

The comet of 1106 long attracted attention from the circumstance of Halley having identified it as the famous comet of 1680, an idea which was first disputed by Dunthorne, on the authority of a manuscript preserved in one of the College libraries at Cambridge, which gives the comet's track from the beginning of the sign Pisces (on February 7 as Dunthorne reads) in the order of the signs to the commencement of Cancer, which agrees closely with the path recorded by the Chinese. He considered that this track "quite overbalanced the probability of the identity of the comet with that of 1680"—and this view has been confirmed by subsequent calculation. Again, when astronomers were searching for earlier accounts which might refer to the great comet of 1843, first detected at noon-day on the date of its perihelion passage, this comet of 1106 was fixed upon by MM. Laugier and Mauvais, as probably identical with it, several of the circumstances mentioned above being overlooked by them, particularly the fact of the comet having been observed so long in the northern part of the heavens, where it is impossible that the comet of 1843 could be located.

On carefully weighing the scanty evidence afforded by the records of the time, it appears likely that the elements of the comet of 1106 bore some resemblance to those of the great comet of 1618 (Pingré's third comet), the inclination being smaller.

THE SATELLITES OF MARS.—Both of the newly-discovered satellites of Mars were observed during September with the 12-inch equatorial of the Morrison Observatory, Glasgow, Missouri, by Mr. Pritchett. On September 7 the two satellites could be seen with the planet entirely in the field, and were very distinct when it was shut out of it, and on September 10 and 13, the inner one was easily observed. The outer satellite was again estimated to be of the fourteenth magnitude. The observations of this satellite were made with wires faintly illuminated with a red light; for observations of the inner one the light of the planet sufficed Unfavourable skies prevented any observations in October, though Mr. Pritchett thinks the satellites might have been well followed during that month.

COLOURED DOUBLE STARS.—In Sir John Herschel's seventh catalogue of double stars from the sweeps with the 20-feet reflector is one the position of which identifies it with Σ 724, and the note attached runs thus: "A very curious double star, the small star is very red." The observation belongs to sweep No. 121, for the epoch 1828·05. Struve measured this object in 1829, but says nothing respecting the colours of the components, which he estimated on his scale 8·7 and 10·0. In 1829·85 the angle was 241·5°, and the distance 6·86″. Has any one confirmed Sir John Herschel's observation on the colour of the smaller star? The position for 1878·0 is in R.A. 5h. 33m. 30s., N.P.D. 79° 5′·5.

In *Memorie del' Osservatorio del Collegio Romano*, 1857-59, p, 173, Secchi mentions a wide double star, which is called *nova*, and is thus measured:—

1856·63 Pos. 335°·25 Dist. 23″·83 { Components 7m. and 8m. A red, B blue.

He has the additional remark, "*Colori superbi.*" This object would appear to be formed by Nos. 3743 and 3744 of Zone + 37° of the *Durchmusterung*; positions for 1855·0:—

		h. m. s.		° ′
3743	R.A.	19 58 23·5	N.P.D.	52 4·1
3744	"	19 58 25·5	"	52 4·7

THE TALKING PHONOGRAPH[1]

MR. THOMAS A. EDISON recently came into this office, placed a little machine on our desk, turned a crank, and the machine inquired as to our health, asked how we liked the phonograph, informed us that *it* was well, and bid us a cordial good night. These remarks were not only perfectly audible to ourselves, but to a dozen or more persons gathered around, and they were produced by the aid of no other mechanism than the simple little contrivance explained and illustrated below.

The principle on which the machine operates we

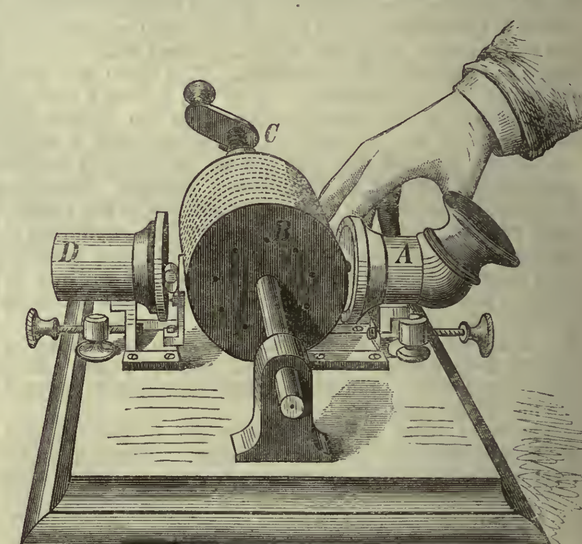


FIG. 1.

recently explained quite fully in announcing the discovery. There is, first, a mouth-piece, A, Fig. 1, across the inner orifice of which is a metal diaphragm, and to the centre of this diaphragm is attached a point, also of metal. B is a

[1] From the *Scientific American* of December 22, 1877.

brass cylinder supported on a shaft which is screw-threaded, and turns in a nut for a bearing, so that when the cylinder is caused to revolve by the crank, C, it also has a horizontal travel in front of the mouthpiece, A. It will be clear that the point on the metal diaphragm must, therefore, describe a spiral trace over the surface of the cylinder. On the latter is cut a spiral groove of like pitch to that on the shaft, and around the cylinder is attached a strip of tinfoil. When sounds are uttered in the mouth-piece, A, the diaphragm is caused to vibrate, and the point thereon is caused to make contacts with the tinfoil at the portion where the latter crosses the spiral groove. Hence, the foil, not being there backed by the solid metal of the cylinder, becomes indented, and these indentations are necessarily an exact record of the sounds which produced them.

It might be said that at this point the machine has already become a complete phonograph or sound writer, but it yet remains to translate the remarks made. It should be remembered that the Marey and Rosapelly, the Scott or the Barlow apparatus, which we recently described, proceed no further than this. Each has its own system of caligraphy, and after it has inscribed its peculiar sinuous lines, it is still necessary to decipher them. Perhaps the best device of this kind ever contrived was the preparation of the human ear made by Dr. Clarence J. Blake, of Boston, for Prof. Bell, the inventor of the telephone. This was simply the ear from an actual subject, suitably mounted, and having attached to its drum a straw, which made traces on a blackened rotating cylinder. The difference in the traces of the sounds uttered in the

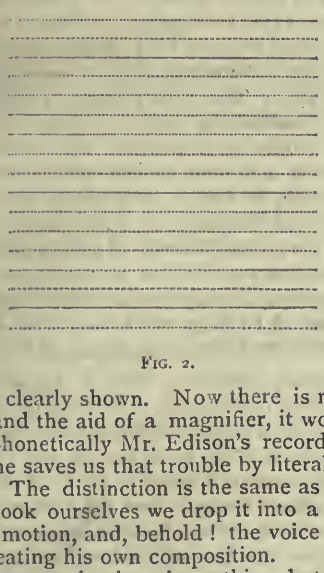


Fig. 2.

ear was very clearly shown. Now there is no doubt that by practice and the aid of a magnifier, it would be possible to read phonetically Mr. Edison's record of dots and dashes, but he saves us that trouble by literally making it read itself. The distinction is the same as if, instead of perusing a book ourselves we drop it into a machine, set the latter in motion, and, behold! the voice of the author is heard repeating his own composition.

The reading mechanism is nothing but another diaphragm held in the tube D on the opposite side of the machine, and a point of metal which is held against the tin foil on the cylinder by a delicate spring. It makes no difference as to the vibrations produced, whether a nail moves over a file or a file moves over a nail, and in the present instance it is the file or indented foil strip which moves, and the metal point is caused to vibrate as it is affected by the passage of the indentations. The vibrations, however, of this point must be precisely the same as those of the other point which made the indentations, and these vibrations, transmitted to a second membrane, must cause the latter to vibrate similar to the first membrane, and the result is a synthesis of the sounds which, in the beginning, we saw, as it were, analysed.

In order to exhibit to the reader the writing of the machine which is thus automatically read, we have had a cast of a portion of the indented foil made, and from this the dots and lines in Fig. 2 are printed in, of course, absolute facsimile, excepting that they are level instead of being raised above or sunk beneath the surface. This is a part of the sentences, "How do you do?" and "How do you like the phonograph?" It is a little curious that the machine pronounces its own name with especial clearness. The crank handle shown in our perspective illustration of the device does not rightly belong to it, and was attached by Mr. Edison in order to facilitate its exhibition to us.

In order that the machine may be able exactly to reproduce given sounds, it is necessary, first, that these sounds should be analysed into vibrations, and these registered accurately in the manner described; and second, that their reproduction should be accomplished in the same period of time in which they were made, for evidently this element of time is an important factor in the quality and nature of the tones. A sound which is composed of a certain number of vibrations per second is an octave above a sound which registers only half that number of vibrations in the same period. Consequently if the cylinder be rotated at a given speed while registering certain tones, it is necessary that it should be turned at precisely that same speed while reproducing them, else the tones will be expressed in entirely different notes of the scale, higher or lower than the normal note as the cylinder is turned faster or slower. To attain this result there must be a way of driving the cylinder, while delivering the sound or speaking, at exactly the same rate as it ran while the sounds were being recorded, and this is perhaps best done by well-regulated clockwork. It should be understood that the machine illustrated is but an experimental form, and combines in itself two separate devices—the phonograph or recording apparatus, which produces the indented slip, and the receiving or talking contrivance which reads it. Thus in use the first machine would produce a slip, and this would for example be sent by mail elsewhere, together in all cases with information of the velocity of rotation of the cylinder. The recipient would then set the cylinder of his reading apparatus to rotate at precisely the same speed, and in this way he would hear the tones as they were uttered. Differences in velocity of rotation within moderate limits would by no means render the machine's talking indistinguishable, but it would have the curious effect of possibly converting the high voice of a child into the deep bass of a man, or *vice versâ.*

No matter how familiar a person may be with modern machinery and its wonderful performances, or how clear in his mind the principle underlying this strange device may be, it is impossible to listen to the mechanical speech without his experiencing the idea that his senses are deceiving him. We have heard other talking machines. The Faber apparatus, for example, is a large affair, as big as a parlour organ. It has a key-board, rubber larynx and lips, and an immense amount of ingenious mechanism which combines to produce something like articulation in a single monotonous organ-note. But here is a little affair of a few pieces of metal, set up roughly on an iron stand about a foot square, that talks in such a way, that, even if in its present imperfect form many words are not clearly distinguishable, there can be no doubt but that the inflections are those of nothing else than the human voice.

We have already pointed out the startling possibility of the voices of the dead being reheard through this device, and there is no doubt but that its capabilities are fully equal to other results just as astonishing. When it becomes possible, as it doubtless will, to magnify the sound, the voices of such singers as Parepa and Titiens will not die with them, but will remain as long as the metal in which they may be embodied will last. The witness in court will find his own testimony repeated by machine, confronting him on cross-examination—the testator will repeat his last will and testament into the machine so

that it will be reproduced in a way that will leave no question as to his devising capacity or sanity. It is already possible by ingenious optical contrivances to throw stereoscopic photographs of people on screens in full view of an audience. Add the talking phonograph to counterfeit their voices, and it would be difficult to carry the illusion of real presence much further.

NOTES

MR. SORBY is busy perfecting his new method of studying minerals. Some very remarkable properties are still unexplained, and only the other day Mr. Sorby made a very great fresh advance in the subject.

IT is probable that Sheffield will be chosen for the meeting of the British Association in 1879. Nottingham was to have been the place of meeting, but a difficulty has arisen respecting the meeting there, and Sheffield has been unofficially written to. The matter is being warmly taken up by some of the principal townsmen, and there can be no doubt with a successful result.

A PHOTOLITHOGRAPHIC plate of the primary triangulation of the United States Geological and Geographical Survey of the Territories, carried on during the summer of 1877, by Mr. A. D. Wilson, chief topographer, has just been published by the United States Geological Survey, under the charge of Dr. F. V. Hayden. The area covered by these triangles extends from Fort Steele, in Wyoming Ty., westward to Ogden, in Utah Ty., a distance of about 260 miles, and north as far as the Grand Teton, near the Yellowstone National Park, including Freemont's Peak of the Wind River Range of the Rocky Mountains. The area embraces about 28,000 square miles, and within it, twenty-six primary stations were occupied, and their positions accurately computed. Besides these occupied stations, a large number of mountain peaks were located, which in the future will be occupied as points for the extension of the topographical work of the Survey. A base line was carefully measured near Rawlin's Springs, on the line of the Union Pacific Railroad, and from this initial base the work was extended north and west to the valley of Bear River, in Idaho Ty. Here a check base was measured, and the system expanded to the neighbouring mountain peaks to connect with the triangulation as brought forward from the first-mentioned base. Along the line of the Union Pacific Railroad the work was connected at six points with the triangulation system of Clarence King's 40th parallel survey. In addition to the importance of this sheet as the base work of the season's topographical work, it presents a most striking feature in the number of remarkably long sights which were taken from the summits of some of the most lofty mountains in the area explored. Many of these sights were over 100 miles in length, while some reach a distance of 135 miles. From Wind River Peak all the prominent points in the Big Horn Mountains were sighted, also the loftier peaks of the Uinta Mountains; the former are located 165 miles to the north-east, while the Uinta Mountains are situated about the same distance to the south-west. As these ranges were not in the scope of the season's work, they are not given on the chart.

THE *Annual Report of the Smithsonian Institution* for the year 1876, which has recently been published, is of general interest. The Institution continues to carry on its usual work with vigour and efficiency. Two important volumes of the Smithsonian Contributions to Knowledge, xx. and xxi., have been issued. The former on the Winds of the Globe, by Prof. Coffin, consisting of 781 quarto pages, is considered to be the most important contribution to knowledge which the Institution has given to the world. It presents a rich mine of information for the use of meteorologists, the physical geographer, and the mariner. Volume xxi. contains the following articles, viz.:—(1) Statements and Expositions of Certain Harmonies of the Solar System, by Prof. Alexander. (2) On the General Integral of Planetary Motion, by Prof. Newcomb. (3) The Haidah Indians by J. G. Swan. (4) Tables of Atmospheric Temperature in America. There has been published an important work on the Antiquities of Tennessee, by Dr. Joseph Jones, and another on the Archæological Collections of the U.S. National Museum; also a supplement to Prof. F. W. Clarke's work on the "Constants of Nature," consisting of tables of specific gravities, boiling and melting points, specific heats, &c. Large additions have during the year been made to the collections of the National Museum in charge of the Institution. In the Appendix to the Report there is a translation of the eulogy on Gay-Lussac by M. Arago; a biographical sketch of Dom Pedro II.; a translation of an important paper of Prof. Pilar on the Revolutions of the Earth's Crust, which will be read with interest by students of physical geography and geology. The subjects discussed in this article are the origin of the earth, central heat, the fluid envelope, organisms, ice, with a concise account of the theory of secular changes of climate resulting from changes in the eccentricity of the earth's orbit, antiquity of man, &c. Then follows a paper by Dr. D. Kirkwood on the Asteroids between Mars and Jupiter. But the article which will probably attract most attention is one by Mr. W. B. Taylor on Kinetic Theories of Gravitation. In this memoir, occupying about eighty pages, is given an interesting historical account of all the principal theories which have been advanced since the time of Newton to the present day to explain the nature of gravitation. Villemot, 1707; Bernouilli, 1734; Le Sage, 1750; Euler, 1760; Herapath, 1816; Guyot, 1832; Faraday, 1844; Seguin, 1848; Boucheporn, 1849; Lamè, 1852; Waterston, 1858; Challis, 1859; Glennie, 1861; Keller, 1863; Tait, 1864; Saigey, 1866; Croll, 1867; Leray, 1869; Boisbaudran, 1869; Guthrie, 1870; Crookes, 1873. These theories are all criticised with considerable acuteness. Mr. Taylor lays down six fundamental characteristics of gravity with which, he asserts, every theory must agree. But unfortunately it is in reference to the truth of some of Mr. Taylor's postulates that the greatest diversity of opinion exists. No kinetic theory of gravitation can fulfil his six conditions. Mr. Taylor seems to misapprehend some of the theories in important points, particularly those of Le Sage and Croll. The Appendix concludes with a number of interesting papers on Ethnology.

WE have already referred to Prof. A. Agassiz's intention of carrying out a series of researches in the Gulf of Mexico. With an assistant he is to be accommodated on board the United States Coast Survey steamer *Blake*, which has just sailed on a surveying cruise that will occupy this winter in the Gulf of Mexico. By a study of the animals dredged from the bottom of the Gulf, Prof. Agassiz will be enabled to make important comparisons with the fauna of the Atlantic, and especially as to growth, habits, migrations, and changes of living forms found in the waters near the British Islands and the Scandinavian Peninsula. The expedition is under the command of Lieutenant-Commander Charles D. Sigsbee, United States Navy, who has had several years experience on coast survey duty, and has been notably successful in deep-sea soundings.

NEW YORK will in all probability have a magnificent new Zoological Garden in Central Park before the end of another year. The Park Commissioners have little doubt that the amount of money, 300,000 dollars, necessary to make a commencement, will be subscribed without difficulty.

THE death is announced of Mr. Robert Hollond, a gentleman formerly well-known in connection with aeronautics.

THE Rev. Horace Waller writes to the *Times* that Col. Mason has been round Lake Albert Nyanza in a steamer, and

corroborates the fact of its being a comparatively small land-locked lake. Col. Mason is in the service of the Khedive.

MR. STANLEY has arrived in Egypt, and is to spend a few days at Cairo. On New Year's Day he was to be entertained at a banquet by Sir George Elliot, M.P., to which the principal English and American visitors and residents were invited.

THE African Association presided over by the King of the Belgians has learnt by telegram that its travellers have safely reached Zanzibar.

AT a recent meeting of the Liverpool Historic Society, Mr. T. Glazebrook Rylands, F.S.A., read an important paper on "Ptolemy's Geography of the Coast from Caernarvon to Cumberland (including Cheshire and Lancashire). The paper was the preliminary result of extensive and long research, during which the author has found out that previous writers have examined Ptolemy's work carelessly or inadequately, and greatly misrepresented his *data*. It has, for example, been inferred for centuries that the Mersey was unknown to Ptolemy, and that the river known as Belisama was identical with the Ribble. This has led to further deductions of an erroneous character; as, for example, that there was a wide sheet of water making the mouths of the Mersey and the Dee undistinguishable, while two islands in it reared their heads, viz., Wallasey, separated by a branch of the tide through Wallasey Pool, and Wirral, separated by a strait almost coincident with the canal from Chester. Mr. Rylands believes—and there can be little doubt of the fact—that he has ascertained the ideas of Ptolemy and verified his measurements and mode of projection in a way wholly unknown to former inquirers. He has thus explained apparent anomalies and corrected misunderstandings of former writers. Commencing southwards at Caernarvonshire, he has verified the positions from beyond Pwlhelli round by Caernarvon and Conway to the Dee; he has verified the positions of the Mersey and the Ribble, and all along the coast to St. Bee's Head, in Cumberland. In several instances where it was thought Ptolemy was in error, Mr. Rylands has shown he is correct, and it is a matter of surprise that where we should expect approximate truth only, the more rigid tests give more accurate results.

THE Bristol Naturalists' Society appears to be in a flourishing condition. It has recently added to its organisation a Physical and Chemical "Section," of which Dr. W. A. Tilden is secretary and Mr. P. J. Worsley president. The recent meetings of the Society have been more largely attended, and there appears to be a revived interest in physical science in the ancient city.

M. GAUTHIER VILLARS has just published a new edition of a highly interesting old book, "Lectures on Chemical Philosophy," delivered at the Collége de France in 1836 by M. Dumas. In this curious work all the prevalent ideas in chemistry were initiated. Not a single sentence has been altered, yet M. Dumas' lectures seem quite fresh and young, ready to be used by students in the highest schools. They were collected by M. Bineau, a gentleman who died twenty years ago, after having been a professor in the Lyons Faculty of Sciences.

THE sittings of the enlarged council of the Paris observatory came to an end last week. The resolutions come to, of which we have already given the substance, have been sent to M. Bardoux, the Minister for Public Instruction. The International Meteorological Service entered, on January 1, the twenty-first year of its existence, and will continue connected with the Paris Observatory, where it was established by M. Leverrier in 1857. The present head of the service is M. Front, a physicist connected with the service for many years, and trained by Leverrier himself. The first physicist-*adjoint* is M. Moureau, formerly a schoolmaster, whom Leverrier remarked for his zeal and assiduity in meteorological researches and observations. The great astronomer required no other scientific qualifications than intelligence and instruction obtained by personal exertion. He turned away many doctors in science and pupils of the highest schools who were wanting in the requirements he was anxious to secure, and sought to find them even in the humblest stations of life.

MR. JOHN FIELDING, of Todmorden, has just presented to the Aquarium at Westminster two specimens of *Proteus sanguineus*, obtained by his courier, C. F. Kohl, from the grotto of Adelsburg. They are said to be the first shown in England. A specimen of *Menobranchus lateralis* has been on view for some little time.

A MOST unfortunate series of disasters followed Mr. Carrington's endeavours to bring to London a collection of specimens of the Mediterranean fauna. Dr. Eisig, of the Naples Aquarium, offered him every facility, suggested localities, and placed some store-tanks at his service. A collection of fish zoophytes and corals was made, and seven tanks were fitted up on a cargo steamer to transport them to England. Shortly after starting a thunderstorm was encountered, the ship was struck by lightning, and the contents of two metal tanks were at once destroyed. Among other things a fine collection of mureen eels averaging 2 feet in length was thus lost. Rough weather for a day or two caused further deaths. After leaving Gibraltar the change of temperature proved excessively fatal, the bright-coloured animals suffering most. The heavy weather in the Channel broke some of the other tanks, so that but few animals reached England alive. Mr. Carrington, however, arranged for supplies with agents at Naples, Messina, Palermo, Valentia, Gibraltar, Tangiers, and Lisbon.

MR. B. RALPH, of Launceston, Cornwall, sends us a ripe strawberry which he gathered on December 29 from a hedge about 400 feet above sea-level. Pink strawberry blossoms, he states, are not uncommon. The thermometer stood at 50° in the shade. Bar. 29·2, with a west wind. Many of the commoner hedgeflowers linger on, such as lychnis and geranium. He also incloses some primroses, blossoming in an exposed situation outside his window.

TWO somewhat forcible shocks of earthquake were felt at Bologna on December 23, and a slight one at Alicante on the preceding day.

PROF. BARRETT, in a recent lecture on the telephone, gave a receipt for making a cheap one. Take a wooden tooth-powder box and make a hole about the size of a half-crown in the lid and the bottom. Take a disc of tinned iron, such as can be had from a preserved meat tin, and place it on the outside of the bottom of the box, and fix the cover on the other side of it. Then take a small bar-magnet, place on one end a small cotton or silk reel, and round the reel wind some iron wire, leaving the ends loose. Fix one end of the magnet near, as near as possible without touching, to the disc, and then one part of the telephone is complete. A similar arrangement is needed for the other end. The two are connected by the wire, and with this Prof. Barrett says he has been able to converse at a distance of about 100 yards.

M. BARDOUX, the new Minister of Public Instruction in France, has held a reception of the several heads of his department and *employés* of the central administration. He delivered a speech insisting upon the necessity for a Republican Government to educate the people, as a good system of public education is the strongest basis on which any Republic can be safely established. According to the *XIX. Siècle* M. Bardoux is not only preparing a Bill for establishing gratuitous elementary education, but also for organising a higher elementary education.

PROF. PFAUNDLER communicated in a recent session of the Vienna Academy the results of some experiments undertaken to decide the question as to the smallest absolute number of vibrations capable of producing a sound. By means of a siren with two openings for blowing, he finds that two isolated vibrations are capable of producing a tone which, by repetition, becomes audible.

THE Meteorological Society of Paris has elected as president M. Hervé Mangon, professor of Agriculture at the Conservatoire des Arts et Métiers.

THE Postal and Telegraph services are to be united in France, as they have been already in England, under a single direction. The first director of the complex organisation will be M. Cocheris, one of the staff of the *Temps* and a well-known writer on matters of political economy.

AT the last meeting, December 19, 1877, of the Russian Geographical Society, M. Mushketov made a very interesting communication on his last journey in the Tian Shan and to the Pamir, where he visited some places never before visited by European travellers. His researches enable us to correct many imperfections in the works of Gordon and Stoliczka, and to obtain many new and important data. A complete geological sketch of the Pamir highlands will soon be published by M. Mushketov. At the same meeting the secretary gave an account of a new expedition to Central Asia, which will start from St. Petersburg at the beginning of this year, under the leadership of Prof. A. E. Middendorff. The expedition has especially in view the study of the agricultural conditions of Turkistan, and the well-known traveller, zoologist, and practical agriculturist who is at the head of the expedition, will be supported in his work by MM. Smirnoff and Russow.

AT the meeting, December 15, of the St. Petersburg Society of Naturalists Prof. Kessler referred to the fishes brought this year by M. Polyakoff from the lakes Ala-Kul and Balkhash. In addition to the seven species which were known before in the Central-Asian fauna he has discovered four new ones, one of which is the interesting fish described by the inhabitants as *Marenker* (its zoological description will soon appear), the flesh and caviare of which are poisonous.

PROF. BERTHELOT, of Paris, is probably the most prolific chemist of the day. We notice in the two last numbers of the *Annales de Chimie et de Physique*, the two last numbers of the *Comptes Rendus*, and the last *Bulletin de la Société Chimique de Paris*, thirty-two various articles under his name. Berthelot's researches are, however, confined to thermal and physical chemistry, and are not delayed by the analytical operations attendant on other branches of chemical investigation.

CAPT. J. O. LUNGINERS, of the Danish vessel *Lutterfeld*, communicates to a Copenhagen paper an interesting account of a novel experience which occurred on December 10, 1876, while on a voyage to Valparaiso. The vessel was at this time in the neighbourhood of Terra del Fuego, about 140 miles from Magellan's Straits, when early in the morning it narrowly escaped collision with an island where no trace of land appeared on the charts. The vessel hove-to until daylight, when the captain proceeded with a boat's crew to the new island, which had gradually diminished in size since the first observation. Around the conical rocky mass the water was hissing, and although no smoke appeared, it was found to be too highly heated to permit of landing. The sinking continued slowly, until at eight o'clock the island was completely submerged, and an hour later the vessel passed over the spot where it had disappeared.

THE December Session of the Berlin Geographical Society was occupied by a long and interesting address from Dr. F. M. Hildebrandt, on the results of his late African explorations. We have already alluded in a late number to the unfortunate result of the expedition to the snow-clad mountains of equatorial Africa, when the explorer was compelled to return with Mount Kenia fairly in sight. The heroism of Dr. Hildebrandt in battling with danger and disease in manifold forms is only approached by the adroitness and ingenuity which characterised his dealings with the natives. Among the Hataitas he was regarded as a magician, and was forced to pronounce incantations on the unfruitful fields. For this purpose, at his request, specimens of all the plants and animals in the vicinity were gathered by the tribe, and after having served as a "fetish," were carefully packed away in the collections. On another occasion he was attacked by several hundred natives, who beat a hasty retreat, when the explorer advanced towards them armed with a photographic camera. Despite the constant succession of misfortunes accompanying Dr. Hildebrandt during his two years' explorations in Africa, he has succeeded in gathering together a large and valuable collection of anthropological and botanical specimens especially, from Cape Gardafui and the Comoro island Johanna. A number of new species and genera, particularly of aromatic plants, were discovered in the former locality.

THE additions to the Zoological Society's Gardens during the past week include two Lions (*Felis leo*) from Upper Nubia, presented by Mr. John Baird; a Green Monkey (*Cercopithecus callitrichus*) from West Africa, presented by Mr. J. Scott; a Bonnet Monkey (*Macacus radiatus*) from India, presented by Mr. J. H. Thompson; a Common Thicknee (*Œdicnemus crepitans*), European, presented by Mr. F. Möll; a Macaque Monkey (*Macacus cynomolgus*) from India, deposited; a Collared Fruit Bat (*Cynonycteris collaris*), a Geoffroy's Dove (*Peristera geoffroii*), bred in the Gardens.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—An examination for a Radcliffe Travelling Fellowship will be held on February 11. Candidates should forward notice of their intention to offer themselves, on or before January 15, to Dr. Acland.

An examination will be held at Queen's College on April 30 to fill up an open scholarship in natural science, of the value of 90*l*., tenable for five years.

LONDON.—We learn that in consequence of the success attending the course of Lectures on Physiology now being delivered at the Working Men's College, Great Ormond Street, by Mr. T. Dunman, the Council of that Institution have arranged for the delivery, by the same gentleman, of a supplementary advanced course of about sixteen lectures, the first of which will be delivered on Friday, January 18. The lectures will be accompanied by practical demonstrations. Mr. Dunman has been appointed to the lectureship in physiology at the Birkbeck Institution, recently vacated by Dr. Aveling.

MANCHESTER.—Mr. M. M. Pattison Muir, F.R.S.E., Assistant-Lecturer in Chemistry, and Demonstrator in the Laboratory of the Owens College, has been appointed Prælector in Chemistry at Gonville and Caius College, Cambridge.

BERLIN.—We notice in the report of a late session of the Prussian House of Deputies a very vigorous presentation, by Prof. Mommsen and Prof. Virchow, of the necessity for a new building for the royal library. This valuable collection of books—over 700,000 in number—is the largest in Germany, and increases so rapidly that the present quarters in the Imperial Palace are most inadequate. The Government shows an inclination to remedy the evil, and it is to be hoped that this chief store of mental pabulum for the Berlin student will soon be provided with a house of its own, and the much-needed catalogue of its treasures finally be published.

BUDAPESTH.—The committee intrusted with the preparations for the first centennial celebration of this university, have decided to invite all foreign universities to send representatives on the occasion. A work on the history of the university is being prepared by Prof. Theodor Pauler, the late Minister of Education.

HEIDELBERG.—In the recently-issued calendar of the University for the present semester we notice a serious falling off in the attendance, the present number of students (461) being 250 less than that for the past summer. This fact is chiefly due to the increasing custom of the German students to gather in the Universities of the great cities during the winter. The theological faculty includes 19 students, the medical, 79, the philosophical, 180, and the legal, 183. Heidelberg still possesses evidently its traditional attractions for English-speaking students, the catalogue containing the names of twenty-one Englishmen and twenty-six Americans, a large proportion of whom are studying under Bunsen. Of the sixty-seven other foreigners in attendance Switzerland contributes eighteen and Russia nineteen. The corps of professors numbers 105, of whom fifty-nine are in the philosophical faculty. Prof. Bluntschli, the leading authority on international law, is the pro-rector for the present year. We notice that Prof. Blum has been forced by advanced age to give up the chair of mineralogy. His connection with the University dates back to 1828, and under his direction the mineralogical department at Heidelberg has long been one of the favourite resorts of students from various countries, the museum ranking among the best in Europe. Prof. Blum's fame as a mineralogist rests chiefly on his thorough and exhaustive researches on pseudomorphs, the results of which are embodied in his work "Die Pseudomorphosen des Mineralreiches." A very complete and practical text-book of mineralogy, as well as the numerous smaller treatises on subdivisions of the science, which have appeared at intervals from his pen, are regarded as standard works.

STRASSBURG.—The grant of money for the new edifices of the university amounts to 10,500,000 marks (over 500,000*l.*). Of this sum 2¼ millions are contributed from the imperial funds, 5½ millions result from Alsace-Lorraine's share of the new imperial bank notes, and the remainder is contributed by the city, the district, and the two provinces. At present the university is attended by 627 students divided as follows among the faculties;—theological, 49, legal, 156, medical, 117, philosophical, 305. Despite the able corps of professors gathered together since the re-establishment of this historic university, the number of students shows a decrease of eighty as compared with 1876, a result due in a great measure to the coldness exhibited by the old French inhabitants towards the German students.

HOLLAND.—The Netherlands School Museum, at Amsterdam, was opened on December 24, 1877, in presence of Mr. Heemskerk (recently Prime Minister of Holland), and several authorities connected with the Educational Department. Mr. A. van Otterloo, for the committee, in his opening speech alluded to the valuable co-operation of England in the exhibition. The authorities afterwards inspected the museum, and expressed their high appreciation of the interesting collections of school appliances exhibited by the School Board for London and others.

ST. PETERSBURG.—A new High School for ladies is to be opened at St. Petersburg for the special purpose of preparing female teachers for women's colleges. The School is provided with the necessary money by a young lady, and it will be conducted by the professors of the St. Petersburg University.

Prof. Tarkhanoff, of the St. Petersburg Medical Academy, having assisted at the examinations in physiology and anatomy of the thirty-six ladies who have now finished their five years' course at the High School of Medicine at St. Petersburg, publishes a report on those examinations. The answers of the ladies, he says, were definite, clear, and often vivid. Deep and very accurate knowledge was shown in anatomy and histology, the examinations having been made according to the extensive programmes existing in ordinary universities. On the average the answers were quite as good as those of male students; but the answers of three or four ladies, by their completeness and brilliancy, produced a deep impression on the examiners, and greatly exceeded all the professor has ever witnessed either as a student or professor.

CHARKOW.—The annual calendar of this Russian university shows an attendance of 442 students. Over half of this number are freed entirely from the payment of lectures, while a third receive annual stipends varying from 180 to 340 roubles. The corps of instructors numbers sixty-four.

SOCIETIES AND ACADEMIES

LONDON

Royal Astronomical Society, December 14, 1877.—Dr. Huggins, F.R.S., in the chair.—A paper by Dr. Wolf, of Zurich, set forth that the sun-spot period varies from seven to sixteen years, eleven years being the average.—A paper by Mr. C. V. Boys described a new astronomical clock. Mr. Christie and Lord Lindsay criticised it.—A photograph of the sun was presented by M. Janssen. It is one of those taken daily at Meudon, measuring one foot in diameter. Dr. De la Rue said it was the finest example of celestial photography he had ever seen. It was not taken with an equatorial, but an instrument after the fashion of the Kew photoheliograph with a 5½-inch object-glass. The picture was not taken at the principal focus, but in that of a secondary magnifier, corrected independently of visual focus. He pointed out the tornadoes visible on the photograph, and spoke of the importance of a physical observatory to register the changes which occur on a tremendous scale every hour, sun-spots being phenomena of comparatively small importance. Capt. Abney spoke in corroboration, and said that M. Janssen at first thought these photographed tornadoes had an atmospheric origin. Mr. Christie said that similar phenomena had been found on the Greenwich photographs, and they had nothing to do with the collodion.—Mr. Glaisher read a paper on the law of force tending to any point whatever in the plane of motion in order that the orbit may always be a conic.—Mr. Lynn gave a description of Mr. Howlett's drawing of the solar spot of October 31 to November 3, being about 15″ diameter.—Lord Lindsay concluded the description of his spectroscope for nebulæ referred to last month.—Mr. Christie made some remarks and criticised it, and the meeting then adjourned.

Photographic Society, December 11, 1877.—Papers were read by Capt. Abney, R.E., F.R.S., on fog-producing emulsions and their rectification, and by H. B. Berkeley, on emulsions. Capt. Abney showed that the cure, or rather the elimination, of fog in emulsions (as also in dry plates) would be effected by the introduction of either bromide, iodine, or nitric acid into the emulsion. Nitric acid prevented the formation of any chemically-produced sub-bromide of silver, and reduced the fog to the state of bromide; where pure bromide is present, it seems almost impossible that there should be fog. If an emulsion plate is exposed to light, and afterwards partly dipped into copper bromide, then exposed in the camera and developed, the portion treated with the bromide will be found to be free from fog and perfectly clear.

PARIS

Academy of Sciences, December 17, 1877.—M. Peligot in the chair:—The following papers were read:—On the order of appearance of the first vessels in the shoots of some Leguminosæ (third part), by M. Trécul.—Note on the ring of Saturn, by M. Tisserand.—On intramolecular work, by M. Boileau.—On an essential improvement of the navigation lock with mixed oscillation, by M. De Caligny.—M. Cailletet was elected correspondent for the section of mineralogy, in room of the late M. d'Ommalius d'Alloy (obtaining thirty-three votes against nineteen for Mr. James Hales).—Production of crystallised sulphide, selenide, and telluride of silver, and of filiform silver, by M. Margottet. The former are obtained by passing vapours of sulphur, selenium, and tellurium, over silver (heated red) by means of a current of nitrogen.—The silver gets covered with the crystals. The crystallised sulphide is transformed into metallic and filiform silver by a current of dry hydrogen at 440 degrees. The corresponding reduction of the selenide and telluride takes place only at the highest temperatures the glass can bear.—Use of lacs of eosine and fluoresceine for preparation of decorative paintings without poison, by M. Turpin. A potassic or sodic solution of eosine, *e.g.*, treated by an acid, gives a precipitate of eosic acid insoluble in water; this washed till the water begins to take a rose-colour is insoluble in the hydrate of oxide of zinc, and so forms a very rich lac (eosinate of zinc) varying from rose to deep-red, according to the quantity of eosic acid used.—Vine districts attacked by phylloxera (1877), by M. Duclaux. It is noted with reference to L'Aude that the vineyards bordering on the sea (some kilometres in width) are preserved much longer than the others.—The natural enemies of the phylloxera in Germany, by M. Blankenhorn. The small extension of centres of phylloxera there is attributed to the fact that the stocks have been frequented, previous to the phylloxera infection, by natural enemies of the

insect (which are specified).—On the intermediary integrals of the general equation with partial derivatives expressing that the problem of geodesic lines, considered as a problem of mechanics, supposes a rational integral with reference to components of the velocity of the moving body, by M. Lévy.—Calculation of the longitude or the hour of Paris at sea, by occultations of stars, by M. Baills.—On the conditions with limits in the problem of the elastic plates, by M. Boussinesq.—On the equation of Lamé, by M. Brioschi.—On apparatus for projection with polarised light, by M. Laurent. For polarisers the author uses Nicols made of two, three, or four pieces of spar, each piece with two faces cut parallel to each other and the cleavage; then he cements them together with a hard mastic, and operates the whole like a single piece of spar. The analyser used is a Nicol of 22 mm. diameter; it is placed at the principal focus of the projection lens.—Action of oxychloride of carbon on toluene in presence of chloride of ammonium, by MM. Ador and Crafts.—Action of stable anhydrous acids on stable anhydrous bases; explosion of the compound, by MM. Solvay and Lucion.—Anhydrous phosphoric acid and oxide of sodium may exist intimately mixed in fine powder without reacting at the ordinary temperature, but a rise of less than 100° causes instantaneous combination with remarkable violence. M. Lucion sees here no confirmation of the dualistic theory or argument against the unitarian.—On the sensibility of the pericardium in the normal and pathological states, by MM. Bochefontaine and Bourceret. The sound pericardium is sensible; the external face apparently more than the internal. The sensibility can be shown by mechanical excitations. The pericardium inflamed experimentally shows a lively sensibility, at least on the external face and inwards.—Maturation and diseases of the cheese of Cantal, by M. Duclaux. Enough water remains for development of ferments, and there are present lactic acid and albumen.—Observations on the zoological affinities of the genus Phodilus, by M. Milne-Edwards.—On the measurement of the dihedric angles of microscopical crystals, by M. Bertrand.—On the signification of various parts of the vegetable ovule, and on the origin of those of the seed (concluded), by M. Baillon.—Preparation of alcoholic chlorides and their application to the production of colouring matters, by MM. Monnet and Reverdin.

December 24, 1877.—M. Peligot in the chair.—The following papers were read:—On some applications of elliptic functions (continued), by M. Hermite.—On the rotatory power of metastyrolene, by M. Berthelot. Metastyrolene, derived in the cold state from active styrolene, has rotatory power (just as metaterebenthine shares the rotatory power of terebenthine). On the other hand, inactive styrolene, as prepared by the pyrogenic method, gives an inactive metastyrolene.—On Saturn's ring (continued), by M. Tisserand.—Observations on the Bahmié cotton plant, by M. Naudin. This plant, which is found in Egypt, and is very productive, is not, as supposed, a cross between cotton (*Gossypium*) and Gombo (or *Hibiscus esculentus*). It differs from the old race merely in aspect. It is still in process of improvement. The botanical species is the *Gossypium barbadense* of Linnæus and Parlatore, or Sea Island cotton. It requires much heat, and is recommended for Algeria, where the cotton industry has been declining.—Notions concerning intramolecular work (continued), by M. Boileau.—On M. Boiteau's recent communication regarding comparison of the phylloxera of the oak with that of the vine, by M. Balbiani.—On anthogenesic Homoptera, by M. Lichtenstein.—Progress of the phylloxera in the south-west of France, by M. Duclaux.—On the results obtained by use of sulphide of carbon for destruction of phylloxera, by M. Marion. Reiterated treatment with small quantities is recommended.—On the ventilation of the transport-ship *Annamite*, by M. Bertin. After three hours the volume of air evacuated under the sole action of heat from the chimneys was over 29,000 cubic metres, and might rise to 40,000. This movement of air would secure a renewal of air in the hospital about eight times in the hour.—The death of Ruhmkorff was referred to.—M. Dumas, *à propos* of MM. Cailletet and Pictet's almost simultaneous success in liquefaction of oxygen, read a passage from Lavoisier, showing he had anticipated such results.—On the condensation of oxygen and carbonic oxide, by M. Cailletet. His method was to expand the gases suddenly; when cooled to − 29°, and compressed to the extent of 300 atmospheres; a thick mist appears. This is had from oxygen, even at ordinary temperature, if it have had time to lose the heat acquired simply through compression. Hydrogen, under similar treatment, gave no such mist. Nitrogen was not experimented with.—Experiments by M. Pictet on liquefaction of oxygen, by M. de Lognes. The apparatus is described.—M. Dumas opened a sealed letter deposited by M. Cailletet on December 3, announcing his discovery. M. Pictet's results were announced on December 22. Several members expressed opinions on the subject.—New observations on the *rôle* of pressure on chemical phenomena, by M. Berthelot. He calls attention to the fact that the decomposition of chlorate of potash into oxygen and chloride of potassium an exothermic reaction, and not limited by its inverse, is not stopped by a pressure of 320 atmospheres.—On the employment of graphic methods in the prediction of occultations, by M. Tissot.—On the transformations of contact of systems of surfaces, by M. Fouret.—Experimental researches on magnetic rotatory polarisation; magnetic rotations of luminous rays of various wave-lengths, by M. Becquerel. He experimented specially with bichloride of titanium, interposing a spectroscope between the eye and the analyser in his former apparatus; and he notes some differences between the positive and negative magnetic rotations.—Ordinary and extraordinary indices of refraction of quartz for rays of different wave-lengths as far as the extreme violet, by M. Sarasin. The numerical results for lines of cadmium, sodium, zinc, and aluminium are tabulated.—Engraving on glass by electricity, by M. Planté. The surface of a plate of glass or crystal is covered with a concentrated solution of nitrate of potash (poured on it). A horizontal platinum wire, connected with one of the poles of a secondary battery of fifty to sixty elements, is placed in the liquid along the edges, then holding in the hand the other platinum electrode, covered, except at the end, with insulating matter, one touches the glass with it, and draws characters, &c., which remain distinctly engraven.—On acid acetates (continued), by M. Villiers.—On experiments showing that meningo-encephalitis of the convexity of the brain produces different symptoms, according to the points of this region that are affected, by MM. Bochefontaine and Viel.—On the conditions of development of Ligulæ, by M. Duchamp. He made two pigeons (which are pretty far removed from aquatic birds), swallow some ligulæ from a tench. After four and five days respectively they were killed, and each had in its intestine a living ligula with genital organs developed, and the matrices full of eggs (just as with the duck).—On a miocene alios in the neighbourhood of Rambouillet, by M. Meunier. This points to sudden cataclysm.—On thermal coloured rings by M. Decharme. He remarks on the difference in these on tinned and on zinked iron plates.

VIENNA

Imperial Academy of Sciences, November 8, 1877.—On the least absolute number of sound-impulses that are necessary to production of a tone, by M. Pfaundler.—On generalisation of known triangle propositions to any perfect *n* angles inscribed in a conic section, by M. Cantor.—On the perfect square in general, by the same.—On citramalic acid, by M. Morawski.—On accessory projections in the skull of leporides, by M. Mojsisovics.—On arbitrary and spasmodic movements, by M. Brücke.—On cork and corked tissues generally, by M. Höhnel.

CONTENTS

THURSDAY, JANUARY 10, 1878

THE SALARIES OF THE OFFICERS IN THE BRITISH MUSEUM

THE inadequacy of the salaries of the officers of the British Museum has long been a standing grievance. It is manifestly impossible to give any valid reasons why the literary and scientific men of this great national establishment should not receive emoluments at least equal to those granted in the ordinary branches of the Civil Service. The obstinacy of the trustees in clinging to obsolete principles of priority, and in endeavouring to keep entirely in their own hands the right of nomination to all the more important posts, has, no doubt, been the main cause why the Treasury have until recently refused to do justice to a most meritorious and ill-treated branch of the public service. From the "Correspondence between the Trustees of the British Museum and the Treasury," which has lately been issued as a Parliamentary Paper, we are glad to find that in this instance, as on former occasions, the present Ministry has been induced to do justice where their predecessors in office have persistently ignored righteous claims. After a long correspondence, commenced in May, 1876, and extending over some fifteen months, it seems to have been finally settled that the salaries of the keepers of the various departments shall be raised to 750*l.* per annum after five years' service, instead of stopping at 600*l.*, the former limit, and that the salaries of the assistant-keepers shall rise to 600*l.* after five years' service, instead of being restricted to 450*l.* as heretofore. The assistants in the various departments will, in future, be divided into two classes, the first, or upper class, with salaries commencing at 250*l.* per annum, and rising by annual increments of 15*l.* to 450*l.*; those of the second, or lower class, commencing at 120*l.*, and rising by increments of 10*l.* to 240*l.* This will create a considerable general improvement in the position of these subordinates, of whom the junior assistants, as they are called, have hitherto commenced at 90*l.*, and the senior assistants have never risen beyond 400*l.* But the trustees have agreed to regard the new second class for the future as an "educational class," from which those persons who show special aptitude for the work of the different departments may be promoted to the first class, whilst those who have no extraordinary abilities must remain content with the maximum salary of the lower class. Another concession that the trustees have been compelled to make in order to obtain the above-mentioned advantages is a reduction in the number of the assistants of the upper class. The Treasury justly point out to the trustees that the scheme of having a first class of assistants double the number of that of the second class, is "inconsistent with all ordinary classification," and that the comparative numbers of the two classes "ought to be exactly reversed." This the trustees have, as it appears, somewhat unwillingly undertaken to effect, by a gradual reduction of the number of first-class assistants as vacancies occur, and by making all future appointments into the second class, except when "an opportunity occurs of securing the services of a person possessing very special qualifications."

A third point which the trustees "are prepared to reconsider" is the number of keeperships, now amounting to thirteen, and in order that the Treasury may have greater control in this matter, they have undertaken not to fill up any keepership which may hereafter become vacant, "without the previous concurrence of the Treasury." A still more important proposal made by the Treasury and "conceded by the trustees," is that the position of keeper should be considered as a "staff appointment, to which no officer within the Museum should have any right of succession by seniority." This "concession" will, we trust, do away with the practice of putting round men into square holes, in order to obtain for them an additional salary, which in former years has, we fear, been followed in some instances at the British Museum.

One remaining point, which has much exercised the well-known economy of the Secretary of the Treasury, we are pleased to see he has been obliged to give up. It was proposed that the keepers who occupy the residences attached to the British Museum ought to give up a certain portion of their salaries in lieu of rent. In reply to this ingenious suggestion, the trustees very justly urge that those keepers who reside on the premises have important duties to perform, in having to take in turn the general charge of the whole museum under the principal librarian, for which the accommodation of a residence is no more than a fair equivalent. This contention was ultimately allowed to prevail, and on the whole, we think, there is every reason to be grateful to the Government for the improvements effected by the new scheme in the position of the *employés* at the British Museum. Even in these hard times it cannot be said that a place of 750*l.* per annum with a good residence attached and a pension in future when work is no longer possible, is not such a prospect as may well attract some of the cleverest youths of the period who have a leaning towards literature or science to seek the place of "junior assistant" in the British Museum.

JULES VERNE

Hector Servadac, or the Career of a Comet.—From the Earth to the Moon.—Around the Moon.—Twenty Thousand Leagues under the Sea.—Around the World in Eighty Days.—The Fur Country.—A Winter amid the Ice, &c., &c. (London: Sampson Low and Co.)

THESE remarkable works, which we owe to the genius of Jules Verne, the first-named being that which has last appeared, are well deserving of notice at our hands, for in the author we have a science teacher of a new kind. He has forsaken the beaten track, *bien entendu*; but acknowledging in him a travelled Frenchman with a keen eye and vivid imagination—and no slight knowledge of the elements of science—we do not see how he could have more usefully employed his talents. He will at once forgive us for saying that when we compare his romances of the ordinary type, such as "Martin Paz," with those we have given above, we think that he, as well as his readers, is to be congratulated upon the new line he has opened out.

There have been many books before his time in which the interest has centred in some vast convulsion of nature, or in nature generally being put out of joint, but in these

there has been no attempt made to reach the *vraisemblable;* indeed in most cases there has not been sufficient knowledge on the part of the author to connect his catastrophe either with any law or the breaking of one. But with Jules Verne for once grant the possibility of his chief incident, and all the surroundings are *secundem artem.* The time at which the projectile was to be shot out of the columbiad towards the moon was correctly fixed on true astronomical grounds, and the boy who follows its flight will have a more concrete idea of, and interest in, what gravity is and does, possibly, than if he were to read half-a-dozen text-books in the ordinary way. Once grant the submarine vessel and the use made of electricity, and the various scenes through which the strange ship passes are sketched by no 'prentice hand. To take the most extreme case, if it be possible to imagine one in such a connection—Algeria torn from the earth by a comet and started on an orbit of its own; the astronomical phenomena have been most carefully thought out, and children of larger growth will, if they choose, find much to learn as well as to amuse them. Indeed it is very rare that one finds our author tripping in such matters, although he does sometimes. One case that occurs to us is when, in the "Fur Country," he refers to the midnight sun touching the edges of the *western* horizon without dipping beneath it; and even this may be due to the translator, for we have not the original French edition to refer to.

Thus much premised let us see how, in "Hector Servadac," his last work, the author attempts, as part of his task, to inculcate scientific truths, remarking that his plot is carefully kept out of view till the end of the volume. He and his faithful servant are stunned by a crash, in which the earth groaned as if the whole framework of the globe were ruptured, while the sea and air became one, and both glowed in a radiance intenser than the effulgence of the northern lights. In the midst of a gigantic earthquake-wave he found the moon's disc becoming much larger than it was before, and a new blazing star appearing suddenly in the firmament. Strange to say watches, which are not stopped, mark two as the sun rises in the west! Next point. Their respiration became more forced and rapid, like that of a mountaineer when he has reached an altitude where the pressure of the circumambient air has become reduced; when they jump they fly. The horizon is contracted. There are more surprises: a strange body (the retreating earth) seems to contend in splendour with the sun; but the true condition of affairs has not revealed itself yet, for he is anxious to go and look for his fellow-men; *en attendant,* however, they must eat.

"'By jingo!' he exclaimed, 'this is a precious hot fire.' Servadac reflected. In a few minutes he said:—

"'It cannot be that the fire is hotter, the peculiarity must be in the water.'

"And, taking down a centigrade thermometer which he had hung upon the wall, he plunged it into the skillet. Instead of 100°, he found that the instrument registered only 66°.

"'Take my advice, Ben Zoof,' he said; 'leave your eggs in the saucepan a good quarter of an hour.'"

So much for the careful treatment of the first forty pages. At last the truth dawns upon Hector, and he finds others on the newly torn-off fragment, including even the very astronomer who predicted the comet. A new point in favour of the metric system is here introduced; for our astronomer, anxious to determine the density and mass of *Gallia,* as the fragment had now been named (this is more pardonable than Gallium), finds that not only the metre of the archives, but all other measures whatever had disappeared. He shows that—

10 5-franc pieces	37 mm.	in diameter
10 2-franc ,,	27 mm.	,,
20 50-cent. ,,	18 mm.	,,

exactly make a metre.

A German Jew (M. Verne has his ideas of the different nationalities) is made to lend this sum at an enormous rate of interest, and the experiment proceeds.

"By the appointed time the engineer had finished his task, and with all due care had prepared a cubic decimetre of the material of the comet.

"'Now, gentlemen,' said the Prof. Rosette, 'we are in a position to complete our calculations; we can now arrive at Gallia's attraction, density, and mass.'

"Every one gave him their complete attention.

"'Before I proceed,' he resumed, 'I must recall to your minds Newton's general law, "that the attraction of two bodies is directly proportional to the product of their masses, and inversely proportional to the square of their distances."'

"'Yes, then,' continued the professor, 'keep——"

"'Yes,' said Servadac; 'we remember that.'

"'Well, then,' continued the professor, 'keep it in mind for a few minutes now. Look here! In this bag are forty five-franc pieces—altogether they weigh exactly a kilogramme, by which I mean that if we were on the earth, and I were to hang the bag on the hook of the steel yard, the indicator on the dial would register one kilogramme; this is clear enough, I suppose?'

"As he spoke the professor designedly kept his eyes fixed upon Ben Zoof. He was avowedly following the example of Arago, who was accustomed always in lecturing to watch the countenance of the least intelligent of his audience, and when he felt he had made his meaning clear to him, he concluded that he must have succeeded with all the rest. In this case, however, it was technical ignorance, rather than any lack of intelligence, that justified the selection of the object of this special attention.

"Satisfied with his scrutiny of Ben Zoof's face, the professor went on:—

"'And now, gentlemen, we have to see what these coins weigh here upon Gallia.'

"He suspended the money bag to the hook; the needle oscillated, and stopped.

"'Read it off!' he said.

"The weight registered was one hundred and thirty-three grammes.

"'There, gentlemen, one hundred and thirty-three grammes! Less than one-seventh of a kilogramme! You see, consequently, that the force of gravity here on Gallia is not one-seventh of what it is upon the earth!'

"'Interesting!' cried Servadac, 'most interesting. But let us go on and compute the mass.'

"'No, captain, the density first,' said Rosette.

"'Certainly,' said the lieutenant; 'for, as we already know the volume, we can determine the mass as soon as we have ascertained the density.'

"The professor took up the cube of rock.

"'You know what this is,' he went on to say. 'You know, gentlemen, that the block is a cube hewn from the substance of which everywhere, all throughout your

[1] "On this subject an amusing anecdote is related by the illustrious astronomer himself. One day, just after he had been alluding to this, as his usual habit, a young man entered the room, and feeling sure the lecturer knew him well, saluted him accordingly. 'I regret I have not the pleasure of your acquaintance,' said M. Arago. 'You surprise me,' replied the young student. 'Not only am I most regular in my attendance at your lectures, but you never take your eyes off me from the beginning to the end.'"

voyage of circumnavigation, you found Gallia to be composed—a substance to which your geological attainments did not suffice to assign a name.'

"The professor took the cube, and, on attaching it to the hook of the steel yard, found that its apparent weight was one kilogramme, and four hundred and thirty grammes.

"'Here it is, gentlemen; one kilogramme, four hundred and thirty grammes. Multiply by seven; the product is, as nearly as possible, ten kilogrammes. What, therefore, is our conclusion? Why, that the density of Gallia is just about double the density of the earth; which we know is only five kilogrammes to a cubic decimetre. Had it not been for this greater density, the attraction of Gallia would only have been one-fifteenth instead of one-seventh of the terrestrial attraction.'

"The professor could not refrain from exhibiting his gratification that, however inferior in volume, in density, at least, his comet had the advantage over the earth."

We have given this long extract to show the pleasant way in which, in this latest form of French light literature, amusement is combined with instruction. It would not be fair to the book to say more of the plot or of the *dénouement.*

We have dwelt especially upon Jules Verne's latest book, but equal praise must be given him for all those we have named. A boy, for instance, who had read how the frozen island in the "Fur Country" was kept together by Dr. Black's device, would at once understand the *rationale* of Pictet's and Cailletet's recent splendid work, to say nothing of the physical geography he would have gradually absorbed in following the strange adventures recounted in that volume.

We are glad to have such books to recommend for boys' and girls' reading. Many young people, we are sure, will be set thirsting for more solid information.

OUR BOOK SHELF

The Geometry of Compasses; or, Problems Resolved by the mere Description of Circles, and "the Use of Coloured Diagrams and Symbols." By Oliver Byrne. (London: Crosby Lockwood and Co., 1877.)

THIS is only our old friend, "La Geometria del Compasso di Lorenzo Mascheroni" (Paris, 1797), decked out in the manner we have indicated in the quoted portion of the title. The order of sequence has been departed from, but this is not a material point. The constructions are the same and the proofs the same with, we believe, one exception, in which case we give the preference for simplicity to Mr. Byrne.

There are twenty problems, which are in most cases given in duplicate, first construction and figure in colours, then proof and unadorned figure on the next two pages.

The merits and nature of Mascheroni's work are well known; hence the present work, for reasons given above, is good. But we cannot call this Mr. Byrne's book. Problem XX., which is the last, is an elegant construction for dividing the circumference into seven equal parts by plane geometry. But for this the compiler is indebted to an able mathematician, Dr. Matthew Collins. The book is very neatly and correctly got up, and for frontispiece has a hand with a pair of compasses transferring a given length.

Proceedings of the American Philosophical Society. Vol. xvi., No. 99. January to May, 1877.

PROF. COPE has several noteworthy papers in this part: one, on the Batrachia of the coal-measures of Ohio, describes the new genus, Ichthycanthus, and the new species of Leptophractus and Tuditanus. He also describes remains of a Dinosaurian from the trias of Utah; the humerus is one of the longest, and distally the most contracted known in the group. These remains are the first discovered fossils in the triassic beds of the Rocky Mountain regions. Another valuable paper is on the brain of Coryphodon. One of the longest contributions will be much esteemed by geologists, viz., Mr. Ashburner's measured section of the palæozoic rocks of Central Pennsylvania (Huntingdon County), a section extending vertically through 18,394 feet. A very valuable series of physiological experiments is recorded in a paper by F. L. Haynes, on the asserted antagonism between nicotin and strychnia. Philology is well represented by a paper on the Timucua language, by Mr. A. S. Gatschet; this language, formerly spoken in Florida, appears to be the oldest within the American Union of which writings of some extent are preserved.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

The Radiometer and its Lessons

WITH reference to the controversy between Mr. Stoney and Mr. Osborne Reynolds about the laws of the conduction of heat in gases, it seems desirable to call the latter gentleman's attention to the fact that neither Clausius' nor Clerk Maxwell's investigations, as published in the *Philosophical Magazine*, affect the controversy between them.

The latter, in his papers in the *Philosophical Magazine*, vol. xxxv., lays particular stress upon the fact that he supposes the motions of the molecules to be uniformly distributed in every direction. He says, however, on page 188: "When one gas is diffusing into another, *or when heat is being conducted through a gas*, the distribution of velocities will be different in the positive and negative directions instead of being symmetrical, as in the case we have considered." From this theory of the uniform distribution of velocities he deduces the formula (29), (31), and (32), as he numbers them, and to which he subsequently refers. On page 214 he gets an equation (143) which represents the transference of heat through the medium, and says: "The second term contains quantities of four dimensions in $\xi\,\eta\,\zeta$, *whose value will depend upon the distribution of velocity* among the molecules. *If* the distribution of velocity is that which we have proved to exist when the system has no external forces acting on it, and when it has arrived at its final state we shall have by equations (29), (31), (32) . . ." certain results from which he deduces his equation for the conduction of heat in gases.

When he says "has arrived at its final state" it is evident from his reference to the equations that he means the state of a gas in which neither diffusion nor conduction of heat nor currents of any kind are going on. It will thus be seen that his final result is only a first approximation and could not possibly be expected to hold within distances comparable with the mean length of the path of a molecule between two encounters.

Clausius in his paper as translated in the *Philosophical Magazine* for June, 1862, does suppose a distribution of velocity among the molecules of such a kind that the velocity and number of molecules moving in the positive and negative directions is different, but assumes the mean between them to be the same as the number moving in a direction normal to the direction of the transference of heat. This is evident from the fact that what he practically does is to assume that the number of molecules moving in a direction making an angle θ with the direction of transference of heat can be expressed by a formula of the form—

$$n = n_0 (1 + e \cos \theta),$$

for he neglects e^2 throughout his investigation. In this form it is evident that n_0 is the number when $\theta = \frac{\pi}{2}$ and is the mean of the values $n_1 = n_0 (1 + e)$ and $n_2 = n_0 (1 - e)$, which represent the numbers going towards and from the points of high tempera-

ture. From the fact that c^2 enters into the investigation at all it is evident that this is only an approximation to the true distribution. In accordance with this Mr. Stoney has shown conclusively that in a compressed Crookes's layer the number of molecules moving parallel to the direction of the transference of heat is greater than the number of those moving in any direction normal to it, so that the expression Clausius derived from his assumption cannot be considered as expressing the whole state of affairs.

It is remarkable that to this order the expression for the pressure on *any* plane is the same, but Clausius gives another term in his expression for the pressure on a plane normal to the direction of transference of heat to which he attaches, indeed, only an indefinite coefficient because it is of the order c^2, and he was purposely neglecting quantities of that order. He might have prophesied, however, from the existence of such a term that at distances comparable with c a force would be manifested such as Mr. Crookes has since discovered. Now this c is by definition a quantity of the order of the length of the mean path between successive encounters, and hence these terms, varying with c^2 would become of importance at distances comparable with the length of this mean path.

I believe, then, that I have shown that neither Clausius nor Clerk Maxwell have considered the case in dispute between Mr. Stoney and Mr. Osborne Reynolds, and that as far as their investigations bear upon it they tend very much to strengthen Mr. Stoney's case. I have also shown that Clausius was on the point of anticipating both Crookes's force and Mr. Stoney's explanation of it. GEO. FRAS. FITZGERALD

Trinity College, Dublin

Prof. Eimer on the Nervous System of Medusæ

SOME of your readers may remember that a few months ago I published in NATURE an abstract of a lecture which I had delivered at the Royal Institution on "The Evolution of Nerves" In this lecture I mainly treated of my recent researches on the nervous system of *Medusæ;* and stated, among other things, that I was the first to publish the observation concerning the paralysing effect of removing the margins of nect calyces.[1] Within the last few days, however, I have received a communication from Prof. Eimer, of Tübingen, informing me that he has the right to claim priority as regards the publishing of this observation. I therefore send you this note in order that I may rectify the injustice which I previously did to Dr. Eimer in your columns.

The facts of the case are simply these: Dr. Eimer made his observation a few months later than I made mine; but, as he communicated his observation within a few weeks after he had made it to the *Physikalisch-medicinischen Gesell. chaft zu Würzburg*, his publication preceded mine. He has therefore the right to claim priority as regards this observation, and also as regards some further physiological experiments by which he followed it up—all of which I have been careful to detail in my Royal Society publications.

So much in justice to Dr. Eimer. In justice to myself I must now explain that, although, since the publication of my Croonian lecture in 1875, I have been aware that Dr. Eimer's work was independent of mine, it is only within the last few days I have learned from him that the publication of his work was prior to mine. The reason of the ambiguity on this head is explained in a newly-published memoir by Dr. Eimer, where it is stated that his previous memoir, having been published in the *Würzburg Verhandlungen* without its proper title-page, the initials "*d. J.*" ("this year"), which occur in the paper itself, refer, not to the date on the volume, but to the year preceding. My prolonged ignorance concerning Dr. Eimer's claim to priority, has, therefore, not been due to any fault on my part; and as in all my previous publications on this subject I have spoken of Dr. Eimer's work as subsequent to my own, I may here add that I think the fact of his having been so long in acquainting me with the true standing of the case, displays a laudable spirit of indifference on his part to the matter of mere priority.

GEORGE J. ROMANES

18, Cornwall Terrace, Regent's Park, N.W.

Mr. Crookes and Eva Fay

AFTER Mr. Cooper's courteous explanation which appeared in last week's NATURE (p. 183), I gladly exonerate him from blame.

To the publication of my letter in the *Banner of Light*, if Mr. Cooper thought it likely to do Eva Fay any good, I have no ground of complaint; but what I did, and do now, protest against, is the unauthorised publication of a lithographed *facsimile* of my letter in such a manner, and with such surroundings, as to leave no doubt that the intention was to throw discredit on my testimony as a trustworthy experimentalist.

I am glad to find that Mr. Cooper was no party to this breach of etiquette, and I willingly withdraw any expressions in my letter in NATURE (vol. xviii. p. 43) which may appear to reflect on him.

As a fitting climax to this controversy, may I request you to publish the subjoined letter from Eva Fay, which appeared in the *Banner of Light* for December 22 last?

London, January 7 WILLIAM CROOKES

"To the Editor of the *Banner of Light*, Boston, U.S., December 22

"I WISH to state a few facts in reference to an article in your paper of December 8, referring to myself, in a letter of Mr. Crookes on Dr. Carpenter's attack.

"First, it is untrue that Mr. Crookes gave me a letter speaking of the spiritualistic nature of my manifestations, and referring to Fellows of the Royal Society. The only letter, to my knowledge, that Mr. Crookes ever wrote regarding my mediumship (with the exception of the one written to Mr. Cooper) appeared in the London *Daily Telegraph*, and other journals, March 11, 1875.

"Second, in reply to Dr. Carpenter's statement that an offer was made by my managers in May, 1875, of an equivalent sum of money for me to 'expose the whole affair,' I will now say to Dr. Carpenter, as I did to my managers, *I have nothing to expose.*

"I am in receipt of a letter, dated November 18, 1877, asking me if I will fix a price to visit England under the title of an 'Exposée,' and show how I am supposed to have hoodwinked members of the Royal Society.

"My reply was as follows:—'As poor as I am, and as clever as I am supposed to be by Dr. Carpenter and others, I am obliged to decline your tempting proposition to replenish my exchequer by attempting impossibilities. I sincerely hope to be able to maintain myself and child in a more honourable occupation.' "ANNIE EVA FAY

"Akron, Ohio, December 10, 1877"

Volcanic Phenomena in Borneo

MR. WALLACE, in his work on the "Geographical Distribution of Animals," has the remark that no volcano, active *or extinct*, is known to exist within the area of the island of Borneo, notwithstanding that it is almost environed by a volcanic belt in full activity at a short distance. In fact, it seems to be generally understood that this vast island now represents, and has continued to represent for long past time, a perfectly quiescent area in so far as manifestations of subterranean energies are concerned. This view is doubtless strictly correct in regard to the existence of any volcanic vent which is now in action, or which has been so within the historical period; but it would be erroneous to deduce from it, as seems natural to do at first sight, the inference that the area is one of entire quiescence, or that it has been so free from volcanic action in any but the most recent times.

Speaking solely with reference to the north-west district, it may be observed that shocks of earthquake have been recorded more than once by credible witnesses during late years, viz., one in June, 1874, a second in June, 1876, and two more in July, 1876. These were recorded the first in Sadong, the three others in Saràwak. According to native testimony, slight shocks are by no means rare, and a severe one is particularly held in remembrance, which took place seventy or eighty years ago, and was accompanied by "a rain of ashes." Seismometrical observation would probably show that they are very frequent. These shocks seem to indicate that the island is directly affected by the proximity of the volcanic band above referred to.

As for the period of time preceding the historical epoch, there are not wanting signs that this part of Borneo was the theatre of a display of considerable volcanic energy, and it has yet to be shown that its date of activity was anterior to the deposition of the sandstone conglomerate formation of the country, which is, with the exception of very recent deposits, the most modern of the stratified rocks of this part of the island, it having been assigned—I know not with how much truth—to a later tertiary

[1] I first published this observation in a note to NATURE, which appeared in November, 1874.

date on the evidence of the plant-remains found in the coal-seams which are associated with it.

Owing to the difficulty of determining questions of relative superposition in a country so densely clothed with vegetation, and to the insignificant depth of the sections, natural and artificial, which are accessible, coupled with the remarkably disturbed condition of large tracts of the sedimentary rocks—it is not possible to define at present the relations of the igneous to the sedimentary rocks of the district. Nevertheless, such evidence as I have myself been able to collect goes to support the hypothesis that the last outbreak of volcanic activity was posterior in date to all but the more modern deposits of shales, clays, river-gravels, &c., or, in other words, that it preceded more or less immediately, the last submergence of north-west Borneo—though separated from that submergence by a long interval, and possibly being the concomitant of an antecedent elevation of the land.

The traces of this outbreak remain in the existence of thermal springs, two at least of which occur in association with hills of trappean and basaltic rocks; the country in many parts is dotted with hills of basalt, columnar basalt, and felspathic porphyries, and in the intervening lowlands is seamed with dykes of porphyritic, hornblendic, and siliceous rocks; the sedimentary strata are greatly disturbed when the igneous rocks occur, being often upheaved at high angles and much plicated, and locally the sandstones and shales have been metamorphosed; whilst masses of a volcanic-conglomerate (?) are occasionally met with.

Philippine Islands, September 27 A. H. EVERETT

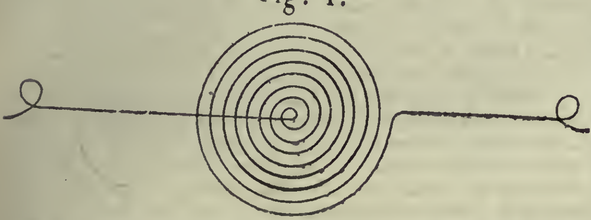
New Form of Telephone

HAVING had the pleasure of listening to Mr. Preece and Prof. Graham Bell explaining that most wonderful invention, the telephone, at the late meeting of the British Association in Plymouth, I endeavoured to obtain the instrument for my own use, and was ultimately successful.

It soon struck me that if the disc or diaphragm whose vibration causes the induced current in the coil of copper wire must be a magnetic substance, and not simply a conductor, then if I could succeed in getting an electro-magnet to vibrate in a similar manner it might be possible to get as powerful a sound.

With this object in view a coil of insulated copper wire was fastened to a card, as shown in Fig. 1.

Fig. 1.



The wire used was No. 28 cotton-covered, and it was sewed to the card with thread.

The iron disc was taken out of one of the telephones, and the coil-diaphragm put in its place through which a current was passed from a single Bunsen cell. On making connection with the other telephone, talking, singing, and whistling were heard distinctly at both.

Various coils have since been tried both with thicker and also thinner wire, but as yet the results have not been as good as when the iron disc is used.

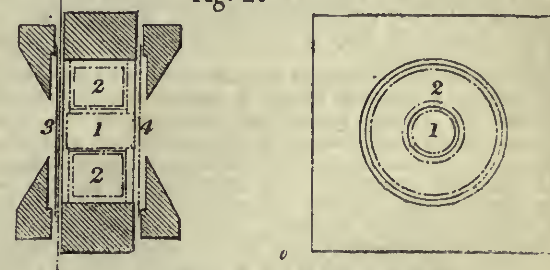
When two such coils are used, one superposed on the other, the loudness of the sound transmitted is increased to some extent. The same result is produced by adding another Bunsen cell. With a Daniell's cell the sound is very feeble. When a coil is placed in each telephone the result is rather unsatisfactory as yet.

It has also been noticed that a simple conductor as a coil of copper wire also transmits sound but very faintly.

A small apparatus (Fig. 2) has been made to show the effects above described. A piece of wood about three inches square and about one inch thick has a hole bored through it about two inches in diameter. A reel (2) containing about 250 yards of silk-covered copper wire is placed in the hole with a piece of soft iron about half an inch in diameter as axis (1).

A coil-diaphragm (3) is placed across one end of the hole at a very short distance from the soft iron core, and is covered by a mouth-piece. Across the other end of the hole at a similar distance from the core is placed a thin iron disc (4) which is also covered by a mouth-piece. On a current being passed through the coil-diaphragm this apparatus acts as a telephone, and messages can be sent from either side of it.

Fig. 2.



1. Iron core. 2. Coil of wire. 3. Coil diaphragm. 4. Iron disc.

The iron disc and core may be removed, and the coil-diaphragm alone acts in an exactly similar manner.

The above are the results of some experiments which have occupied my spare time lately, and not having seen anything similar published I forward them to you as they are rather interesting. The whole of the experiments have been conducted with the aid of my friend, Mr. G. B. Nicoll, who has also made many important suggestions. JAMES M. ROMANIS

Shooting Stars

THE number of shooting stars seen here during the last six months (July to December) is 2,259 in 168 hours of watching. This number includes 385 Perseids observed between August 3 and 16. Of the remaining 1,874 1,028 were seen during seventy-five hours in the mornings and 846 during ninety-three hours in the evenings. After making certain allowances for time spent in registering the paths (and omitting the Perseids), the horary numbers appear to have been as follows:—

16·4 A.M., 10·9 P.M., 13·4 A.M. and P.M.

From these figures I estimate that the aggregate number of shooting-stars as bright as, or brighter than, 5th mags., which entered the earth's atmosphere in this particular part of the world by night and day during the last six months, was about 236,700. The horary number has already been mentioned as 13·4 for one observer. Now a single pair of eyes certainly cannot command more than a fourth part of the visible sky, so that we must adopt 53·6 as the horary rate over the whole sky. From this we readily deduce the diurnal number as 1286·4, and the aggregate for the six months, 236,697·6 as above.

When it is further considered that the average height of ordinary shooting-stars is only about seventy miles, and that therefore observers at widely distant stations must each see a distinct set altogether, we are able to form some remote idea of the vast number that enter our atmosphere every day.

Bristol, December 26, 1877 W. F. DENNING

Gentiana asclepiadea and Bees

THIS gentian is very abundant on the mountain slopes round Engelberg, as visitors to that part o Switzerland well know. As I was botanising in the neighbourhood, in the autumn of this year, I observed that most of the flowers were pierced with a round hole at the base. Presently I saw a bee come to one of the pierced flowers, and thrust in its proboscis in search of honey. The flowers of this beautiful, sweet-smelling gentian are long and funnel-shaped, and very contracted at the base, and, as the bee that visited it was a "fair large" one, like Sir Torre's diamond, and not of the narrow hive-bee type, it could not possibly have effected its purpose by entering the flower in the usual way at the top, and had no doubt resorted to this method of extracting the honey. I only saw this one kind of bee visit the flowers, but I saw many of them at work, and all acted in the same way. One of them came to some of the flowers, which I had gathered, as I held them in my hand. I cannot say that I saw a single flower actually pierced by a bee; the day was warm, even for Engelberg, and the bees were very

quick in their movements, which increased the difficulty of observation, but that the bees themselves were the agents, in making the holes, there can be no reason to doubt.

Highfield, Gainsborough, December 21 F. M. BURTON

Photography Foreshadowed

THE first prophetic allusion to the photographic art, the discovery of which was to take place eighteen centuries later, is perhaps found in the story of the miraculous occurrence told in the life of St. Veronica.

The second instance is about the year 1690; but intermediate instances may probably be found. I extract from the works of Fénelon [1] the following passage from an imaginary voyage in 1690:—"There was no painter in the country, but when anyone wished to have the portrait of a friend, a beautiful landscape, or a *tableau* which represented any other object, he put water in large basins of gold or silver; then placed this water opposite the object he wished to paint. Soon the water congealing became like a looking-glass, in which the image of that object remained ineffaceable; and it was a picture as faithful as the brightest mirrors." One could wish that the author had entered into detail as to the manner "of placing this water opposite the objects he wished to paint."

The third instance is about 1760, that is seventy years later, and seventy-nine years before 1839, the date of Daguerre's discovery. It is reported [2] by Ed. Fournier, who extracted from what he calls "un assez mauvais livre," written by a certain Tiphaigne de la Roche [3], the entire passage, extremely curious, but rather long. This passage contains many details. The "water" of Fénelon is replaced by "a material very subtle, very viscous, and very ready to dry and harden." "They" (certain "elementary spirits") coat with this material a piece of linen, and present it to the objects which they wish to paint," &c.

In the two last examples the pictures formed reproduce the images of the objects, with their natural colours and their forms, so that the objects are seen as if reflected in a mirror. The photographs of the present day are still very far from this ideal perfection, which, however, they will probably never cease to approach without ever being able to reach.

Rotterdam J. A. GROSHANS

[1] Paris, Auguste Desrez, 1837, tome 2^me, p. 643.

[2] Le Vieux-Neuf, Histoire Ancienne des Inventions et Découvertes Modernes Paris, Dentu, 1859.

[3] Giphantie, à Babylone, MDCCLX., 12°.

Average Annual Temperature at Earth's Surface

HAVING lived for many years both in the southern and northern hemispheres, I have a very strong impression that if means were taken to ascertain, with more or less approximation, the average annual temperature at the earth's surface, by a combination of the daily averages of a sufficient number of places of observation, there would be found a very considerable difference in the yearly values of the said average annual temperatures. But whether, on inquiry, there should prove to be a decided difference or an absolute agreement between these averages, the fact in either case would surely be worth ascertaining, and could not fail to be instructive. It might be objected that it would be impossible to obtain the observations of the daily average temperature from such a number of observatories as would render the desired annual average for the whole earth of any value, but I think this objection overstates the difficulty. Suppose that the subject were taken up by some one of the meteorological authorities in Great Britain, it would not be difficult to obtain from existing daily records, a good average annual value for the temperature of the British Islands. Similarly, an average annual value could be obtained for the temperature, from the daily averages in the various colonies and dependencies of the British Empire; and I take it to be certain, that the conductors of the various meteorological observatories all over the empire would cheerfully respond to an invitation to co-operate in such a work. In a similar scientific spirit it is to be hoped that the observatories of all civilised countries would be willing to exchange their observations, and an approximate result could thus be arrived at, possibly in two or three years. Certainly, it might be at first a rough approximation only, but it would be yearly becoming better with the rapid increase of meteorological observatories all over the world. And as it is not too much to hope that, sooner or later, the whole habitable earth will be civilised and covered with observatories, it is certain that the figures ultimately obtained to represent the average annual temperatures at the earth's surface would have the value of scientific approximations of considerable accuracy. If this be so, it cannot be too early to begin these statistics now.

Supposing that these annual averages should exhibit a difference in their yearly values, it is probable that these differences would vary in sympathy with the total sun-spot areas of the years to which they belonged. What could be done for temperature, could be done at the same time for other subjects of meteorological investigation, and it is impossible to anticipate at present what light these tabulated annual averages might be able to throw upon various problems of solar and terrestrial physics.

Balham, December 4 D. TRAILL

ON A MEANS FOR CONVERTING THE HEAT-MOTION POSSESSED BY MATTER AT NORMAL TEMPERATURE INTO WORK

IN a previous article [1] we considered how, by a simple mechanical means, diffusion renders it possible to derive work from matter at normal temperature. As the subject is an important one we propose to develop it somewhat further here.

2. The normal temperature of objects on the earth's surface represents a vast store of energy in the form of molecular motion. The sea (for example) at normal temperature possesses an amount of molecular energy which (by computation), if it were entirely utilised, would be competent to lift it to a height of upwards of seventy miles. The air and the crust of the earth itself possess comparable amounts of energy. It might therefore well be asked beforehand whether it is not possible to transfer some of this intense molecular motion to masses and utilise it. It may be observed that this intense store of energy is being continually dissipated in space in the form of waves (by radiation). The energy possessed by the molecules of matter, however, maintains (as is known) a constant normal value on account of the waves of heat received from the sun, whose mechanical value at the earth's surface (as represented by the results of Herschel and Pouillet) is normally equal to about one-horse power per square yard of surface. Here, therefore, we have a continual store of motion kept up in the molecules of matter on the earth's surface to be wasted in great part in imparting motion to the ether of space. It would certainly look, *à priori*, as if there ought to be some means of utilising this store of motion.

3. The second law of thermodynamics would (as is known) assume that this would not be practicable. This law was propounded simply as what was considered a legitimate inference from the observed behaviour of heat. But a great advance in the knowledge of the nature of heat has been made since that time, and it should be noticed that the law is (admittedly) by no means *theoretically* necessary or requisite to satisfy the principle of the conservation of energy. Indeed a conceivable case opposed to it has been pointed out by Prof. Maxwell, though one not capable of being practically carried out. It was my purpose in the last article to direct attention to a physical process opposed to the law and admitting of practical realisation, in the effects attendant on the diffusion of matter. At the time when this law was enunciated the character of the motion termed "heat" (as illustrated in the now accepted kinetic theory of gases) was unrecognised, and therefore the *mechanism* of the diffusion of gases was not understood. Under these conditions, therefore, it would not be much a point for surprise if increase of knowledge should show the law not to be generally applicable (or not to admit of that general application which is implied by the use of the term "*law*").

4. It may serve greatly to facilitate the following of this subject if we visualise the relations of heat and work more closely. Since "*heat*" is simply a *motion* of small portions of matter (termed "molecules"), and since the

[1] "On the Diffusion of Matter in Relation to the Second Law of Thermodynamics," see NATURE, vol. xvii. p. 31.

transference of this motion to visible masses is called "*work*," so therefore the conversion of heat into work is no more than the transference of motion from small to large portions of matter, *i e.*, the transference of motion between portions of matter of *different* dimensions. *The mechanical equivalent of heat* therefore simply represents *the equivalence in energy between the motions of portions of matter of different dimensions* (molecules and visible masses). To deny, therefore, that the heat possessed by matter at normal temperature could be converted into work would be to assume that by a certain difference in dimensions the conditions are such that motion can no longer be transferred from the smaller portions of matter to the larger. This would evidently, *à priori*, be by no means a necessary assumption; indeed it would appear, perhaps, rather strange that by no device could such a thing be done.

5. At the first sight one difficulty in the way of utilising this motion that surrounds us on all sides is that the larger scale portions of matter (visible masses) are immersed among the smaller scale portions of matter (molecules) which surround the visible masses on all sides (as the molecules of the surrounding air, &c.), so that a perfect equilibrium of motion exists on all sides; so that it becomes impossible to transfer the motion to the larger scale mass in the one direction or in the other, and we cannot lay hold of each moving molecule individually, on account of its minute size.

6. It is an observed fact (and demonstrated theoretically) that portions of matter in motion among themselves tend to acquire the *same* energy of motion (called "temperature" in the case of molecules). In accordance with well-known facts, whenever the energy of this system of small moving portions of matter is greater in one part than in another, *i.e.*, whenever the equilibrium of energy is upset, then we can transfer some of the energy to larger scale masses (convert heat into work). Is there, however, no other means of converting heat into work but through *inequality of energy?* It was pointed out in the last article that *inequality of velocity* (by the mechanism of diffusion) will serve the same purpose. The portions of matter (molecules) which by equal temperature possess equal energy, possess, when their *masses* are unequal, unequal velocities. This inequality of *velocity* can be utilised for work as well as inequality of *energy*.

7. Since size is only *relative*, or there is nothing absolute in size, it will be quite legitimate to suppose molecules magnified up to a larger scale so as to be visible, and we do this as in dealing with the *mechanism* of a process, it is almost impossible to visualise or conceive clearly the results without this condition, and it is our object, on account of its practical bearing, to exhibit the process involved in a clear light. Suppose, therefore, the molecules of two diverse gases (oxygen and hydrogen) to be magnified up to visible dimensions, and as we are not concerned with the shape or form of the molecules, we may simply represent the molecules of the two gases by a number of spheres, those representing hydrogen possessing each one-sixteenth of the mass of those representing oxygen, and also possessing a normal velocity four times as great. This is known to be the fact in the case of the two gases when at the same temperature. We will further suppose the spheres inclosed in the two separate halves of a cylinder with a piston between them. The spheres may either be supposed perfectly elastic or their motion kept up artificially in some way; just as in the case of a gas the motion of its molecules is kept up by the molecular vibrations of the sides of the cylinder.

8. The spheres of the two sets possess *equal* energies of motion, the one set making up in mass for what they want in velocity. The colliding spheres in each compartment will arrange themselves (according to a known principle) so that the number of spheres in unit of volume of each set is the same, and therefore the pressure exerted by their impacts on opposite sides of the piston will produce perfect equilibrium, so that the piston remains immovable. Now the question is, supposing that (as in the case of molecules) we cannot lay hold of each of these spheres separately, is there any means of utilising the *inequality* of velocity for the performance of work? [This is the question we have to make in the case of two gases at the same temperature, whose molecules we cannot grasp, and which possess *unequal* velocities.] If we could by any device get a number of the spheres from one compartment into the other without changing their velocities in the act, then the pressure would evidently rise in one compartment, and we should thus obtain a capacity for work without the performance of work. It is evident that this could be done by making several perforations in the piston, about the size of the spheres themselves, so that the spheres, in impinging against the piston would sometimes happen to encounter the void space of a hole, and thus pass through with unchanged velocity into the opposite compartment. If the spheres of the two sets were moving with *equal* velocities, it is evident that as many on an average would pass through one way as the other, and there would therefore be no disturbance of the equilibrium of pressure, and consequently no work to be derived. But from the fact that the spheres are moving with *unequal* velocities, a different result will ensue It will be evident that the number of spheres passing through the hole will be proportional to the number of times they strike against the piston, for the chances that a sphere will encounter a hole will be proportional to the number of its impacts against the piston, *i.e.* to the *velocity* of the sphere. So the velocity of the spheres in one compartment being four times that in the other, four times as many lighter spheres pass through one way, as heavier spheres pass through the other. The number of spheres in one compartment will therefore rapidly augment, and thus the *pressure* against the piston will rise, and the piston will be finally driven towards the opposite end of the cylinder, and in this act *energy* will be transferred from the spheres in the one compartment to those in the other; or part of the energy could be transferred to an outside mechanism in a self-acting manner if desired, by simply connecting the piston to the mechanism.

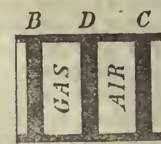
9. Now if precisely the same thing can be done in the case of two gases, it is evident that here the energy being *heat*, we have in the result attendant on the motion of the piston, the transference of heat from one portion of gas to another at normal temperature, *i e.* the transference of heat in a self-acting manner from a *colder to a hotter portion of matter;* and if desired, a conversion of a part of the heat of the gas (at normal temperature) into *work* by cooling it down below the temperature of the coldest of surrounding objects.

10. In the case of a gas it is clear that we cannot make perforations analogous to the above sufficiently small to suit molecules, but to attack molecules we must have recourse to *molecular* mechanism, or to attempt to handle them like the spheres we must have recourse to mechanism on a suitable scale. We have such a mechanism in a porous diaphragm (such as of pipeclay or plumbago) which represents a piston with *molecular* perforations. Such a diaphragm, if fitted as a piston into a cylinder will exhibit, with the molecules of two separate gases possessing different molecular velocities (such as molecules of oxygen and hydrogen), precisely the same phenomena as those exhibited, simply on a magnified scale in the case of the spheres; or the above description applies word for word. We have by the motion of the porous piston *the conversion of the heat-motion of the gas at normal temperature into work*, the transference of heat automatically from the *colder* portion of gas to the warmer. The second law of thermodynamics only holds when the molecules brought into contact happen to be of the *same*

kind, or, more accurately speaking, of the *same mass.* This latter case is evidently exceptional, and if a case be exceptional the term "*law*" becomes no longer applicable to it.

11. The rates of diffusion of hydrogen and oxygen across the porous diaphragm are known to be as four to one, *i.e.* as the *molecular velocities.* The above illustration of the spheres may serve to exhibit the *physical basis* or cause of this fact in a clear light. The mere statement that the rates of diffusion are inversely as the square roots of the molecular weights of the gases, evidently throws no light on the cause or *physical basis* of the action, which is always the main thing to realise in physical science. The fact that diffusion is in the above ratio to the molecular weight, evidently only happens to be true because the molecular *velocity* is in that same ratio to the molecular weight, otherwise molecular weight has nothing whatever to do with the rate of diffusion. So it will be equally apparent, from the above illustration, that the rate of diffusion of a gas through a porous diaphragm has nothing whatever to do with the *pressure* of the gas, but depends, *cæteris paribus*, on the number of molecules of the gas in unit volume. An increase of the number of molecules in unit volume (by adding to the number of impacts of the molecules against the vessel) increases the pressure, and this is why diffusion *appears* to be dependent on pressure, though evidently *physically* it has nothing to do with it. This serves to explain how, provided the molecular velocities of the gases are considerably diverse, such enormous differences of pressure can take place by diffusion through a porous diaphragm, the pressure having no power whatever to adjust itself through the diaphragm; for the passage of a molecule through the diaphragm simply depends whether, in its normal motion, it happens to encounter a pore or not. The above illustration may also serve to show that the velocity of propagation of any impulse ("wave") by a system of bodies in free collision can only be dependent on the *normal velocity of the bodies*, just as a system of couriers interchanging motion among themselves convey a message at their own rate. So the molecules of a gas interchanging motion among themselves convey an impulse at their own rate, and thus the velocity of sound in a gas can be solely dependent on, and proportional to, *the velocity of the molecules of the gas*, and on nothing else. This must evidently be true on the basis of the kinetic theory, and this theory being now accepted, it would be not unreasonable to expect that in so fundamental a matter as the propagation of sound, an explanation of it on the basis of this theory would be looked to, for a *statical* theory of the propagation of sound appears scarcely to harmonise with the *dynamical* theory of gases. We have alluded to this fact as briefly as possible, having the illustration of the spheres at hand. There may be a liability to lose sight of facts like the above unless due care be taken to realise molecular phenomena by picturing them on a larger scale. The velocity of sound in hydrogen is four times greater than in oxygen, solely because the velocity of the molecules of hydrogen is four times greater than the velocity of the molecules of oxygen—nothing conceivably to do with the molecular weight of the gas, excepting in so far as a less molecular weight determines a higher molecular *velocity.*[1] The rate of propagation of the wave is affected by temperature in so far as the *velocity* of the molecules of the gas (in whose motion the *heat* of the gas consists) is affected by temperature.

12. As an illustration of a simple form of apparatus adapted for converting normal temperature heat into work, and admitting of continuous actuation, the following rough sketch may serve:—Let the annexed diagram represent a cylinder containing three pistons, B, D, C, the



central one, D, of which is furnished with any porous diaphragm (such as of plumbago, or porous earthenware). Let any light gas (hydrogen being the most effective) be supposed introduced into one-half of the cylinder, some heavier gas (or air) filling the other half. All three pistons are supposed (first) fixed. Then, as is known, diffusion commences through the porous diaphragm, everything remaining necessarily at normal temperature so long as the pistons are fixed and no work is done. The rapidly moving molecules of the light gas pass in greater numbers through the pores of the diaphragm than those of the heavy gas (or air), so that the pressure rises in the compartment originally filled with air. As soon as the pressure has attained a maximum, the central piston is automatically released, and is thus driven by the excess of pressure towards the opposite end of the cylinder, the portion of gas which does the work being chilled and the heat transferred in the form of work to the outside machinery with which the central piston is connected. A certain part of the heat goes to the portion of gas *towards* which the piston is driven, heat thus passing from a colder to a hotter body (for as soon as the portion of gas commences to be chilled, it is already the colder). Simultaneously with the stroke of the central piston, a convenient automatic arrangement connected with the machinery oscillates the two end pistons inwards and outwards, expelling in the inward stroke (through convenient openings) the diffused mixture of gas and air, and by the outward stroke drawing in a fresh supply. Of course the valves suitable for this are not given, as it is only our purpose to sketch the *principle* of such an apparatus as a scientific point, and having no regard to any question of commercial value or not. Clearly the power derived would depend on the specific gravity of the gas used, and would be proportional (*cæteris paribus*) to the area of the piston. Coal gas would give a less power than hydrogen. A diffused mixture of gas and air is necessary for gas engines, the mixture being exploded in them. It is clear that it would be possible, by means of an apparatus of the above character, to derive power in the act of mixing the gas and air previous to exploding the mixture. The gaseous mixture, after passing through the apparatus, could be stored in some reservoir or receptacle, so as to recover (before combustion in the gas engine) from surrounding objects the heat which it lost by conversion into work in the diffusion engine. By this procedure it may be observed that the heat converted into work is derived from *the normal store of heat possessed by surrounding objects*, and their store is finally made good by the sun, which latter may therefore be regarded as the ultimate source of the energy derived.

13. In view of the numerous porous structures existing in the animal and vegetable world (*porosity* being a distinguishing characteristic of animal and vegetable organisms), also taking into consideration the prevalence of gases of different molecular weights, notably oxygen and carbonic acid (which are known to be intimately connected with animal and vegetable processes); the conclusion would seem warranted, and even necessary, that work on the above principle must take place widely in nature, and thus part of the store of energy accumulated in materials on the earth's surface by the sun, is made to fulfil a useful end, instead of being dissipated uselessly in space.

S. TOLVER PRESTON

[1] It is evident that though the velocity of the wave is proportional to the velocity of the molecules, the *absolute* velocity of the wave must be to a certain fixed degree less than that of the molecules; for the molecules in their normal motions are moving more or less *obliquely* to the path of the wave. This I have pointed out in a paper, published in the *Philosophical Magazine* for June, 1877, where the true mathematical relation for the velocity has been determined by Prof. Maxwell, and is there given.

ARARAT[1]

IN the childhood of mankind the dwellers in Western Asia cherished the story of a great flood which drowned all their race save one man and his family. They told the tale from father to son, how the flood rose till it covered their highest hills, and how the ark in which their ancestor had saved himself, his family, and a motley crowd of animals floated on the waters until, when these abated, it came to rest on the first emerging summit of the land. They chose as the scene of this new starting-point for humanity the loftiest peak of which they had knowledge—a vast snowy cone shooting far into the blue air above, and shrouding itself every day in cloud and storm. No one had ever climbed to its mysterious summit since the ark rested there. But generation after generation looked up to it with awe and veneration from the plains of Armenia. The story spread far away into other lands. It became part of the religious teaching of nearly a half of mankind. No mountain is so familiar, all the world over, as that from which Noah is famed to have descended to re-people the earth. The first conception which, as children, most of us have formed of a mountain, arose out of this story of Ararat.

Apart from its legendary associations and the mystery arising from its reputed inaccessibility, there must be something strangely fascinating about Ararat. Men who have seen much of mountains in many countries speak of it as the noblest mass among them all. The summit of its snowy cone (17,000 feet) greatly exceeds any European peak in elevation, and sweeps up from the level plain of the Araxes (2,500 feet) as from a sheet of water. It looks as if it might well claim to be linked with the oldest of human traditions.

So impressive a mountain, so long associated with man's faith and history, would have been appropriately placed among the most ancient landscapes of the earth's surface. Some scenes suggest only the changes of yesterday; others set us thinking of the earliest condition of our world. We naturally look for a kind of consonance between the venerable antiquity of the associations which gather round Ararat and the primeval character of the mountain itself. But geology delights in contrasts, and nowhere could so impressive a contrast be found between the remoteness of the tradition and the comparative youth of the mountain on which it lingers. Here we find no colossal pyramid of granite with outer folds of more ancient rocks such as have been built up and carved into the oldest mountain-chains. In reality it is but a mountain of yesterday, possibly not so old as the advent of man upon the earth, certainly much younger than many plants and animals now living.

Great and Lesser Ararat from the North-east.

To a student of the evolution of the earth's surface-features there is something profoundly suggestive in the long line of depressions and ridges which separates Europe from Africa, and stretches eastward through the heart of Asia. On the one hand, he sees the basins of the Mediterranean, Black, Dead, Caspian, and Aral Seas; on the other he notes how, in a general sense, parallel with these deep troughs, run massive mountain ridges, including the great axis of the Old World. He finds, on closer research, that while most of these ridges have received their latest upheavals at a recent geological date, they yet for the greater part belong originally to earlier periods of disturbance, some of them, indeed, bearing witness to many successive uplifts during a vast section of geological time. Yet further examination will bring before him evidence that along some of these lines of earth-folding, volcanic action has of old been abundant; and that the present Mediterranean volcanoes are but the lingering remnants of the chain of actively burning mountains which ran through Asia Minor and crowned the peaks of the Caucasus. And he will discover that just as there have been successive uplifts of the same axis or mountain-chain, so have there been widely-separated outbursts of volcanic activity during a long course of ages from the same focus of discharge.

It is in relation to this remarkable history that Mount Ararat acquires its main geological interest. Thanks chiefly to the veteran Abich a good deal is now known of the geology of the Caucasian and Transcaucasian ridges. He has shown how a nucleus of Devonian and car-

[1] Transcaucasia and Ararat. By James Bryce. (London: Macmillan and Co., 1877.)

boniferous limestone rocks appears even under the mass of Ararat, and has drawn the inference from his wanderings in that region, that in the beginning of the Upper Carboniferous Limestone period a great continental upheaval took place during which the Armenian region received its first outlines. The land thus raised he believes to have remained above water until, in the course of the Cretaceous period, it so far sank as to become an island, and continued in this condition even into Pliocene times, when the whole of that region became involved in another vast continental upheaval to which the final modelling of the Armenian highlands was due. These great terrestrial movements were accompanied by the outbreak of volcanic action. Abich regards the diabase, diorite, and porphyry rocks as having been abundantly erupted during the Jurassic period and to have played an important part in the formation of the mountain masses, especially in the Lower Caucasus. To late Tertiary times, however, belong the trachytic and doleritic lavas which have been poured forth on so colossal a scale as to form such mountains as Elbruz, Kasbek, Ala Göz, and the two Ararats.

In Mr. Bryce's recently published volume (to which attention has already been drawn in NATURE) we have a record of the latest and probably the most daring ascent of Mount Ararat. Though not a professed geologist he has had a geological training, and has seen much of many lands, alike in the Old World and in the New. It was not to make out any obscure point in the structure of Ararat that he bent his steps towards that little known mountain. But he had climbed many a peak in Europe, and he no doubt longed to set foot upon the high places of another continent. So he made a pilgrimage to the heights of Armenia, with no thought, however, of writing a book about his journey. The volume he has just published has been partly wrung from him by the importunity of friends, who reasonably supposed that the world might be as much interested as they in knowing more about Ararat. In its charmingly fresh and graphic pages one gets such a living picture of the mountain as cannot be gained from any of the geological memoirs. From long experience of mountain climbing his eyes are so keen and so trained, while his pen is so facile and vivid that we can mount with him as he goes warily over each lava-current, rubbish-cone, and snow-slope. We feel the sharp thin air of the mountain as it blows through his narrative. We join in his quiet chuckle as he halts at a solitary piece of wood far up on the cone and irreverently detaches a fragment for the inspection of those who cannot personally discover whether the true ark still rests on the top of Ararat. And we can sympathise with his awe as he stood among the clouds alone on the summit of the mysterious mountain. It is not for any new scientific facts so much as for the vivid sketch of the general aspect of the huge volcanic mass that his book has an interest to geologists.

A vignette of Ararat forms the frontispiece of the volume, which is here reproduced. In the middle distance is shown the alluvial plain of the Araxes. Below the snowy cone and icy cliffs of the Greater Ararat a deep cleft or recess appears with huge cliffs somewhat like the Val del Bove of Etna, and no doubt due to some of the volcanic explosions of the mountain. On the skyline of this slope, towards the base of the larger cone, some of the late cinder-cones and craters appear. Some of these are still so fresh and perfect that they look as if they had been active only the other day and might blaze forth again to-morrow. The graceful outline of the Lesser Ararat rises on the left. ARCH. GEIKIE

AGE OF THE SUN IN RELATION TO EVOLUTION

ONE of the most formidable objections to the theory of evolution is the enormous length of time which it demands. On this point Prof. Haeckel, one of the highest authorities on the subject, in his "History of Creation," has the following:—"Darwin's theory, as well that of Lyell, renders the assumption of immense periods absolutely necessary. . . . If the theory of development be true at all there must certainly have elapsed immense periods, utterly inconceivable to us, during which the gradual historical development of the animal and vegetable proceeded by the slow transformation of species . . . the periods during which species originated by gradual transmutation, must not be calculated by single centuries, but by hundreds and by millions of centuries. Every process of development is the more intelligible the longer it is assumed to last."

There are few evolutionists, I presume, who will dispute the accuracy of these statements; but the question arises, does physical science permit the assumption of such enormous periods? We shall now consider the way in which Prof. Haeckel endeavours to answer this question and to meet the objections urged against the enormous lapse of time assumed for evolution.

"I beg leave to remark," he says, "that we have not a single rational ground for conceiving the time requisite to be limited in any way. . . . It is absolutely impossible to see what can in any way limit us in assuming long periods of time. . . . From a strictly philosophical point of view it makes no difference whether we hypothetically assume for this process ten millions or ten thousand millions of years. . . . In the same way as the distances between the different planetary systems are not calculated by miles but by Sirius-distances, each of which comprises millions of miles, so the organic history of the earth must not be calculated by thousands of years, but by palæontological or geological periods, each of which comprises many thousands of years, and perhaps millions or milliards of thousands of years."

Statements more utterly opposed to the present state of modern science on this subject could hardly well be made. Not only have physicists fixed a limit to the extent of time available to the evolutionist, but they have fixed it within very narrow boundaries.

Every one will admit that the organic history of our globe must have been limited by the age of the sun's heat. The extent of time that the evolutionist is allowed to assume depends, therefore, on the answer to the question, What is the age of the sun's heat? And this again depends on the ulterior question, From what source has he derived his energy? The sun is losing heat at the enormous rate of 7,000 horse-power on every square foot of surface. And were it composed of coal its combustion would not maintain the present rate of radiation for 5,000 years. Combustion, therefore, cannot be the origin of the heat.

Gravitation is now almost universally appealed to as the only conceivable source from which the sun could have obtained his energy. The contraction theory advocated by Helmholtz is the one generally accepted, but the total amount of work performed by gravitation in the condensation of the sun from a nebulous mass to its present size could only have afforded twenty million years' heat at the present rate of radiation. On the assumption that the sun's density increases towards the centre, a few additional million years' heat might be obtained. But on every conceivable supposition gravitation could not have afforded more than twenty or thirty million years' heat.

Prof. Haeckel may make any assumption he chooses about the age of the sun, but he must not do so in regard to the age of the sun's heat. One who believes it *inconceivable* that matter can either be created or annihilated may be allowed to maintain that the sun existed from all eternity, but he cannot be permitted to assume that our luminary has been losing heat from all eternity.

If 20,000,000 or 30,000,000 years do not suffice for the evolution theory, then either that or the gravitation theory of the origin of the sun's heat will have to be abandoned.

In a former paper (*Quarterly Journal of Science* for

July, 1877) I have proved from geological evidence that the antiquity of our habitable globe must be at least three times greater than it could possibly be had the sun derived its heat simply from the condensation of its mass. This proves that the gravitation theory of the origin of the sun's heat is as irreconcilable with geological facts as it is, according to Haeckel, with those of evolution, and that there must have been some other source, in addition, at least, to gravity, from which the sun derived his store of energy.

That other source is not so inconceivable as has been assumed, for it is quite conceivable that the nebulous mass from which the sun was formed by condensation might have been possessed of an original store of heat previous to condensation. And this excessive temperature may be the reason why the mass existed in a nebulous or rarefied condition. Now if the mass were originally in a heated condition then in condensing it would have to part not merely with the heat of condensation, but also with the heat it originally possessed.

The question then arises—By what means could the nebulous mass have become incandescent? From what source could the heat have been obtained? The dynamical theory of heat affords, as was shown several years ago (*Phil. Mag.* for May, 1868), an easy answer to this question. The answer is that the energy in the form of heat possessed by the mass may have been derived from *motion in space.* Two bodies, each one-half the mass of the sun, moving directly towards each other with a velocity of 476 miles per second, would, by their concussion, generate in a *single moment* 50,000,000 years' heat. For two bodies of that mass, moving with a velocity of 476 miles per second, would possess $4,149 \times 10^{38}$ foot-pounds of kinetic energy, and this, converted into heat by the stoppage of their motion, would give out an amount of heat which would cover the present rate of the sun's radiation for a period of 50,000,000 years.

There is nothing very extraordinary in the velocity which we have found would be required to generate the 50,000,000 years' heat in the case of the two supposed bodies. A comet having an orbit extending to the path of the planet Neptune, approaching so near the sun as to almost graze his surface in passing, would have a velocity of about 390 miles per second, which is within eighty-six miles of that required.

It must be borne in mind, however, that the 476 miles per second is the velocity at the moment of collision. But more than one-half of this velocity, or 274 miles per second, would be derived from their mutual attraction as they approached each other. We have consequently to assume an original or projected velocity of only 202 miles per second. If the original velocity was 678 per second, this, with the 274 derived from gravity, would generate an amount of heat which would suffice for 200,000,000 years. And if we assume the original velocity to have been 1,700 miles per second, an amount of heat would be generated in a single moment which would suffice for no less than 800,000,000 years.

It will be asked, Where did the two bodies get their motion? It may as well, however, be asked, Where did they get their existence? It is just as easy to conceive that they always existed in motion as that they always existed at rest. In fact, this is the only way in which energy could remain in a body without dissipation into space. Under other forms a certain amount of it is constantly being transformed into heat which never can be retransformed back again, but is dissipated into space as radiant heat. But a body moving in void stellar space will retain its energy in the form of motion undiminished and untransformed for ever, unless a collision takes place.

The theory that the sun's heat was originally derived from motion in space is, therefore, for this reason, also more in harmony with evolution than the gravitation theory, because it explains how the enormous amount of energy which is being dissipated into stellar space may have existed in the matter composing the sun untransformed during bygone ages. Or in fact for as far back as the matter itself existed.

In conclusion there are only two sources conceivable from which the sun could have derived his heat. The one is *gravitation*, the other *motion in space.* The former could have afforded only about 20,000,000 or 30,000,000 years' heat, but there is in reality no absolute limit to the amount which may have been derived from the latter source, for the amount generated would depend on the velocity of motion. And when we take into consideration the magnitude of the stellar universe, the difference between a motion of 202 miles per second, and one of 1,700 miles to a great extent disappears, and the one velocity becomes about as probable as the other.

It may be urged as an objection to the theory that we have no experience of bodies moving in space with such enormous velocities as the above. This objection, for the following reason, is of no weight.

No body moving with a velocity exceeding 400 miles per second could remain a member of our solar system; and beyond our system there is nothing visible but the stars and nebulæ. These stars, however, are suns like our own, and visible because, like the sun, they have lost their motion—the lost motion being the origin of their light and heat. Bodies moving in stellar space with these enormous velocities can have neither light nor heat, and, of course, must be invisible to us. They must first lose their motion before the kinetic energy in the form of motion can be transformed into light and heat, so as to constitute visible suns.

JAMES CROLL

ON THE FORMATION OF HAILSTONES, RAINDROPS, AND SNOWFLAKES[1]

THE author commences by recapitulating some of the leading points in a paper which he read before the same Society on October 31, 1876, "On the Manner in which Raindrops and Hailstones are Formed." In this paper, which was published in NATURE (vol. xvi. p. 163), he had shown that the aggregation of the small cloud particles into raindrops or hailstones is sufficiently accounted for by the fact that the larger particles descend faster than the others, and consequently overtake those immediately beneath them, and, combining with these, form still larger particles, which move with greater velocity, and more quickly overtaking the particles in front of them, add to their size at an increasing rate. He also showed that the shape and structure of ordinary hailstones was exactly such as would result from this manner of formation. For he had observed that the shape of hailstones was not as it at first sight appeared, that of more or less imperfect spheres, but that of more or less imperfect cones or pyramids with rounded bases, the conical surfaces being striated, the striæ radiating from the vertex; the texture being that of an aggregation of a number of small ice particles without crystalline form, being packed more closely together toward the base or rounded face of the stone. In this paper the author had reverted to the possibility of making *artificial hailstones* by blowing a stream of frozen fog against a small object, making, as it were, the cloud to rise up and meet the stone, instead of the stone falling through the cloud.

He had not, however, then overcome the difficulty of obtaining such a stream of frozen fog, but gave two sketches of plaster stones, which, as far as their shape and the striated appearance of their surface were concerned, closely resembled hailstones, and which plaster stones had been obtained by blowing some finely-divided

[1] Abstract of paper by Prof Osborne Reynolds, F.R.S., read at the Manchester Literary and Philosophical Society.

plaster of Paris against small splinters of wood by means of a jet of steam.

In the discussion which followed the reading of that paper Dr. Crompton suggested the ether spray, such as is used in surgery, as a means of obtaining a frozen fog, and Prof. Reynolds explains how after various attempts he had succeeded in combining a spray of ether and water so as to form artificial stones. He then proceeds as follows :—

The apparatus is shown in Fig. 1. It consists of a brass tube half an inch in diameter, one end of which is connected with bellows capable of maintaining a constant pressure of about 18 inches of water, on the other end of the tube is a cap over the end of which is a flat plate or diaphragm having a central opening $\frac{1}{8}$ of an inch in diameter which forms the aperture for the blast. Entering through the sides of the main brass tube are two small brass tubes which reach to within $\frac{1}{2}$ an inch of the plate and into the ends of which are sealed fine glass capillary tubes, the glass being very thin; these protrude just through the middle of the aperture, the one about $\frac{1}{16}$ of an inch and the other $\frac{1}{32}$. Through these tubes the water and ether are separately introduced into the blast to form the spray, and it is mainly on the adjustment of these tubes that the efficiency of the apparatus depends. It is essential that the ether tube should be slightly the longest, otherwise the ends become stopped with ice, and I find it better that the ether tube should be somewhat larger than the water tube. The bore of the tubes must be very small, but this is not sufficient, for unless the glass is very thin the spray will not be finely divided. Both the ether and water are forced through the tubes from bottles by connecting the interiors of these bottles with the bellows, and the quantities of ether and water are regulated either by raising and lowering the bottles or by means of the cocks in the pipes.

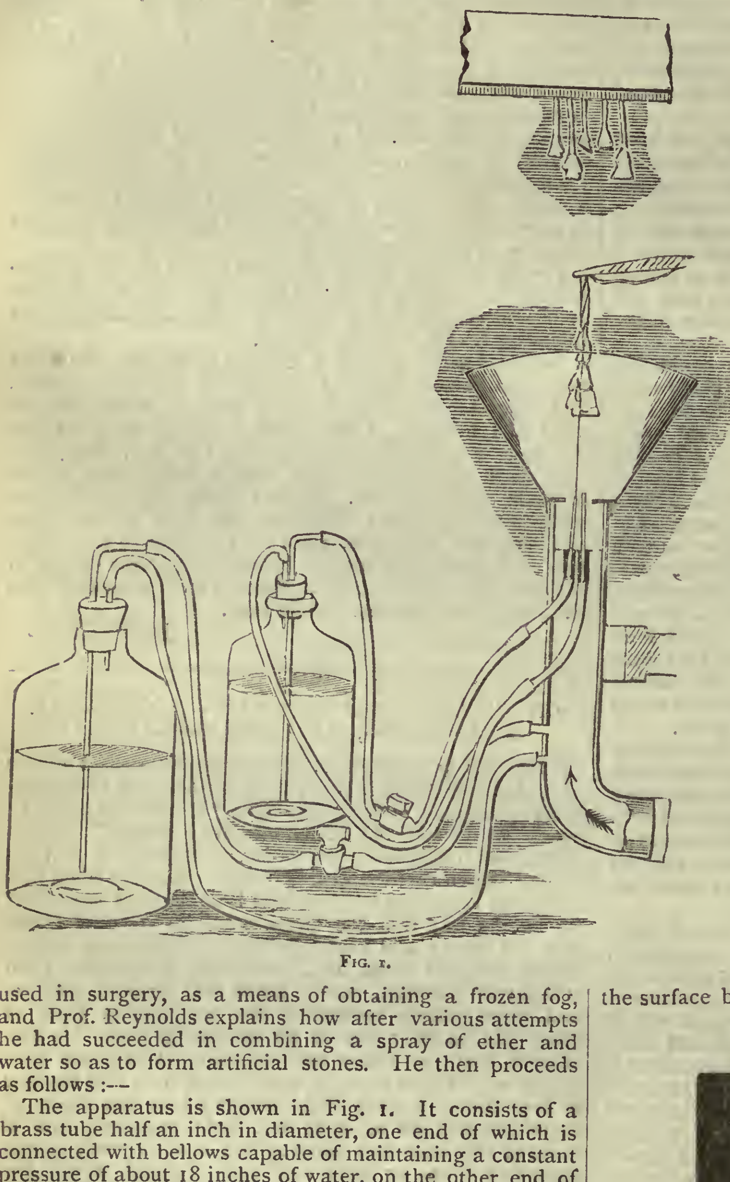


FIG. 1.

The tube is fixed in an ordinary retort-stand, so that the blast is vertical. If then a small splinter of wood is held pointing downwards into the spray, a lump of ice forms on the end of the splinter, and this lump has all the appearance of the hailstones. It is quite white and opaque, it is conical in form and has a rounded base and striated surface.

In this way I have formed stones from half to three-quarters of an inch in diameter. When, however, the stones are growing large it is necessary to move this splinter so as to expose in succession all parts of the face of the stone to the more direct action of the spray.

When using this apparatus in a warm room I have found it best to fix a pad of blotting paper over the jet at a height of 10 or 12 inches. The surface of this pad is cooled by the spray and prevents radiation from the ceiling, which otherwise tends to melt the top of the stone. For a similar reason I have found it well to surround the blast with a wide cylinder or inverted cone of paper, which keeps off radiation without interfering with the action of the jet.

By sticking several pieces of wood into the pad, pointing downwards, a number of stones may be made at once.

In Fig. 2 a medium-sized stone as well as one of the largest stones are shown attached to the splinters of wood. The surface of the cone, where continuous, is truly conical, or rather pyramidal, but this surface is broken, as it were by steps, and a very marked fact is that all the continuous surfaces have the same vertex, and hence the different conical surfaces to which they belong, have not the same vertical angle, the surface being exactly such as would be acquired by the fragments of a sphere so constituted that the fracture tended to follow radial lines.

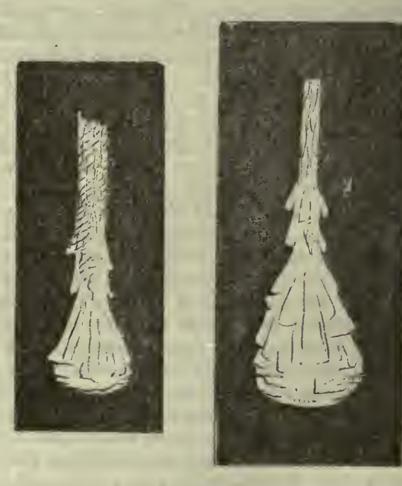


FIG. 2.

Owing to the radiation of the surfaces from a common vertex and the steps which occur between the vertex and the base, the angle of the conical surface of the stone is greater near the vertex than near the base. Thus the smaller stones appear less elongated than those which are larger.

The fact that in the sketches of actual stones, which I gave in my last paper, I showed the steps as less pronounced and the angles larger than they are in the artificial stones, is probably owing in some measure to my having formed my ideas from the observation of favourable specimens chosen from amongst those which fell. The larger angles were probably also, in part, owing to the smaller size of the actual hailstones, which were not much more than one-fourth of an inch across. But I think that it is important to notice that the somewhat imperfect way in which the outside layers in the surface of the artificial stones are continued, may be owing to the narrowness of the jet of air which, on striking the stone, tends to diverge laterally rather than to flow upwards past the sides of the stone, as it would do if the jet were broader, or as the air must do when the stone is falling through it.

The rate at which stones can be formed depends on the amount of water which can be introduced into the spray, the larger stones taking from one to two minutes. At first sight this may seem to be somewhat slow, but the following estimate tends to show that the artificial are probably formed quicker than the actual stones.

The speed of the jet of air at the point at which the stones are formed is nearly equal to that at which the larger stones would fall through the air. This is shown by the fact that if a large stone becomes accidentally detached from its splinter of wood it rather falls than rises, but when this happens with smaller stones they are driven up by the force of the blast.

I find that the speed of the blast varies from 150 to 200 feet per second, *i.e.*, from one-and-a-half to two miles a minute. The larger stones, therefore, traverse from one to four miles of frozen spray. So that if we imagine a cloud as dense as the spray it would have to be from one to four miles thick in order that the stones might, in falling through it, attain the size of the artificial stones ; and considering that the stones would only gradually acquire a speed equal to that of the blast, the time occupied in falling through the cloud would in all probability be very considerable, at least from five to ten minutes after the stone had acquired a sensible size.

As regards the proportion which the density of spray bears to that of a cloud, a comparison may be made from the fact that when working in saturated air at a temperature of 60° or 70° F., the condensation of vapour supplied sufficient ice to form the spray ; and since it is probable that the dense summer clouds, from which hail is formed, result from the cooling of air from temperatures nearly, if not quite, equal to this, there is probably no great difference in the density of the clouds and the spray.

I have not yet had an opportunity of examining the texture of these stones under the microscope, but to all appearance they consist of an aggregation of small spherical particles of ice ; and it seems worthy of notice that while nothing like a snow crystal ever appears to be produced in the ether spray, the moment the blast is stopped the end of the ether tube becomes covered with ice, which often assumes the form of snow crystals.

This appears to indicate the character of the difference between those conditions which result in snow and those which result in hail.

When the cloud-particles are formed at or above the temperature of 32° and then freeze, owing to cooling by expansion or otherwise, the particles as they freeze retain their spherical form. This is what happens in the spray.

On the other hand, when saturated air at a temperature below 32° is still further cooled, the deposition of the vapour will be upon ice, and will take the form of snow crystals.

The aggregation of the snow crystals into flakes is, as I pointed out in my previous paper, accounted for by the larger crystals overtaking the smaller crystals in their descent, and the still more rapid descent of the flakes as they increase in size.

As regards the formation of rain-drops, I have nothing to add to what was contained in my last paper. The same explanation obviously applies to both hail and rain, and any doubt which may have been left by the less direct arguments in my former paper will, I venture to think, have been removed by the verification of my predictions in the production of artificial hailstones so closely resembling in all particulars those formed by nature. And, in conclusion, I would thank Dr. Crompton for the suggestion of the means by which I have been able to produce these stones.

OUR ASTRONOMICAL COLUMN

THE SOUTH POLAR SPOT OF MARS.—Prof. Asaph Hall has instituted a series of measures of the position of the south polar spot of Mars, with the Washington refractor during the late favourable opposition of the planet, having been led thereto by the great discordances in the positions of the spot, as determined so far. He adopts Oudeman's node and inclination of the equator of Mars, which, for the epoch taken, viz., 1877, September, 17·0, G.M.T. give N = 47° 56′, I = 39° 14′, and the angle of position of the south pole 162° 6′, and assumes the time of rotation of the planet 24h. 37m. 22·73s., as found by Mr. Proctor. The observations were made with a power of 400, and on thirty-two nights, from August 10 to October 24, during the whole of which period the spot was always seen with great distinctness, and little change in its appearance noted except what might be accounted for by change of distance. From thirty-four equations of condition treated on the method of least squares, Prof. Hall finds for the angle of position of the south pole of Mars at the above-mentioned epoch 166° 22′, for the radius of the small circle described by the spot 5° 11′, and for the angle of position of the spot at the epoch, with respect to the rotation-axis of the planet, 311° 24′. The various determinations of the south polar distance of this spot are as follow :—

	°	′		°	′
Herschel, 1783 ...	8	8	Linsser, 1862	20	0
Bessel, 1830	8	6	Kaiser, 1862	4	16
Mädler, 1837	12	0	Hall, 1877	5	11
Secchi, 1857	17	42			

On several of the finer nights, when the markings on the edge of the spot were very distinct, it appeared as "a depression in the surface of the planet."

PROF. NEWCOMB'S LUNAR RESEARCHES.—It is understood that if no unforeseen delay occurs in the printing, Part I. of "Researches on the Motion of the Moon," upon which Prof. Newcomb has been engaged for six years past, will be ready for publication in the course of next month. It is devoted to the discussion of eclipses and occultations previous to 1750. An abstract appeared in *Silliman's Journal* for November last.

THE CORDOBA OBSERVATORY.—In an address delivered on November 4, on the occasion of receiving from the Governor of the province of Cordoba the premiums awarded at the Centennial Exhibition in Philadelphia to the Argentine National Observatory and to himself for Lunar and Stellar Photographs, Dr. B. A. Gould gave a brief outline of the successive applications of photography to astronomical purposes since Mr. Bond's experiments with the 15-inch refractor of Harvard Observatory in 1850, with more particular reference to work executed at Cordoba of late in this direction. Dr. Gould expresses

himself satisfied with the results obtained at the Argentine Observatory; the photographs of the moon at full and in the last quarter he thinks may be favourably compared with any obtained elsewhere which he had seen. He refers to "the very beautiful picture of the moon" made with the 4-feet reflector at Melbourne, which was also exhibited at Philadelphia, and adds, he is not sure, if he had seen this elegant photograph before placing his own on exhibition, he would have ventured to compete. Dr. Gould remarks that much of the credit of the stellar photographs is due to the pure air of Cordoba, which is incredibly transparent on the not very numerous occasions when the sky is really clear. The impressions on glass exhibited were of six different clusters, the plate of the cluster X Carinæ containing two images each of 185 stars, and that of η Argus containing 180, and many of the stars as faint as the ninth magnitude. Measurable photographs of not less than eighty-four celestial objects have been secured, of which nineteen are double stars and the remainder clusters. The planets Jupiter, Mars, and Saturn, have also been photographed "with sufficient distinctness to show clearly the details of light and colour on the surfaces of the two former, and the existence of the ring in the latter," but the images have not been sufficiently sharp to allow of successful photographic enlargement.

VARIABLE STARS.—Herr Palisa in *Ast. Nach.*, No. 2,174, mentions his having remarked a new variable star, the position of which for 1877·0 is in R.A. 16h. 4m. 35s., N.P.D. 109° 48′9. It does not occur on Chacornac's chart No. 49; it was 10m. on May 26, 1876, and on July 31 and August 3 of last year, whereas on May 17, 1877, no trace of it was perceptible. The period is therefore no doubt comparatively short.

The star L. 36606 = B.A.C. 6641 appears to vary from 6·5m. to 9m. On October 17, 1852, Argelander estimated it of the former magnitude, Lalande and Piazzi call it an eighth, while about midsummer, 1851, it was little, if anything, over the ninth magnitude.

L 26211 is probably variable from 6m. to 8m., and L. 27307 from 7m. to 9m., and it is not unlikely that further observations will place b^2 Geminorum on the list of variables; it has been rated at a fifth magnitude and as low as 8½.

THE MINOR PLANET EVA.—A planet of the eleventh magnitude, observed by Herr Palisa at Pola on December 29, is mentioned in the *Bulletin International* of January 3, as possibly No. 180, but according to a communication from Herr Knorre, of Berlin, as probably identical with No. 164, detected by M. Paul Henry at Paris on July 12, 1876, which received the name *Eva*. The observations of 1876 extended over an interval of little more than a fortnight, and the elements which have been calculated by Mr. Winslow Upton and M. Bossert are therefore liable to uncertainty, but if we adopt Mr. Upton's orbit and compute for the time of the Pola observation, the place is found to be about a degree only from that observed, and it is therefore probable that No. 180 has yet to be discovered.

THOMAS VERNON WOLLASTON

THE very limited band of scientific English entomologists has just suffered a great loss by the sudden death, on the 4th instant, at his residence, 1, Barnepark Terrace, Teignmouth, of Thomas Vernon Wollaston—a name dear to science, and of which he well upheld the reputation. Accurate, elaborate, and precise *ad punctum*, and naturally of a minutely critical habit, he nevertheless persistently acted upon a broad conception of the science to which he was devoted; and taking advantage of the periodical banishments to a warmer climate imposed upon him in early manhood by pulmonary weakness, set himself the task of thoroughly investigating the coleopterous fauna of the Madeiras, Salvages, and Cape de Verdes, and finally of St. Helena. His philosophical deductions from the vast mass of well-sifted evidence obtained (chiefly by his own bodily toil, though he was always in a more or less debilitated state of health) referring to these isolated groups, may be summed up as corroborating the former existence of that submerged Atlantis whereon geologists differ. From the exhaustive care with which his material was obtained, it seems highly unlikely that his premises were insufficient; and his discussion of the subject so far resembles Mr. Darwin's method that it supplies the objections likely to be raised, and itself practically exhausts criticism by minuteness of observation.

To students of British entomology, Mr. Wollaston is best known by his early papers in the *Zoologist* and Stainton's *Entomologists' Annual and Weekly Intelligencer*, and by his revision of *Atomaria* in *Trans. Ent. Soc.*, 1877. His first scientific contribution was in the *Zoologist*, vol. i. (1843), on *Coleoptera* at Launceston, when a student at Jesus College, Cambridge (where, with the late J. F. Dawson and Hamlet Clark, he imbibed from Dr. Babington a taste for natural science), and his last, a paper in the *Annals and Magazine of Natural History*, on a weevil destructive to the banana in Madeira, was received from him by the writer almost simultaneously with the news of his death. He published many descriptive and analytical papers, almost exclusively on *Coleoptera*, in the above-named publications, the *Journal of Entomology* and the *Entomologists' Monthly Magazine*; but his *magnum opus* is the well-known "Insecta Maderensia," published in 1854, the results of his sojourns in Madeira, to which he first went in 1847. This, from its amount of novelty and classical treatment, at once established his reputation.

His collection, increased by another visit in 1855, having been purchased by the trustees of the British Museum, he prepared a more complete account, which was published as a museum "Catalogue" in 1857. Subsequent visits in 1858 and 1859 resulted in a description of the coleopterous fauna of the Canaries, also published as a museum "Catalogue" in 1864. The acquisition of fresh material compelled him in the next year to write his "Coleoptera Atlantidum," an arduous critical work of nearly 700 pages, followed in 1867 by the "Coleoptera Hesperidum," a valuable descriptive account of the species of the Cape Verde Archipelago, visited in 1866. His last contribution to geographical entomology, "Coleoptera Sanctæ-Helenæ," 1877, contains a multiplicity of unexpected developments (especially after the supposed exhaustion of the productions of the island in Mr. Melliss's work), and shows that St. Helena is the home of a special family, *Cossonidæ*, to which Mr. Wollaston had always devoted attention, having himself described no less than 255 new species in it, as against 67 described by all other naturalists, living or dead.

Of his other works, it may suffice to mention one on the "Variation of Species," published in 1856, and another, "Testacea Atlantica," that will, alas, be posthumous (though complete), being a descriptive account of the land-shells of his favourite hunting-ground.

The amount of work in these publications and in others not referred to, is astonishing, especially to those who know the extreme precision (both in manipulation and writing) and the weak physical condition of the author. Mr. Wollaston became a Fellow of the Linnean Society in 1847, and was also a Fellow of the Cambridge Philosophical Society, but, beyond his university degree, sought no other honorary distinction. He was, we believe, in his fifty-seventh year at the time of his death. E. C. R.

NOTES

WE may remind our readers that on this day, a century ago, one of the great reformers of science—perhaps the most celebrated naturalist of all times—Linné, breathed his last. His

name is too familiar to our readers to necessitate any biographical remarks on our part. His countrymen will doubtless commemorate the day in a fitting manner, and the sanctum at Upsala University, Linné's room, which is still preserved in its original state, will, we are sure, be visited by many a scientific pilgrim.

AT the last general meeting of the Royal Academy of Sciences of Brussels, the five years' prize for natural sciences was awarded to Prof. van Beneder, of Liége, the son of the celebrated zoologist of Louvain.

THE Emperor of Austria has recently awarded the large gold medal "for art and science" to the well-known African traveller, Dr. Oscar Lenz.

THE African traveller, Herr Gerhard Rohlfs, is now organising an expedition for the investigation of the eastern part of the Great Sahara. He will be accompanied by a number of scientific men, amongst others by Prof. Zittel, of Munich. Tripoli will be the head-quarters of the expedition, and its first efforts will be the exploration of the mysterious oases, Wajanga and Kufara, in the south of Anjila, which have never been visited by any European travellers.

AT Frankfort-on-the-Main a new society has been formed with the sole object of watching over the interests of chemical industry.

AMONGST the students of Strassburg University the idea has ripened to erect a monument in memory of Goethe as the most eminent representative of German culture, and as the ideal of a German student. The monument is to stand in front of the new University Building, and is to represent the poet as he appeared at the time of his sojourn at Strassburg, in the prime of youth and strength, and in the costume of that period. Most of the professors of the University regard the idea favourably, and the inhabitants of the city are confidently expected to do the same.

ANOTHER Pompeii has been accidentally discovered in the neighbourhood of Mount Gargano, near Manfredonia. There were found an ancient temple of Diana, a magnificent portico about twenty metres long, with an underground necropolis of great extent. A large number of important inscriptions has already been forwarded to, and exhibited by, the National Museum of Naples. The discovered city is the ancient Sipuntum, near Arpinum, mentioned by Strabo and Titus Livius. The houses are nearly twenty feet beneath the cultivated soil. This town was at the time ingulfed in consequence of a terrible earthquake. The Italian Government has ordered researches to be made on a large scale.

WE are glad to learn that a telegram received at Rome from Cairo announces that the Marquess Antinori had arrived at Zeyla, from which he intended to start at once for Italy. It is not known, however, as yet whether he is alone or accompanied by other members of his expedition.

MR. STANLEY has left Alexandria for England by Brindisi. He is expected to visit Rome, Marseilles, and Paris, on his way home, and speak on his work to the geographical societies of these cities. The Khedive invested Mr. Stanley with the order of the Grand Cross of the Medjidie, accompanied by another order of the next grade, thus conferring upon Mr. Stanley the title of Grand Officer of the Order of the Medjidie.

M. GAUTHIER VILLARS has published the new issue of the *Annuaire* of the Bureau des Longitudes of France, which contains a large number of geographical data. It is the first time that such a quantity of interesting numerical data has been collected in this small volume. In addition the volume contains two essays, one by Dr. Janssen on Solar Photography, and the other on Cosmical Meteorology by M. Faye. The latter denies any connection to exist between either solar spots, magnetic disturbances, or the motions of Jupiter, and the positions of the moon and variations of weather.

THE death is announced of General La Marmora, who always took a lively interest in the progress of science in Italy, and often gave his substantial aid to the establishment of practical scientific schools.

SIGNOR MENGONI, one of the greatest architects of Italy, builder of the well-known Vittorio-Emanuele Gallery at Milan, has fallen from the great arch of that building, whilst giving directions for the completion of this his life-work; he died instantly.

MESSRS. MACMILLAN have in preparation the first part of a "Course of Instruction in Zootomy," by Prof. Huxley, assisted by Mr. T. J. Parker. This part will consist of directions for the dissection of readily-obtainable examples selected from each of the classes of the vertebrata, accompanied by full descriptions of the parts displayed.

WE notice the appearance of a very interesting Russian work by M. Nemirovich-Danchenko, entitled "The Land of Cold," being a description of the author's travels in the White Sea to the coast of Russian Laponia, to Kandalaksk Bay, Novaya Zemlya, and Waigatz Island. The work has no pretensions to be scientific, but it is full of interesting and useful information on the inhabitants of the regions visited. The able descriptions are chiefly devoted to the life of the walrus- and seal-hunters, but it contains, besides lively pictures of such life, abundant statistical data as to the state of those industries, and descriptions of the varied manners in which they are carried on in different parts of Northern Russia. An important part of the work is devoted to descriptions of Samoyedes, Korels, Zyrians, Yuraks, Chukchees, Kamchadalians, Lapps, and Ural Cossacks, based on the author's own notes and other recent information. The work, extending to 520 pages, is illustrated with twenty-five full-page illustrations, and is written in the attractive style characteristic of the author, who is well known in Russia.

THE anniversary meeting of the Vienna Geographical Society was held on December 18. The Society now numbers seventy honorary, 132 corresponding, and 641 ordinary members. The Austrian Minister for Public Instruction has granted a yearly subsidy of 1,000 florins to the Society for the period of three years, and this sum, as well as other donations it has received, have enabled the council to enlarge the Society's library, which during the past year was increased by 234 new works and nineteen geographical views, as well as to facilitate materially the publication of scientific works, and to support geographical exploration. The receipts of the Society during 1877 were 7,332 florins, the expenses 7,110 florins. The President, in his report, announced that the scientific investigations made in Central Africa by Dr. Oscar Lenz and Lieut. Lux, will soon be published, and that the Austrian traveller, Dr. Emil Hollub, after a sojourn of nearly three years in South Africa, will shortly return to Austria.

PHYLLOXERA, that pernicious enemy of the vine, which hitherto had mainly restricted its devastations to the wine-growing districts of France and Switzerland, seems lately to be gaining ground in Germany as well. It is announced that it has appeared in a vineyard at Rauschwitz, near Glogau, as well as in a viticultural establishment at Plantières, near Metz. In the former case the vines had been purchased last spring from one of the numerous horticulturists of Erfurt. The necessary measures are being taken to prevent the spreading of the plague. In France phylloxera seems also on the increase; at Saint Medard and other places of the Gers Department the vines are covered by such masses of the insect that the latter can easily be seen by the naked eye, which is generally not an easy matter.

A NEW weekly serial for horticulturists has been published since January 1 at Berlin under the title *Der deutsche Garten*.

THE contract made between Alsace, Baden and Switzerland, for the protection of the fisheries in the Rhine and its tributaries, has recently come into force. The states mentioned agree to issue similar laws with regard to fisheries, and to further, in every possible way, the maintenance and increase of the valuable species of fish both in the Rhine and in the Lake of Constance. The contract has been signed for the space of ten years, and the participation of the other Rhenish states is much desired.

WE have received another volume of Brehm's "Thierleben," being the third volume on the mammals.

THE observations of shooting stars made in August last at the Royal Observatory of Brussels by M. Houzeau, and at Menin, showed according to a note in the *Bulletin* of the Belgian Academy (Nos. 9 and 10), that the number of these bodies was this year rather small, not exceeding seven per hour on August 9, and sixteen on August 11; on August 10 the sky was covered with clouds.

IN the last session of the Naturforschende Gesellschaft of Görlitz the President, Dr. Peck, made an interesting communication on a newly-discovered enemy of the carp. It appears that large numbers of the spawn of this fish are attacked by the Water-bug (*Ranatra linearis*), which fastens itself firmly on the back of its prey with its forefeet, and by means of its sharply-pointed trunk, sucks out the small amount of blood in the young organism. A series of experiments conducted in some large establishments for fish culture show that the only method of fighting this new foe is to drain the ponds dry and restock them with fish.

A CONSIGNMENT of soles and turbot was sent from the Southport Aquarium on Thursday last to America in charge of Mr. Mather, agent to Prof. Baird, United States Commissioner of Fish and Fisheries. If they arrive safely they are destined to be turned adrift in the Bay of Massachusetts. It appears that while so many members of the *Pleuronectidæ* are common enough on the American coast, soles and turbot are entirely unknown. Hence a journey to England was arranged by Prof. Baird to see if these desirable fish could not be safely transmitted across the Atlantic.

THE members of the Scientific Club will learn with regret that Mr. Logan Lobley, F.G.S., has tendered his resignation of the office of secretary to the Committee of the Club.

WE regret to record the death, on December 22, of Mr. James Whatman Bosanquet, F.R.A.S., M.R.A.S., &c., who was distinguished by his researches in biblical chronology and Assyrian history. He helped forward in many ways the investigations by Mr. George Smith, by Boscawen, and others, which have resulted in the recent famous discoveries. His valuable suggestions with reference to certain solar eclipses as bearing on the subject have frequently been acknowledged by the Astronomer-Royal and by Mr. Hind.

THE death of M. François Vincent Raspail, one of the deputies for Marseilles, is announced. The deceased deputy, who was born in 1794, achieved scientific distinction early in life, and for many years past has held a high reputation on account of his chemical researches. Notwithstanding these scientific pursuits, M. Raspail throughout his life took an ardent and active part in political affairs.

Vanity Fair is informed that the Khedive has granted to a Dutch Company the right of draining Lake Mareotis, and utilising the land reclaimed. Its area is about 75,000 acres, and the company has bound itself to hand over to the Viceroy a certain proportion of the crops raised.

WE have received the first number of the *Revue Internationale des Sciences*, which we recently announced as about to appear. There are two original papers, one by M. Balbiani, on "The Importance and Rôle of Embryogeny," and the other by Prof. von Nägeli, on "The Lower Fungi and the Decompositions which they determine." The rest of the number is mainly occupied with reports of societies.

THE *Gardener's Chronicle* learns with much pleasure that Mr. Bentham has finished the "Flora Australiensis," and that the seventh and last volume of this useful work will shortly appear. The first volume was published in 1863, so that the work has proceeded at the rate of one volume every two years. Not a very rapid rate, it is true; but still it compares favourably with the pace of other publications of the same kind. Mr. Bentham has had the advantage of Baron von Mueller's co-operation in this great work.

A DANISH agricultural journal recommends to those of its readers who wish to provide themselves every winter with a sufficient supply of ice to last during the whole of the summer the following simple means of increasing the thickness of ice during mild winters:—Long and intense cold is necessary to produce a coating of ice of more than two or three inches' thickness upon a surface of water of any considerable extent. But if a hole is made in the ice and the surface from time to time covered with a shallow layer of water, even moderately cold weather will suffice to freeze this water, and by repeating the experiment ice of ten inches or a foot in thickness is obtained without much difficulty. The Danish journal therefore proposes the use of portable pumps to be placed into the ice-holes for the purpose described.

THE apparatus used by M. Cailletet for the liquefaction of the gases was constructed by M. Ducretet, the philosophical instrument maker, and was put into operation in the laboratory of the Paris Normal School during last week, where it has been visited by a number of scientific men.

TWO shocks of earthquake were felt at Beachburg, Renfrew co., Ontario, on the morning of December 18 last, the first being between the hours of one and two, the last between five and six o'clock. The latter was so severe as to shake the houses and arouse the inmates from their beds. Beachburg is situated in the same district in the Ottawa Valley in which the earthquake of November 4 was felt most severely.

THOSE who have visited that charming watering-place, Tenby, in South Wales, will know how exceptionally rich the locality is in fossils, sea-shells, and especially in bone caves, some of which contained human remains and stone implements. Mr. Smith of Gumfreston, who has just died, is celebrated for the researches he made in the limestone caves and barrows of the neighbourhood, and his collection of bones, implements, urns, &c., is most extensive and interesting, and, on the authority of Prof. Rolleston, one of the most complete ever got together by a single individual. Through the liberality of Mr Chas. Allen and others, the whole of the money for the purchase of this collection is forthcoming, but only on condition that a suitable building shall be provided to hold it. At its last meeting the British Association made a money grant for the further examination of the Tenby bone caves, so that it is of the utmost importance to science that a good local museum should be established to prevent these most valuable specimens being scattered all over the country. Those of our readers who really wish practically to help in promoting the cause of local museums have now an opportunity of doing so by forwarding subscriptions to Charles Allen, Esq, 10, Norton Tenby, South Wales. At the same time the people of Tenby and of Pembrokeshire generally will surely have public spirit enough and a sufficiently clear perception of their own interest not to let this fine collection slip through their hands.

MR. DENIS D. REDMOND writes from Dublin in reference to Dr. Röntgen's telephone alarum, calling attention to one which he has found very effectual. He simply sends the current of an ordinary magneto-electric machine through the instrument, which produces a loud hum that is distinctly heard many yards away.

THE last number of the *Isvestia* of the Russian Geographical Society contains three letters from M. Potanin from Khobdo and Ulassutai, which, though written in January, March, and July, reached the society only in October. The winter in Khobdo was very cold; the thermometer stood in January as low as $-27°$ Cels. at noon, and even $-37°$ at seven o'clock in the morning; but the western gales brought a much warmer temperature, those of October 15, November 24, and December 24, having been the heaviest, and the last causing a rise of temperature from $-19°·8$ to $-0°·4$ Cels. There was little snow, so that the birds could easily find their food, and M. Potanin has noticed no less than fifty species (the insectivorous *Podoces hendersonii* was among them), which wintered at Khobdo. In March M. Potanin started for Hami. He crossed the eastern part of the Altai Mountains, the Altain Nuru, and soon reached the Gobi Steppe, which takes two days to cross, one night having to be passed without food for the horses and without water. On the southern frontier of the Steppe he was at the Chinese town Santaru. Thence he crossed the Mechin-ola Chain, which runs parallel to the Tian-Shan, and entered the Bukul depression. Hami was reached on May 23, and the travellers, who were kindly received by the authorities, stayed for some time. They returned thence to Ulassutai, *i.e.*, after having crossed the chain mentioned above, turned east, following a series of Sart's settlements at the northern foot of the eastern part of Tian Shan, or Karlyk-Tagh, covered with perpetual snow. At Nom-Tologoy settlement they turned north, crossed for a second time the Gobi Steppe, and afterwards the Altai ridge, and reached Ulassutai on July 25. A survey was made throughout the route, and collections of birds and plants, especially alpine, were obtained. From Ulassutai M. Potanin intended to visit the almost unknown tracts at the sources of the Yenissei, Lake Kosogol, and thence to return by way of Lake Ubsa-nor to Biysk.

THE *Allgemeine schweizerische Gesellschaft für die Gesammten Naturwissenschaften*, of Zurich, has just published the second part of its volume for 1877, which contains but one, but a very elaborate treatise, on the spiders of Switzerland. The paper occupies no less than 320 quarto pages, and is accompanied by six well-drawn plates. The author is Prof. Hermann Lebert, and his work is a most valuable addition to zoological science.

A NEW ethnographical museum is about to be erected in Paris, and is to contain everything that is of any value in relation to the science of ethnography.

THE new volume of the *Popular Science Review* commences well. The January number has several good articles, that on "The Old and the New Chemistry" being specially interesting.

IN reference to our note last week on the specimens in the Westminster Aquarium, it is the specimen of *Menobranchus lateralis* which is said to be the first shown in England.

THE additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Mr. F. Wood; a Striped Hyæna (*Hyæna striata*) from Arabia, presented by Capt. F. Cotton; a Red and Yellow Maccaw (*Ara chloroptera*) from Cartagena, presented by Capt. King; a Naked-eared Deer (*Cervus gymnopis*) from Venezuela, presented by Mr. Cyril Graham; a Robben Island Snake (*Coronella phocarum*) from South Africa, presented by Messrs. Rice and Jamrach; a Macaque Monkey (*Macacus cynomolgus*) from India, deposited.

AMERICAN SCIENCE

IN the December number of the *American Journal of Science and Art*, Mr. Holden collates various observations, by the Herschels and others, on the trifid nebula M 20, discovered by Messier, June 5, 1764, who, however, gives no details concerning it. The result of the inquiry is to show (1) that from 1784, when Sir William Herschel first described it somewhat in detail, to 1833, the remarkable triple star observed in the nebula, was centrally situated between the three nebulosities; (2) from 1839 to 1877 the triple star was not centrally situated, but involved in one of the nebulosities (A). The idea that the triple star has a large proper motion being thought improbable, it is concluded that the nebula has undergone marked changes of position, or brilliancy, or both, during the period 1784 to 1877. The conjecture was thrown out by Sir John Herschel, that "perhaps this singular object has a proper motion."

In a recent survey of the Connecticut Valley, one of the most interesting features is the discovery of a massive gravel ridge, often nearly covered by the alluvium of the highest terraces extending from Lyme, N. H., to Windsor, Vt. (twenty-four miles). It occupies nearly the middle of the valley, and resembles the gravel ridges that have been known under the various names of *kames*, in Scotland, eskers in Ireland, and asar in Sweden. The theory of the origin of the kames, commonly accepted, is that they were heaped up through the agency of marine currents, during a submergence of the land. It seemed impossible to account thus for the kames in the Connecticut and Merrimack valleys (one is found in the latter also), which, being bordered on both sides by high hills, would have been long estuaries open to the sea only at their mouths, and therefore not affected by oceanic currents. The date of their formation is known to be between the period when the ice-sheet moved over the land, and that closely following, in which the more recent and modified drift was deposited in the open valley from the floods that were supplied by the melting ice; and Mr. Warren Upham, who describes these kames, is thus led to attribute their formation to the action of the glacial rivers, which flowed in channels on the surface of the ice-sheet; the kames having been formed at or near their mouths, extending along their valleys, as fast as the ice front retreated.

Among many important discoveries made last summer by the United States Fish Commission are those of two new species of fishes, named respectively *Macrurus bairdii* and *Lycodes verrillii*. Particulars of these and of a number of other unusual forms are communicated by Messrs. Goode and Bean.

The Museum of Yale College has recently received the greater portion of the skeleton of a huge reptile which proves to be one of the most remarkable animals yet discovered. It was found by Prof. Lakes and Engineer Beckwith in upper Jurassic beds in Colorado on the eastern flank of the Rocky Mountains. The present species (*Stegosaurus armatus*) was probably thirty feet long, and moved mainly by swimming. Some of the teeth preserved have compressed crowns and are inserted in sockets (one is 112 mm. long, greatest diameter of crown, 24 mm.), others are cylindrical and are placed in rows, either in thin plates of imperfect bone or in cartillage (the latter may prove to be dermal spines). The body was protected by large bony dermal plates (one of these was over three feet in length).

Prof. Marsh also contributes a notice of some new Dinosaurian reptiles from the Jurassic formation.

The employment of chromic acid in various volumetric determinations is recommended by Mr. Hinman, who gives examples of his mode of procedure.

We learn from the *New York Tribune* that the last earthquake in the West was supposed to have radiated from a locality in Nebraska that has been popularly regarded as the site of a volcano. Prof. Samuel Aughey, of the Nebraska State University, has recently made an examination of the ground. The seat of disturbance is on the banks of the Missouri, in Dixon County, about thirty-six miles from Sioux City. A bluff, about 1,100 feet long and 160 feet high, sloping at an angle of 60° to 80° toward the river, is at present the place where the phenomena are most exhibited, but other bluffs at a few miles' distance have been similarly affected. On the bluff sounds were heard proceeding from the interior, especially on placing the ear to the ground. Flames sometimes broke forth, occasionally at night. Steam escaped from crevices. On digging into the bluff, intense heat stopped the work after proceeding a few feet. Selenite, alum, and magnesic sulphate in crystals were abundant. Prof. Aughey regards these features as not volcanic in the usual sense

of the term, but simply the result of local chemical action. The formation is cretaceous. The bluff is capped by calcic carbonate. Beneath are shales containing ferric bisulphide in crystals of pyrites. Below the shale is a soft limestone, containing carbonates of magnesia and alumina. The chemical reactions consequent upon part of the soil being soaked with water after its fall toward the river, have been the decomposition of the pyrites, the production of sulphuric acid, and the attack of the acid on the alkaline carbonates. The heat evolved in the first of these reactions is, of course, very great; in the latter part the violence must be increased by the liberation of carbonic anhydride. All the authenticated disturbances are thus easily explained. Prof. Aughey does not connect them with the earthquake.

Prof. J. L. Campbell, of Washington and Lee University, has been collating and discussing the data for the great meteor which was seen in many parts of Virginia on the afternoon of November 20. He concludes that its height was about 100 miles; but this estimate is merely approximate. Its course seems to have been 8° or 10° west of north. Its explosion appears to have taken place over the south-east corner of Halifax County, about fifteen or twenty miles a little south of west from Clarksville, 100 miles from Richmond, eighty from Lexington, and fifty-five from Raleigh. It was a meteor of unusual size and brilliancy, and detonated loudly when it exploded.

The corner-stone of a building for the accommodation of the Davenport Academy of Natural Sciences was laid on October 4, and is almost the first edifice west of Chicago intended for purely scientific purposes; the building is expected to be ready for occupation this month. The Academy is a young institution, which has grown very rapidly, and has already assumed a prominent position among establishments of this kind in the United States. This is due principally to the excellent character of its *Transactions*, filled with interesting information, and especially rich in subjects relating to American archæology. Part I of vol. ii. has been sent us.

If the descriptions are not overdrawn, a remarkably convenient small steam engine has been invented in Philadelphia. It is an oscillating engine, attached to a tank holding about two gallons of water. The boiler is of about a quart capacity; the steam-chest half that size; the whole concern occupies a space of about 10 inches square and 18 high, and weighs 35 pounds. It is designed for use with any sort of light machinery, and is said to be suitable for a variety of domestic work. The details of the contrivance are not yet stated, but assurances are given that it cannot, under any circumstances, explode; that it is as manageable as an ordinary gas burner, since the inventor has succeeded in dispensing with water and steam gauges and automatic floats, so that the whole apparatus is simple, and no skill is required to operate it. The kitchen of the future is expected to contain one of these engines, to chop hash, turn the coffee-mill and the roasting-jack, sift ashes, and mangle the family linen.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

EDINBURGH.—The matriculation returns for the past year have now been completed, and show a considerable increase in the number of students in residence over any former period. The numbers on the register for 1876 were 2,302, for 1877 they amount to 2,560. The students are divided between the several faculties as follows:—In the faculty of arts, 953 students; of theology, 67; of law, 364; and of medicine, 1,176. The ground is now being cleared for the erection of the University Extension Buildings, which, with the aid of the Government grant, will be vigorously proceeded with, and thus furnish the additional accommodation so urgently required for the increasing number of students, and for the fuller development of the teaching resources of the University.

TAUNTON COLLEGE SCHOOL.—A first-class microscope by Smith and Beck, a handsome clock and centre-piece, a purse containing 136*l.*, and addresses emblazoned on parchment, from the old boys, the parents, and the friends of the school, have been presented to the Rev. W. Tuckwell, on his leaving Taunton.

FRANCE.—M. Bardoux will propose to the French Parliament, during its present session, to organise, in each department (there are eighty-nine), a high primary school after the model of the École Turgot, one of the municipal schools of Paris. He will also introduce a bill for enlarging the Sorbonne, the traditional head-quarters of the University.

BERLIN.—The professorship of botany, which has been vacant since the death of Alexander Braun last March, is now to be filled by Prof. Eichler, of Kiel, who has accepted a call to this position as well as to the directorship of the Botanical Gardens in Berlin. He enters upon his duties next April. During the *interim* the gardens are under the direction of Prof. Koch.

GÖTTINGEN.—The present attendance at the University is 909, a slight decrease on the past summers. They are divided among the faculties as follows: theology, 86; medicine, 115; law, 275; philosophy, 433. The representation of foreign countries is unusually small, with the exception of America, which supplies a contingent of 27. The corps of instructors, numbering 116, includes 9 in theology, 26 in medicine, 14 in law, and 67 in philosophy and science.

ERLANGEN.—The University is attended at present by 448 students, a slight increase on the number of the past summer. Bavaria contributes 305, the remaining 143 coming from the other parts of Germany and from abroad. Medicine includes 110, pharmacy 56, chemistry and the natural sciences 32, mathematics and physics 10.

SCIENTIFIC SERIALS

THE current number of the *Quarterly Journal of Microscopic Science* commences with Dr. Roberts' address at the Manchester meeting of the British Medical Association on the doctrine of Contagium Vivum and its application to medicine.—Following this is Part 4 of Mr. Archer's *résumé* of recent contributions to our knowledge of "Fresh-water Rhizopoda," including the Rhizopoda, Monothalamia, Monostomata.—Prof. Carl Vogt's account of *Loxosoma* is abstracted, with notes, by Rev. T. Hinks. The genus is confirmed as a Polyzoon, and allied to *Pedicellina* Its ova and reproductive buds are described, as well as the different organs, in detail.—A paper by Prof. Arthur Boettcher treats of the results arrived at by treating red blood corpuscles with alcoholic solution of corrosive sublimate.—Dr. Klein contributes a paper on the minute anatomy of the epidermis in small-pox of sheep.—The last paper is Prof. Lankester's important notes on the embryology and classification of the animal kingdom; comprising a revision of speculations relative to the origin and significance of the germ-layers. This paper has since been separately published.

Annalen der Physik und Chemie, No. 10.—On the border angle and the expansion of liquids on solid bodies, by M. Quincke.—On the specific heat of vapours and their variations with the temperature, by M. Wiedemann.—Determination of the ratio of the specific heats for air at constant pressure and constant volume by the velocity of sound, by M. Kaiser.—On the internal friction of solid bodies (continued), by M. Schmidt.—On the doctrine of aggregate states, by M. Ritter.—Manometric method of determining the specific gravity of gases, by M. Reeknagel.—On the disaggregation of tin, by the Editor.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, Dec. 13, 1877.—"Experimental Researches on the Electric Discharge with the Chloride of Silver Battery," by Warren De la Rue, M.A., D.C.L., F.R.S., and Hugo W. Müller, Ph.D., F.R.S. Part I.

The paper in question deals mainly with the striking distance between terminals of different forms in air and in other gases at ordinary atmospheric pressures; and in air at reduced pressures short of the partial vacua of the so-called vacuum tubes.

The authors have found that the discharge of the battery, with one or two poles in the form of a point, presents several interesting phenomena which precede the true jump of the spark, and which do not occur with other forms of terminals; for example, discs or spherical surfaces. With 8,040 cells the striking distance between a paraboloidal point, positive, and a disc is about 0·34 in. (8·64 millims.), but there is always a luminous discharge. Very apparent, far beyond the distance measurable by their micrometer-discharger, namely, 1·16 inch (29·5 millims.), as they have before stated.[1]

The current which passes during the luminous discharge which

[1] *Proc. Roy. Soc.*, 1876, vol. xxiv. p. 169.

precedes the jump of the true spark is extremely feeble, in comparison with that which takes place after the spark has passed and the voltaic arc has formed ; even when the point and disc are not more distant than ·02 inch beyond the striking distance 0·34 inch for 8,040 elements, it is only $\frac{1}{2,571}$th part of it.

The appearance of the discharge is very different, according as the point is positive or negative ; it is intermittent in both cases, but is much less discontinuous when the point is negative than when it is positive, as can be seen with a microscope having a rotating minor placed in the bend of the body between the objective and eye-piece. The appearances observed are shown in the wood engravings which illustrate the paper.

Between a point and a disc the spark is longest with the point positive, when from 5,000 to 8,000 cells are used ; but for a less number of elements, 1,000 to 3,000, it is longest when the point is negative.

The length of the spark is greatly influenced by the form of the point ; thus with a point in the form of a cone of 20 degrees the striking distance is 0·184 inch with 5,640 cells, and 0·267 inches with 8,040, while with a point approaching a paraboloid in form, and with the same base and of the same height as the cone, it is 0·237 inch with 5,640 cells, and 0·343 inch with 8,040.

The striking distance between a point and a plate is in accordance, very nearly with the hypothesis of this distance, increasing in the direct ratio of the square of the number of elements, at all events up to 8,040 cells, thus[1] :—

Number of cells ...	1,000	2,000	3,000	4,000	5,000	6,000	7,000	8,000
	in.	in.	in.	in.	in.	in.	in.	in.
Distance observed ...	0·0051	0·0221	0·0554	0·103	0·159	0·222	0·286	0·352
Distance calculated...	0·0055	0·0220	0·0495	0·088	0·1375	0·198	0·2695	0·352

Between plane, spherical, or cylindrical surfaces, the striking distance does not follow this law ; on the contrary, the increase is nearly, but not quite, in the ratio of the number of cells.

	1,000 cells.	8,000 cells.
	in.	in.
Between spherical surfaces	0·0050 ...	0·0810
Plane ,, ,, ...	0·0104 ...	0·0852
Two concentric cylinders	0·0071 ...	0·0991

The striking distance between two paraboloidal points was found to be with—

1,080 cells.		8,040 cells.
in.		in.
0·005	... ...	0·401

The nature of the metal used for terminals has, in almost all cases, no influence on the length of the spark, but there is one striking exception, namely, in the case of aluminium ; when an aluminium point is used the spark is longer than with points of all other metals tried, in the ratio of 1·242 to 1.]

The length of the spark is different in various gases ; for example, air, oxygen, nitrogen, hydrogen, and carbonic acid, and the ratio between the lengths of spark in various gases varies with the forms of the terminals. The length of the spark bears no simple relation either to the density of the gas or its viscosity.[2]

The paper contains an account of a few experiments on the length of spark in air at different pressures, from 141·5 millims. to 760 millims. Between a point and a disk the length of the spark increases nearly, but not quite, in the ratio of the dilatation ; but between two spherical surfaces it increases far more rapidly, and it is possible that at a certain degree of rarefaction the striking distance may be coincident for spherical surfaces and points.

When a strong resistance is interposed in the circuit, 4,000,000 ohms for example, the discharge is completely changed in character ; instead of the ordinary spark and production of the voltaic arc, very brilliant snapping sparks pass between the terminals at more or less rapid intervals, exactly like the sparks of a small Leyden jar. Then pierce a piece of writing with minute holes.

It has been found that an accumulated charge of a condenser of 42·8 microfarads capacity, charged with the potential of 3240 cells, produced neither an elongation nor a contraction of a metallic rod 0·2 inch when suddenly discharged through. This charge deflagrates 10·5 inches of platinum wire 0·0125 inch in diameter.

More dense sparks were obtained with one of App's coils for producing 6-inch sparks when the primary was connected with 1080, 2280, 3480 chloride of silver cells, than when it was used with a zinc-carbon, bichromate of potash battery of six cells producing a current 300 times as great, thus showing the influence of high potentials in inducing secondary currents.

These currents of high potentials have also a marked effect in inducing magnetism, when the actual current is taken into account.

The second part of the paper, which is in course of preparation, will deal with the discharge in rarefied gases, in the so-called vacuum tubes.

[1] *Proc. Roy. Soc.*, 1876, vol. xxiv. p. 167.
[2] *Proc. Roy. Soc*, vol. xxvi. p. 227.

Chemical Society, December 20, 1877.—Dr. Gladstone, president, in the chair.—The following papers were read :—On the constitution of the terpenes and of camphor, by Dr. Armstrong.—Communications from the laboratory of the London Institution, by Dr. Armstrong.—On the hydrocarbons from *Pinus sylvestris*, with remarks on the constitution of the terpenes, by Dr. Tilden. The author has examined the terpenes from Russian turpentine oil and *Oleum foliorum pini sylvestris*. He considers that there are probably only three isomerides amongst the natural terpenes, and suggests a formula for these bodies derived from that of diamylene.—On citric acid as a constituent of imperfectly ripe mulberry juice, by Dr. Wright and Mr. Paterson. This juice was found to contain 26·83 grm. of citric acid and 3·26 grm. of potash salts per litre ; the authors point out that it may be valuable as an antiscorbutic, and as a substitute for lime juice.—On cuprous chloride and the absorption of carbonic oxide and hydrochloric acid gas, by J. W. Thomas. The author suggests a ready method of making a solution of cuprous chloride for gas analysis, but finds that although a solution of this salt absorbs carbonic oxide readily, sixty-three per cent. of the gas may be again liberated on neutralising the solution with potash. To avoid such an error he just neutralises the solution of cuprous chloride with ammonia and in this way prepares a solution which introduces into the absorption tube neither free ammonia nor free acid, but which absorbs carbonic oxide with facility. The author has also observed that a saturated solution of ammonic sulphate absorbs hydrochloric acid gas with great readiness, forming an acid salt and ammonic chloride.

Anthropological Institute, December 11, 1877.—Dr. John Evans, D.C.L., F.R.S., president, in the chair.—Dr. James F. N. Wise was elected a member.—Mr. Worthington Smith exhibited some objects from Maiden Bower, and a series of camera lucida drawings of several stone monuments in Wales.—Mr. A. Jukes Browne, F.G.S., exhibited a series of flint flakes, scrapers, and arrow points from Egypt, and read an interesting paper on the subject. He described the geological formation of the country round Helwan about sixteen miles south of Cairo, whence the flints were obtained, and explained the denuding action of the Nile in this locality. He thought that the finding of separate implements in each site pointed to there having been flint manufactories on those spots which, moreover, were near the hot springs. No adzes or celts were found, but fragments of horses' teeth split into long pieces were among the flints. The flints used in the manufacture of these implements were pebbles found on the lower plateau which had been washed down from the hills of eocene limestone above, the upper beds of which abound in siliceous concretions of various sizes.—Mr. Jukes Browne also exhibited some flint implements from a site on the borders of the Fens in Lincolnshire, which appeared to have been a station or manufactory similar to those at Helwan. The president and Mr. Moggeridge made some remarks on the above.—Mr. J. Park Harrison communicated a further report on the "cave-pits" at Cissbury. He said that the galleries belonging to it, and the pits adjoining, appeared to have been used as places of shelter and concealment for some considerable time after they were excavated. No evidence existed at present that they were habitations. One shaft, to which there was access from the cave-pits, was found to have been left unfinished with the horn tools lying where the work had been interrupted. Several small oval pits, the largest only 5 feet long, and 4 feet 6 inches deep, were met with this autumn for the first time in the neighbourhood of the shafts. Among their contents were sling stones and small pieces of flint and fractured rubbing-stone bearing marks of fire, fragments of pottery of various dates, a few flint implements and many flakes ; also three weights formed of chalk (similar to some found in Mr. Tindale's pit) ; a carding-comb, a small iron hook, and three pieces of burnt clay with the impress of sticks on wattles. A few bones of calf, roebuck, pig, and goat, with two or three shells, were the only animal remains. They would appear to have been preserved by the charcoal and charred matter in contact with them. If the little pits were graves

they would appear to have been used for secondary interments, or been otherwise disturbed. The absence of human bones might be due to atmospheric influence, as in many other cases of burial by inhumation. There was black mould at the bottom of all the little pits. Coarse potsherds, flint implements, and burnt pebbles, were also found in the neighbourhood of the small pits near the surface, and may possibly mark the spots where flint-workers of an earlier period were interred. A discussion followed in which several members took part.

Institution of Civil Engineers, December 18, 1877.—Annual General Meeting.—Mr. George Robert Stephenson, president, in the chair.—The numbers of the several classes of members on November 30, 1877, were:—Honorary members, 16; members, 925; associates, 1,670; and students, 448; together, 3,059, as against 2,844 at the same date last year, showing an increase at the rate of about 7½ per cent. The income proper for the year had amounted to 9,903*l.* 5*s.* 3*d.*, the life compositions and admission fees and building fund (all regarded as capital), to 2,113*l.* 13*s.*, and the dividends on trust funds to 462*l.* 16*s.* 6*l.* The general expenditure had reached 10,278*l.* 2*s.*, and the payments on account of trusts were 486*l.* 8*s.* 5*d.* The disbursements were thus 374*l.* 16*s.* 9*d.* in excess of the income. The funded property (including the cash balance) belonging to or under the control of the Institution, was now 38,773*l.* 4*s.* 11*d.*—Mr. John Frederic Bateman, F.R.S., was elected president.

Victoria (Philosophical) Institute, January 7.—Mr. C. Brooke, F.R.S., in the chair.—It was announced that exactly one hundred members had joined during the past year.—A paper on limitations in nature was read by Mr. S. R. Pattison, F.G.S.

EDINBURGH

Royal Society, December 17, 1877.—Sir William Thomson, president, in the chair.—Mr. Alexander Buchan read the report of the deputation from the Society to Upsala to assist in celebrating the four hundredth anniversary of the University of Upsala.—Mr. J. B. Hannay then read a paper on a new method of determining the cohesion of liquids by the size of its normal drop, which he considered was that obtained by allowing the drops to succeed one another as rapidly as possible. He found that the weight of the drop of liquid dropping from a column of the same liquid increases at the rate at which the drops follow one another. This, he thought, was due (1) to the fact that the rate of flow of liquid through the neck of the drop was faster when drops succeeded rapidly, and (2) because the flow lasted for a longer time. He found also that cohesion decreases with rise of temperature, but rather quicker than the density.

PARIS

Academy of Sciences, December 31, 1877.—M. Peligot in the chair.—M. Faye presented the *Annuaire du Bureau des Longitudes* for 1878.—The following papers were read:—On the constitution of the solar surface and on photography regarded as a means of discovery in physical astronomy, by M. Janssen. Photography has two advantages over optical observation. If the time of exposure be accurately determined, so as to prevent superposition, or what may be called *photographic irradiation*, the true relations of luminous intensity of the object are expressed. Further, when the luminous action is very short the photographic spectrum is reduced to a narrow band near G; thus very tolerable photographic images of the sun may be had with simple lenses of long focus, and chemical achromatism is much more easily realised than optical. M. Janssen has so arranged that the time of luminous action can be reduced to $\frac{1}{3000}$ of a second in summer. The images are more latent and require slow development, &c. But they throw new light, especially on the solar granulations, which are found more or less of spherical form; the irregular grains are made up of small spherical elements. The state resembles that of our clouds. These spherical elements and their distribution probably result from a breaking up by gaseous currents. The luminous power of the sun, then, resides chiefly in a small number of points of its surface, and the spots are not the principal element of the variations that star undergoes.—Constitution and brecciform structure of the meteoric iron of Santa Catharina (Brazil); deductions from its characters, concerning the history of meteoritic rocks, and especially the habitual association of carbon with sulphide of iron, by M. Daubrée. The association referred to may be explained by the action of sulphide of carbon on iron. If an iron bar be thus treated at a red temperature, it gets coated with a crystalline substance which has the characters of pyrrhotine.—On the order of appearance of the first vessels in shoots of *Fœniculum vulgare* and *dulce*, by M. Trécul.—Note on waves and eddies of various kinds in a canal whose current is alternately intercepted and renewed, and in which the depth can be varied, by M. de Caligny.—On the condensation of gases supposed incoercible, by M. Cailletet. Pure dry nitrogen, compressed to 200 atmospheres at + 13°, then suddenly expanded, condenses distinctly in small droplets; The liquid retires from the walls to the centre. Pure hydrogen compressed to 280 atmospheres and expanded, gave momentarily, a very fine mist. Air was also liquefied by a direct experiment. M. Berthelot corroborated M. Cailletet's account.—M. De Lesseps announced that the *personnel* of the first scientific and hospital station of the International African Association had reached Zanzibar. They had met Stanley and got useful advice from him.—On a storm which occurred over the south part of the Suez Canal on the night of October 23-24. In a few hours an artificial lake of about five million cubic metres was formed on the west side of the canal by the rains.—Kinematics and dynamics of current waves on a liquid spheroid; application to the evolution of the elliptic protuberance about a spheroid deformed by attraction of a distant star, by M. Guyon.—On a new experiment on liquefaction of oxygen, by M. Pictet. The oxygen jet in the electric light showed a white central part (of liquid or even solid elements) and an exterior blue part, indicating return to the gaseous state.—On a note by M. Boussinesq on conditions with limits in the problem of elastic plates, by M. Levy.—On a theorem of M. Villarceau; remarks and consequences, by M. Gilbert.—On a new kind of bird of nocturnal prey from Madagascar, by M. Milne-Edwards. This belongs to the same zoological type as the white owls, but has osteological peculiarities.—The peripheric organs of the sense of space, by M. Cyon. Having shown that there are intimate relations between the semicircular canals and the centres of innervation of the muscles of the eye, he considers that sensations caused by excitation (through the otoliths) of the nerve terminations in the ampullæ of these canals, through movements of the head, serve to form our notions of the three dimensions of space.—On the evolution of red corpuscles in the blood of superior animals, viviparous vertebrates, by M. Hayem. The red corpuscles are developed from small, colourless, delicate, very alterable elements termed *hæmatoblasts*.—Experiments proving that there is during life a figured ferment in typhoid human blood, by M. Feltz.—On the cause of spontaneous alteration of eggs; reply to a reclamation of M. Gayon, by MM. Bechamp and Eustache.

CONTENTS

THURSDAY, JANUARY 17, 1878

THE DENSITY OF LIQUID OXYGEN

THE magnificent experimental methods devised by MM. Cailletet and Pictet have already begun to increase the number of the "Constants of Nature." M. Pictet, although in a neck-and-neck race he was beaten by Cailletet in the liquefaction of hydrogen, has left his competitor in the rear with regard to a result of the first importance on the density of oxygen. The noble rivalry between the École Normale Supérieure of Paris and the Atelier de Physique of Geneva bids fair not only to continually increase in interest, but to become the central feature in the progress of physical science for some time.

A telegram from M. Pictet announcing that hydrogen had been solidified was sent to M. Dumas on January 11. The illustrious chemist read the telegram at a sitting of the Société d'Encouragement, of which he was the chairman, and which was holding its regular semi-monthly meeting on that very evening. M. Dumas reminded his hearers with his wonted force and propriety of expression, that in the first edition of his "Traité de Chimie," published about forty years ago, he had called hydrogen a *gaseous metal*. He said he had been led to hold this view by seeing how small was the affinity of hydrogen for metals and how great for metalloids.

M. Dumas said moreover that his peculiar ideas had received some degree of confirmation from the discovery of the large conductibility of hydrogen for heat and electricity, but that the first real demonstration had been given by MM. Cailletet and Pictet. It was for him a great satisfaction having lived long enough to see that most important fact established so clearly, "*That you may feel certain, gentlemen, that in drinking a glass of water you are certainly absorbing a metallic oxide.*"

M. Pictet, in the experiments, the results of which were telegraphed to M. Dumas, as we have seen, prepared the hydrogen by the decomposition of potassic formiate by means of potassic hydrate. This reaction, according to Berthelot, gives the gas of the utmost purity. The pressure was commenced at 8.30 P.M., it was increased gradually, and in a little more than half an hour (at 9.7) it reached 650 atmospheres. At this moment the pressure remained stationary for some seconds, the stop-cock was opened, and a jet of a *steel blue colour* escaped with a strident noise, comparable to that heard when a bar of iron is plunged into water.

This jet suddenly became intermittent, and the spectators observed a hail of solid corpuscles projected with violence on the ground, where they produced a crackling noise. The stop-cock was then again closed, the manometer indicating 370 atmospheres. This slowly descended to 320, at which point it remained stationary for some minutes. Then it rose to 325. The stop-cock was again opened, the jet was now so intermittent, that it was believed that an actual crystallisation of hydrogen (!) had gone on inside the tube. This was proved by the fact that liquid hydrogen flowed out of the jet when the temperature was increased by the stoppage of the pumps.

M. Dumas, considering oxygen as belonging to the sulphur group, and isomorphous bodies as having the same atomic volume, *i.e.*, the quotient obtained when the atomic weight is divided by the density, had concluded that, the atomic volume of sulphur being $\frac{32}{2}$, that of oxygen would be $\frac{16}{1}$, and reciprocally, that the density of liquid or solid oxygen would be $\frac{16}{16}$, that is the atomic volume divided by the atomic weight $= 1$, which is the density of water.

M. Dumas having communicated these considerations to M. Pictet, has elicited a most interesting response from him. He writes :—

"You arrive at the expression of the density of liquid oxygen as being represented by $\frac{16}{16} = 1 = \delta$ in the solid state, and probably the liquid one also, neglecting the variation due to expansion.

"I have the great satisfaction of being able to announce to you the complete experimental demonstration of the theoretical views enunciated by you now some time ago at Geneva. This demonstration has been arrived at as follows :—

"I know directly and very exactly—

"I. The exact volume of the interior of the wrought iron shell and the volume of potassic chlorate decomposed into oxygen and potassic chloride.

"II. The temperature of the shell at the moment of complete decomposition.

"III. The volume of the tube in which the condensation of oxygen is brought about.

"IV. The pressure before and after condensation.

"V. The pressures indicated by the manometer after two or three successive jets, till the moment the point of saturation is reached, and after which the gas issues in a gaseous form.

"These various data, combined with the gaseous density pressure and temperature lead me to the conclusion that a difference of 74·26 atmospheres on the manometer represents the variation of pressure corresponding to the condensation of oxygen in the tube immersed in the carbonic acid.

"This variation has been exactly observed in the three last experiments which I have made with the assistance of many of my colleagues here at Geneva.

"The quantity of liquid oxygen which we had in the tube was 45·467 grammes, corresponding to a volume of 46·25 cubic centimetres. But it is possible that the highest part of the thin tube had some centimetres in length not occupied by the liquid. This may explain the difference of 0·8 gramme found.

"Moreover, very volatile liquids have such considerable expansions that it is indispensable to have exactly the temperature to which they are subjected, in order to determine their true density. However this may be, there is an absolute verification, within small limits, of error of the theoretical calculation regarding this physical constant."

In addition to this important result, in another experiment, M. Pictet has used polarised light to determine the presence or absence of solid particles of oxygen in the jet. The jet was illuminated by means of the electric light, and observed with two Nicol prisms. A very strong polarisation was observed, indicating the presence of solid particles, which in all probability were really solid particles of oxygen.

FRANKLAND'S RESEARCHES IN CHEMISTRY

Experimental Researches in Pure, Applied, and Physical Chemistry. By E. Frankland, Ph.D., D.C.L., F.R.S. &c. (London : John Van Voorst, Paternoster Row.)

THE numerous and valuable investigations of Dr. Frankland in general chemistry are so well known, that chemists will doubtless regard the issue of his collected researches with lively satisfaction, partly on account of the ease with which the various memoirs can be referred to in the fine volume before us, but chiefly because the work is likely to prove of special value as an aid in the higher education of chemical students.

Any criticisms of the statements of fact or of theory contained in such a set of "collected researches" would be so clearly out of place in NATURE, that we need offer no apology for dealing with the work before us from a general rather than from a technical point of view. Indeed, almost all the *matter* contained in the volume has long been the common property of all engaged in the pursuit of chemistry, while the *manner* in which the investigations are presented to the reader is alone new. The chief interest of the work as a whole is due to the fortunate circumstance that its varied contents have been grouped by the distinguished author of the researches, who has bound them together with a species of commentary that enables the reader clearly to appreciate the relations of the parts in each line of inquiry, and to obtain such glimpses into the working of the mind of the investigator as the study of formal papers can rarely afford.

The subject-matter of this fine volume of rather more than 1,000 pages is conveniently divided into three sections. Section I. contains the author's researches in Pure Chemistry ; Section II., those in Applied Chemistry ; and Section III., the investigations that belong to the physical side of the science.

Section I. is fitly introduced by a chapter on the peculiar system of notation now employed by Dr. Frankland. This introduction was rendered necessary by the translation of the older formulæ employed in the earlier memoirs into those more recently adopted by the author. Although Dr. Frankland's system of notation is undoubtedly interesting, we fear that its use throughout the volume will detract from the educational value of the work in the eyes of those chemists who think that the expressions in common use can be made to serve the same purposes as those employed in the South Kensington School.

As the work stands, however, the chapter in question is useful in its place, and it may induce some chemists to adopt the author's system who have hitherto held aloof from it.

The first of the series of researches given is that on the transformation of cyanogen into oxatyl. This well-known inquiry was carried on in conjunction with Dr. Kolbe at a time when the investigation of the then recognised "compound radicles" had commenced to excite much interest, more especially in view of Liebig's recently propounded theory of conjugated compounds. The investigation led to the highly important conclusions that most of the organic acids owe their acidity to the presence of the group COOH (the semi-molecule of oxatyl), and that their basicity depends on the number of these groups contained within their molecules ; while it was shown that the synthesis of many acids of the acetic series could be effected by the conversion of the cyanogen of alcoholic cyanides into the oxatyl semi-molecule by the action of alkalies. This research has since borne rich fruit, and it seems to have led, almost directly, to the most important of the author's discoveries, namely, to the isolation of the alcohol radicles by the action of zinc on iodides of radicles containing half the number of atoms of carbon. Although this research was one of the most important contributions to synthetic chemistry that had then been made, its full value was not understood till M. Wurtz completed Dr. Frankland's work by the discovery of methyl-ethyl, and other mixed radicles, which he prepared by the action of zinc on mixtures of alcoholic iodides, thus filling up the gaps in Frankland's list, and rendering the method a general one for ascending the homologous series.

In the course of experiments on the action of zinc on the iodides of alcohol radicles, Frankland made the remarkable discovery that the metal can unite directly with the alcohol radicles and form the curious and interesting compounds now termed "organo-metallic," of which zinc-methyl and zinc-ethyl are those most commonly known. The author says "zinc-methyl and zinc-ethyl were the first of these bodies with which I became acquainted ; they were discovered on July 12, 1849, in the laboratory of Prof. Bunsen at Marburg, during my work on the isolation of the organic radicles. After making the reaction for the isolation of methyl by digesting methylic iodide with zinc, and after discharging the gases, I cut off the upper part of the tube in order to try the action of water upon the solid residue. On pouring a few drops of water on this residue a greenish blue flame several feet long shot out of the tube, causing great excitement amongst those present. Prof. Bunsen, who had suffered from arsenical poisoning during his researches on cacodyl, suggested that the spontaneously inflammable body, which diffused an abominable odour through the laboratory, was that terrible compound which might have been formed by arsenic present as an impurity in the zinc used in the reaction, and that I might be already irrecoverably poisoned. These forebodings were, however, quelled in a few minutes by an examination of the black stain left upon porcelain by the flame ; nevertheless, I did afterwards experience some symptoms of zinc poisoning."

The discovery of the large group of organo-metallic bodies and the secondary investigations to which the author was thereby led induced him to propound the theory of "atomicity," now taught in one form or another in our schools of chemistry.

Having discovered the organo-metallic bodies just referred to, Dr. Frankland appears to have turned his attention to the production of analogous compounds containing *unmetallic* bodies united directly with alcohol radicles, and in this direction he was successful, as he showed that boron could be made to afford some highly interesting compounds of the desired kind. This line of investigation, however, was not pursued to any considerable extent, as the author evidently desired to concentrate his attention upon the study of the action of members of the organo-metallic group upon various organic bodies,

hoping thereby to succeed in replacing the oxygen of many oxygenated compounds by alcohol radicles derived from zinc-methyl, zinc-ethyl, and allied bodies. In the primary research of this new series, nearly all of which were conducted in conjunction with the late Mr. B. F. Duppa, ethylic oxalate was the subject of experiment, with the result that a portion of its oxygen was replaced by methyl, and the first step taken in the synthesis of acids of the lactic series. A large number of new compounds were discovered, and the relations of the members of the lactic series of acids to each other and to the acrylic and to the fatty group of acids clearly made out. Then followed researches on the members of the acrylic series which were suggested by those on the lactic acids and which also afforded a rich harvest of results.

Up to this point Dr. Frankland's investigations are seen to have been intimately connected with one another and to have resulted in some of the most valuable contributions yet made to synthetical chemistry; but the last research of importance included in this section of the volume seems to stand alone, for it is concerned with the synthesis of acids, ethers, and ketones of the fatty series by a method differing from that previously employed in the important particular that the alcohol radicles were substituted for hydrogen and not for oxygen. These new investigations resulted in the discovery of a mode of effecting the synthesis of the acids of the fatty series and of bodies related to them, of dissecting their molecules, and thus, in some measure, of determining their structure. Although these researches were not directly connected with those that preceded them, there can be scarcely a doubt that they were suggested by the insight into the constitution of the acids gained in the course of Dr. Frankland's previous researches.

A few short papers—on Gas Analysis, on the Composition of Air from Mont Blanc, on the Analysis of Organic Compounds containing Mercury, and on the Combustion of Iron in Compressed Oxygen—bring Section I. to a close.

Section II. contains the author's researches on Artificial Light, on Drinking Water, on the Purification of Foul Water; together with miscellaneous work in Applied Chemistry. Section III. includes Dr. Frankland's valuable memoirs on the Influence of Atmospheric Pressure on Combustion, on the Spectra of Gases of Vapours (an investigation carried out in conjunction with Mr. Lockyer), on the Source of Muscular Power, and on Climate.

The contents of these two sections are much too interesting to be lightly passed over—and those of Section II. in some degree challenge criticism—but we must leave them for consideration in another article and now return to Section I. This section forms just half the book, and by far the most important half. In fact Dr. Frankland's work is so naturally divisible into two parts that we regret he has not issued it in two volumes rather than in its present form, for its value as a work of reference would not have been lessened thereby, while the section of chief educational importance (Section I.) would have been rendered more easily accessible to students. This is, however, but a trifling fault—if a fault it happens to be—but the really important fact remains that we can point students to the volume before us for a clear and detailed account of some of the most remarkable researches of our time in synthetic chemistry. It is difficult to over-estimate the importance of inducing senior students to consult original memoirs rather than abstracts of researches. The temptation to rest content with a statement of results is great, but we have no hesitation in expressing the opinion that the careful experimental study of a single good memoir, on a subject suited to the capacity of the student, is of far greater value to him than the immediate knowledge of the contents of a volume of the "Abstracts" given in the *Journal* of the Chemical Society, useful though these are when properly employed. The publication of such groups of researches as Dr. Frankland's will, we believe, do much to promote the kind of higher chemical education referred to, and to foster a taste for research amongst senior students of chemistry.

J. EMERSON REYNOLDS

(*To be continued.*)

OUR BOOK SHELF

Bericht über die Thätigkeit der botanischen Section der schlesischen Gesellschaft im Jahre 1876. Erstattet von Prof. Dr. Ferdinand Cohn, zeitigem Secretair der Section.

THIS is a journal of the proceedings of the ten ordinary and one extraordinary meetings of the Silesian Society held during the year 1876. The chief contributors are Professors Goeppert and Cohn, and their communications relate to a great variety of subjects. The most important paper of Goeppert's is on the effects of the cold of December, 1875, on the vegetation in the Breslau Botanic Garden, much interesting information being given on the action of cold on plants, the effects of snow in protecting vegetation, and the action of frost on roots. Another interesting paper, by the same author, is on Plant Metamorphoses. The indefatigable industry of Prof. Cohn is well shown in this journal, as he contributes a large number of valuable papers. His recent visit to Britain affords materials for two papers, while a short communication on Spontaneous Generation is interesting on account of the ingenious form of the tube in which the experiments were made, the shape being that of a capital N turned upside down. The other papers of interest are chiefly connected with the newly-published "Cryptogamic Flora of Silesia," noticed a short time since in our columns. The last paper is by Uechtritz on the Phanerogams of the Silesian Flora, and occupies a large part of the whole *Bericht.*

A List of Writings Relating to the Method of Least Squares, with Historical and Critical Notes. By Mansfield Merriman, Ph.D. (From the *Transactions* of the Connecticut Academy, vol. iv., 1877, pp. 151–232.)

MR. MERRIMAN is already favourably known as the author of a good text-book on the "Elements of the Method of Least Squares." In this work he gave a short "list of literature," and said he could easily have extended its limits; indeed he hoped some time to publish an extended list. All students of this branch must be greatly indebted to Mr. Merriman and to the Connecticut Academy for this excellent critical list of writers. There are 408 titles, classified as 313 memoirs, 72 books, and 23 parts of books, dating from Cotes (1722) down to 1876. Of these 408, 312 are described from actual inspection. We could wish for similar lists in other branches, for then much time would be saved and students could easily determine what books would be most advantageous to them, and also get an idea of what had already been done by previous investigators. There are numerous clerical errors, easily to be corrected, but we are surprised that so well-informed and painstaking a writer should call Sir W. Thomson, Thompson, and Dedekind, Dedakind, as he does on all occasions when their names occur.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

The Radiometer and its Lessons

I AM sorry to have again to correct Mr. Stoney; but I cannot allow the statements contained in his letter to pass unnoticed.

1. There is nothing in my earlier paper that is "*admittedly erroneous.*" If there is error in these papers I am not aware of it.

2. These papers do not "conclude with Prof. Reynolds's own expression of opinion that residual gas is not the cause of the force observed by Mr. Crookes." Nor have I ever held or anywhere expressed such an opinion.

3. In the passage to which Mr. Stoney refers, Clausius does not imply that the law established by himself and Maxwell, viz., that the only condition of thermal equilibrium in a gas is that of uniform temperature, depends on the mean path of the molecules; and it was this law that I instanced as being at variance with Mr. Stoney's assumptions (1) that gas is a perfect non-conductor of heat; (2) that a layer of gas across which the temperature varies can exist in a state of thermal equilibrium without the passage of heat from the hotter to the colder part. Mr. Stoney has nowhere that I can see given any proof of these assumptions, and I venture to prefer the authority of Professors Maxwell and Clausius, supported as it is by the whole evidence of facts.

4. Mr. Stoney says that I have excluded the polarisation of gas from my explanation. Mr. Stoney has not, that I am aware, defined what he means by polarisation, but if he measures the polarisation of a gas conducting heat by the excess of momentum carried across any ideal surface in one direction over and above that which is carried in the opposite, this polarisation is independent of the length of the mean path, and forms an essential part of my explanation.

There is one statement in Mr. Stoney's letter which is not erroneous. He says:—"I cannot find anywhere in Prof. Osborne Reynolds's writings an explanation of the thing to be explained, viz., that the stress in a Crookes's layer is different in one direction from what it is at right angles to that direction."

I do not at all admit that this is "the thing to be explained," and I am quite sure that Mr. Stoney would find no explanation of it in my writings.

In the passage quoted above Mr. Stoney has, for the first time, so far as I know, expressly stated his belief that Mr. Crookes's phenomena depend on such a difference of stress. I have thought all along that his views were based on such an assumption, but I did not like to take it for granted. It is almost a pity, if I may use the phrase, that he did not express himself thus clearly at first, as in that case I might have done before what I am about to do now, viz., prove definitely that such a condition of stress can have nothing to do with the cause of Mr. Crookes's results—that, so far from explaining, such a condition of stress is inconsistent with, these results, and this, not in mere matters of detail, but as regards the fundamental direction in which the force acts.

Throughout all the experiments that have been made one invariable law as to the direction of motion has been found to maintain, which is that the force always tends to drive the vanes or bodies in the direction of their colder faces. Thus when a body is free to move in a sufficiently rarefied medium, if its front be heated it will move backward, while if its front be cooled it will move forward, always moving towards its colder face. There are no exceptions to this rule.

Let us now suppose that we have two bodies, A and B, free to move in a sufficiently rarefied medium. Suppose A to be initially hot and B cold, while the medium and surrounding surface are at the mean temperature of A and B. Then, owing to the radiation of heat between the two bodies, that side of A which is opposite to B will be cooled faster, and hence be colder than the other side of A. Hence according to the law stated above, A must move towards B, and this it is found to do by experiment. On the other hand, that side of B which is opposite to A will become heated by radiation faster, and hence become hotter, than the other side of B, and hence B will move away from A. Thus if both bodies were free to move, we should have B running away from A, and A running after B.

This aspect of the phenomenon is perhaps the most paradoxical that presents itself; it is nevertheless in strict accordance with experiment, and it was by instancing this case that I was enabled to show that the force could not by any possibility be directly due to radiation (see *Phil. Trans.*, vol. 166, p. 728).

The same reasoning now enables me to show, just as conclusively, that the force which causes the motion in the bodies cannot be due to the stress, in the layer of gas which separates the bodies, being greater in the direction joining the bodies than it is at right angles to this direction. For the only effect of such a difference in the stress would be to cause the bodies to separate; therefore, instead of A following B, it would be forced back in the direction of its hottest side, or in a direction opposite to that in which it is found experimentally to move.

This case, therefore, shows the fundamental error of Mr. Stoney's view. Although he allows that the intervening gas is the medium of communication, he assumes, none the less, that the force acts directly between the two bodies (the heater and cooler), in which case action and reaction must be equal between the two bodies. Experiment, on the other hand, shows conclusively that the force acts independently between each body and the gas which surrounds it; the pressure being always greatest on the hottest side. The force which acts on the body reacts on the gas, causing it to move in the opposite direction, and the wind thus caused tends to carry all opposing obstacles with it. Hence, in the case above, the motion given to the air at the one body must to some extent affect the opposing surface, but this surface forms only one obstacle, while the action of the wind is distributed throughout the entire chamber, in which it acts in the manner so beautifully shown by Dr. Schuster's plan of suspending the vessel. A simple analogy to what happens in the case of A and B is furnished by two steamboats, the one following the other. The water thrown back by the screw of the first would stop the second, but only to a small extent.

When answering Prof. Foster in a former letter, I said "that it is contrary to the kinetic theory that the increase resulting from rarefaction in the mean path of the gaseous molecules should favour the action." In making this statement all I meant to imply was that the action was independent of any relation between the mean path and the distance of the hot surface from the cold surface, which was the only point in question. Although my statement was strictly true in this sense, it appears to me, on further consideration, that it might include more than I intended.

I hope that nothing I have said, either in my earlier papers or in this controversy, has led any one to suppose that I regarded my explanation as entirely complete. I suggested, and to some extent established, the true source of the force, namely the heat communicated to the residual gas, and although now my suggestion appears to have been universally accepted, it may be remembered that at the time my first paper was written the only other suggestions as to the cause of the motion observed by Mr. Crookes were of a widely different character. As regards the working out of the detail of my explanation, there has been one point which I could not quite see through, viz., the influence which the hot molecules receding from the surface might have on the rate at which the cold ones would come up, and although I have been trying to satisfy myself on this point ever since my first paper was published, it is only within the last three months that I succeeded.

Now, however, I have arrived at a result which, although somewhat unexpected and striking, will, I hope, be found to reconcile what has hitherto appeared to be anomalous in the phenomena already known, and to have suggested certain hitherto unexpected phenomena which now only await experimental verification.

OSBORNE REYNOLDS

January 15

Sun-spots and Terrestrial Magnetism

PRECISELY because the article (NATURE, vol. xvii. p. 183) on "The Sun's Magnetic Action at the Present Time," is by so able a mathematical physicist as Mr. John Allan Broun, and because of all sides of the solar problem there is none wherein he is so *facile princeps* as the magnetic, I venture to think this a good opportunity for asking a question which has troubled me much of late, and which is this:—

The sun-spot cycle and the terrestrial magnetic diurnal oscillation cycle are looked on now generally as being, if not actual cause and effect, at least as equally both of them effects of one and the same cause, and necessarily, therefore, synchronous. Yet if we inquire of the sun-spot observers the length of their cycle, they declare it (as see Prof. Rudolph Wolf's admirable

and exhaustive paper in the last volume of the *Memoirs* of the Royal Astronomical Society) to be 11·111 years. While if we ask the magnetic men the length of the cycle of their needle manifestations, they (as in Mr. Allan Broun's first paragraph on p. 183) declare it as confidently to be 10·5 years.

Wherefore I would request to be kindly informed if the maxima of the two cycles do approximately agree just now, where will they be, relatively to each other, after a dozen cycles hence? And the answer may or may not assist in clearing up certain apparent anomalies in the Edinburgh earth-thermometer observations. PIAZZI SMYTH

15, Royal Terrace, Edinburgh, January 11

On the Insects of Chili and New Zealand

IN Mr. McLachlan's note "On Some Peculiar Points in the Insect Fauna of Chili" (NATURE, vol. xvii. p. 162), I see, with surprise, the remark that "the large islands of New Zealand furnish us with no indication whatever of forms parallel with those found in Chili," for it is well known that many Lepidoptera belonging to European genera do occur in New Zealand, although, perhaps, neither *Argynnis* or *Colias*. Amongst a small number of Lepidoptera from New Zealand which lately came into my hands, I notice species of the following European genera:—*Sesia*, *Cloantha*, *Nonagria*, *Heliothis*, *Hybernia*, *Larentia*, *Fidonia*, *Cidaria*, *Coremia*, *Camptogramma*, *Asthena*, *Acidalia*, *Scoparia*. Except in the case of *Sesia tipuliforme*, it is not probable man has had any hand in the introduction of them. None, except the *Sesia*, are identical with European species, although several approximate, and the causes which have led to the existence of *Agynnis* and *Colias*, in Chili, are probably the same as those which have planted the insects I have named in New Zealand.

In Mr. Darwin's "Origin of Species," Chapter XII., we find a suggested Explanation of the Presence of the Forms of the Northern Temperate Zone in South America and New Zealand in the occurrence of alternate glacial epochs at the North and South Poles, and although the observations especially refer to plants, they are applicable to the insects which would, doubtless, accompany them in their supposed migrations. Perhaps it is not an entirely satisfactory explanation, and with his usual candour, Mr. Darwin admits that it does not meet all difficulties. In describing the wanderings of the plants, Mr. Darwin uses terms (figurative of course) which endow them with extraordinary if not voluntary powers of locomotion, as, indeed, they would seem to require in reality, for effecting such wonderful migrations, and as regards insects Mr. McLachlan goes further, and suggests that some of them "mistook the points of the compass and went southward."

Now the pertinacity with which the Lepidoptera adhere to particular plants and stations, and prefer death to change of either, is a much more noticeable character than their ability to emigrate, and seems to me a serious bar to the acceptance of a theory involving great changes of food and a double journey across the equator; possibly some of the polyphagous species might survive it, but even these, according to Mr. McLachlan, appear to have got a little muddled in their reckoning. Most of the insects I have named are eminently select in their diet, and how are we even to conceive of the wingless female of Hybernia performing the vast journey?

I do not know that we have evidence that change of climate induces migration of the Lepidoptera. There is a large colony of *Bryophila perla*, which has been stationed on an old wall here for the last twenty years, and although there are miles of similar lichen-covered walls in the neighbourhood, I have never seen a specimen fifty yards from head-quarters, and even under the threat of a new glacial epoch, I do not think it would consent to move on.

In saying there are no indications of similar forms on the northern portions of the Andes, I am not sure whether Mr. McLachlan refers to Lepidoptera or Trichoptera, so I will mention that I have received several species of *Colias* captured on the eastern Cordillera of New Granada. The genus probably ranges through the whole chain of the Andes.

Douglas, Isle of Man, January 2 EDWIN BIRCHALL

Macrosilia cluentius

IN NATURE (vol. viii. p. 223) I have spoken of a *Sphinx* which, with its proboscis of 0·25 metre length, would be capable of obtaining nearly all the nectar of *Anagræcum sesquipedale.* Lately my brother, Fritz Müller (Itajahy, Prov. St. Catharina, Brazil), sent me the wings of another specimen of the same species, and Dr. Staudinger, of Dresden, stated by comparison of these wings with the Sphingidæ of his collection that the name of the species is *Macrosilia cluentius*, Cramer.

Lippstadt, January 9 HERMANN MÜLLER

Meteor

I TAKE the liberty of forwarding the following particulars relative to a meteor which I saw on Sunday last at 4h. 24m. P.M., that is to say, about twenty minutes after sunset. As, however, the day had been very fine, there was not only full daylight in the west, but only a trace of twilight in the north-west direction, in which I saw the meteor. I may add that the sky was slightly overcast by watery clouds in that direction:—

Point from which seen, Salthill, near Kingstown; direction in which seen, north-west; elevation above horizon, 10° to 15°; length of luminous "tail," 5° to 6°; inclination from vertical, about (towards south) 10°; time, 4h. 24m. P.M.; colour of tail and of globe of explosion, light blue.

Judging from the elevation and from the fact of its being visible notwithstanding the strong twilight and the interposed clouds, I conclude that this meteor must have been remarkably brilliant and that it exploded over or beyond the West Coast of Ireland. It is for these reasons that I take the liberty of calling attention to it, as others may have seen it under more favourable conditions. P. W. REILLY

Royal College of Science for Ireland,
Stephen's Green, Dublin, January 15

Philadelphia Diplomas

IN NATURE, vol. xvii. p. 183, there appears a note by Dr. C. M. Ingleby on the "Philadelphia Diplomas." Permit me to say that the only institutions in Philadelphia legally authorised to grant medical diplomas are the University of Pennsylvania, a school which has long ago celebrated its centenary, and the Jefferson Medical College. The so-called University of Philadelphia is a hybrid concern, the medical department of which is under the management of the Eclectic Medical School.

January 10 RICHD. C. BRANDEIS

Great Waterfalls

I SHALL be much obliged if you, or any of your readers, can inform me in what book I can find accounts of any of the following great waterfalls:—The Tequendama Fall, near Sta. Fé de Bogota, South America; the Cauvery Falls, near Seringapatam, India; the Alatau Falls, Alatau Mountains, Central Asia; the Guava, or Guayra Falls, on the Alto Parana, South Brazil; Falls of the Rio Grande, near Guadalajara, Mexico. These great falls, five of the most remarkable in the world, are shortly noticed in books of geography, but I have hitherto been unable to obtain any detailed particulars or description of them.

Eltham, January 7 ARTHUR G. GUILLEMARD

BIOLOGICAL NOTES

SELF-FERTILISATION OF PLANTS.—This subject, around which the genius of Mr. Charles Darwin has thrown a halo, seems likely to give rise to further controversy. The Rev. G. Henslow, in a communication laid before the first meeting this session of the Linnean Society, gave an exposition of the views he had arrived at; these in many respects being at variance with those promulgated by Mr. Darwin. The author acknowledged how indebted he stood towards the latter, whose vast storehouse of facts and close reasoning necessitated constant reference to his writings; but the author's own deductions therefrom, and additional researches, nevertheless, confirmed him in hesitating to accept some of Mr. Darwin's conclusions. According to Mr. Henslow, the chief facts and bearings of the self-fertilisation of plants may thus be summarised: 1. The majority of flowering plants are self-fertile. 2. Very few are known to be physiologically self-sterile. 3. Many are morphologically self-sterile. 4. Self-sterile plants become self-fertile by (*a*) withering of

the corolla, (*b*) its excision, (*c*) loss of colour, (*d*) closing, (*e*) not opening, (*f*) absence of insects, (*g*) reduction of temperature, (*h*) transportation. 5. Highly self-fertile forms may arise under cultivation. 6. Special adaptations occur for self-fertilisation. 7. Inconspicuous flowers are highly self-fertile. 8. Cleistogamous flowers are always self-fertilised. 9. Conservation of energy in reduction of pollen. 10. Relative fertility may equal or surpass that of crossed plants. 11. It does not decrease in successive generations. 12. It may increase. 13. Free from competition self-fertilised plants equal the intercrossed; (*a*) as seedlings, (*b*) planted in open ground. 14. They may gain no benefit from a cross from the same or a different stock. 15. They are as healthy as the intercrossed. 16. They may be much more productive than flowers dependent upon insects. 17. Naturalised abroad they gain great vigour; and (18) are the fittest to survive in the struggle for life.

PHYSIOLOGICAL ACTION OF NICOTIN.—About twenty years ago the Rev. Prof. Haughton called attention to the fact that there was an antagonism between the actions of nicotin and of strychnia. His experiments were on frogs. About ten years afterwards Dr. Wormley experimented in the same direction with cats; and some five years ago Dr. Reese performed a series of experiments with these drugs on dogs. Not satisfied with the results of any of these experimenters and recognising the great importance of the subject, Dr. Haynes has made a long series of experiments on dogs, cats, rabbits, and rats, and after some 143 experiments. has come to the following conclusions:—"The recorded cases of strychnia poisoning treated by tobacco are extremely unsatisfactory. If they prove anything it is merely that tobacco is a powerful emetic." "Haughton's experiments on this subject (really only two in number) were performed in such an unscientific manner as to be utterly valueless." "Strychnia and nicotin are in no degree antagonistic poisons." "Strychnia increases the convulsive action and does not diminish the motor paralysis of nicotin." "Nicotin (even in paralysing doses) increases the convulsive action of strychnia." "Both poisons cause death by paralysing the respiratory organs. They may affect respiration in different ways, but the result is the same." Animals may be killed by injecting together doses of the two drugs which, singly, are not fatal. (*Proceedings* of the American Philosophical Society, vol. xvi., No. 99.)

GLASSY SPONGES.—Drs. W. Marshall and A. B. Meyer have published a memoir, as one of a series of communications to the Zoological Museum at Dresden, "on some new or little-known sponges belonging to the Hexactinellidæ found in the Philippines." It seems but the other day since one could have numbered on the fingers of one hand all the known species of this family, so well known to many by that beautiful typical form, the Venus's flower-basket (*Euplectella*), and now the number of described species is very large. In 1872 one of the authors (Dr. Meyer) was staying at Cebú one of the Philippine group, where *Euplectella aspergillum* is a regular article of trade, quoted at so much a dozen, and where it is not surprising that he should discover a number of other lovely forms in this memoir described and figured. Among the more interesting forms are the following:—*Hyalocaulos simplex*, *Myliusia zittelii*, and two species of *Aulodictyon*, all of these found living attached to the basal portion of *Euplectella*. *Semperella schultzei* is figured of a natural size from a specimen twenty-one inches in length, and figures of the spicules of the various new species are also given.

A MALE NURSE.—The interest of the reproduction of Batrachians is by no means yet exhausted. A Spanish naturalist, Jimenez de la Espada, has recently discovered additional facts respecting *Rhinoderma darwinii* (of Chili), which was first made known by Mr. Darwin. He finds that the supposed viviparous birth of the young from the female is a very different phenomenon. It is the males which are the nurses, and they have an extraordinary brood-sac, developed as a pouch from the throat, and extending over a great portion of the ventral surface of the animal. In this cavity a number of living tadpoles were found, in number of individuals, and the length of the tadpoles was about 14 mm. How these are first developed and nourished is not yet known. Dr. J. W. Spengel translates a portion of the Spanish paper in the current number of the *Zeitschrift für wissenschaftliche Zoologie*, vol. xxix. part 4.

STRUCTURE OF CYCADEÆ.—E. Warming, of Copenhagen, publishes (in Danish with French abstract) some fresh researches on this subject ("Recherches et Remarques sur les Cycadées," Copenhagen, 1877). He confirms in general the results previously arrived at by A. Braun and others, from the structure of the ovule and seed, the pro-embryonic characters, the mode of formation of the pollen and pollen-plant, and of the growth of the stem and roots, &c., that the Cycadeæ are very nearly allied to the Coniferæ; and in particular he places them near to the Gingko (*Salisburia adiantifolia*). Among Cryptogams he considers them to come nearest to Marattiaceæ and Ophioglossaceæ among Filicineæ. He proceeds then to discuss the homology of the ovule of Phanerogams, on which he thinks the structure of that of the Cycads—intermediate between Vascular Cryptogams and Angiosperms—throws much light. The phanerogamic ovule he considers to be composed of two parts, of different morphological origin, viz., a nucleus which is homologous with the macrosporangium; and a lobe of the leaf which bears the nucleus, consisting partly of the funiculus and partly of the integuments. In Angiosperms the nucleus rests on the surface of the leaf; in Gymnosperms it is partly imbedded in it. No part of the ovule is of axial origin (*caulome*).

THE BRAIN OF A FOSSIL MAMMAL.—Prof. Cope has been able to take a cast of the cranial cavity of a specie of the Tapiroid genus Coryphodon, from the Wahsatch beds of New Mexico. This has revealed remarkable primitive characters: (1) the small size of the cerebellum; (2) the large size of the region of the corpora quadrigemina; (3) the cerebral hemispheres were small, and (4) the olfactory lobes were very large. The medulla oblongata is wider than the cerebral hemispheres. In profile the brain closely resembles that of a lizard. These characters are so extraordinary that Prof. Cope considers them sufficient to mark a primary division of mammalia, which he, following Owen, calls Protencephala. Prof. Cope describes and gives figures of a cast, the skull cavity, in the *Proceedings* of the American Philosophical Society, vol. xvi., No. 99.

INSECTIVOROUS PLANTS[1]

SINCE the appearance of Mr. Darwin's work on "Insectivorous Plants" the want of direct proof that the plants profit by their carnivorous habits has been somewhat widely felt. Thus we find expressions to this effect by MM. Cassimir de Candolle, Cramer, Duchartre, Duval-Jouve, Faivre, Göppert, E. Morren, Munk, Naudin, W. Pfeffer, Schenk, &c., &c.

The assent which many naturalists have given to Mr. Darwin's explanation of the meaning of the structure and physiological properties of carnivorous plants rests on a sound basis, namely, the impossibility of believing that highly specialised organs are unimportant to their possessor, and the difficulty of giving any rational explanation except the one proposed in "Insectivorous Plants." Mr. Darwin himself felt the desirableness of direct evidence on this head, and the experiments intended to

[1] From a paper "On the Nutrition of *Drosera rotundifolia*," by Francis Darwin, M.B., read before the Linnean Society, January 17, 1878.

decide the question only failed through an accident. The present research by Dr. F. Darwin is practically a repetition of the same experiments.

The widely-spread belief that insectivorous plants thrive equally well when deprived of animal food rests on very insufficient grounds. Many observers have based their opinion on the general appearance of the plants, and in no case has observation been sufficiently extended in point of time or details of comparison. The plan of the present research was therefore (1) To cultivate a large number of plants. (2) To continue observation for a considerable space of time, during which artificial starving and feeding of two sets of plants was to be kept up. (3) To compare the starved and fed plants in a variety of ways and especially as to the production of seed.

With this object about 200 plants of *Drosera rotundifolia* were transplanted (June 12, 1877), and cultivated in soup-plates filled with moss during the rest of the summer.

Each plate was divided into halves by a low wooden partition, one side being destined to be fed with meat, while the plants in the opposite half were to be starved. The plates were placed altogether under a gauze case, so that the "starved" plants might be prevented from obtaining food by the capture of insects. The method of feeding consisted in supplying each leaf (on the fed sides of the six plates) with one or two small bits of roast meat, each weighing about one-fiftieth of a grain. This operation was repeated every few days from the beginning of July to the first days of September, when the final comparison of the two sets of plants was made. But long before this it was quite clear that the "fed" plants were profiting by their meat diet. Thus, on July 17 it was evident that the leaves on the "fed" side were of a distinctly brighter green, showing that the increased supply of nitrogen had allowed a more active formation of chlorophyll-grains to take place. It may be inferred, partly from microscopical examination of the starch in the leaves, but more certainly from the final comparison of dry weights, that the increase of chlorophyll was accompanied by an increased formation of cellulose. From this time forward the "fed" sides of the plates were clearly distinguishable by their thriving appearance and their numerous tall and stout flower-stems.

The advantage gained by the fed plants was estimated in many ways. Thus, on August 7 the ratio between the number of "starved" and "fed" flower stalks was 100 : 149·1. And by comparing the number of stems actually in flower it was clear that the starved plants were losing the power of throwing up new flower stems at an earlier date than their rivals. In the middle of August the leaves were counted in three plates, and were found to be 187 on the starved, and 256 on the fed side—or in the ratio of 100 : 136·9.

At the beginning of September the seeds being ripe, all the flower-stems were gathered, and the plants of three plates were picked out of the moss and carefully washed. As it seemed probable that one advantage of the fed over the starved plants would be the power of laying by a larger store of reserve-material, three plates were allowed to remain undisturbed after the flower-stems had been gathered. The relative number of plants which will appear in the spring on the "fed" and "starved" sides will be a means of estimating the relative quantities of reserve-material.

The following list gives the result of counting, measuring, and weighing the various parts of the two sets of plants. It will be seen the number of plants (judging from the three plates examined) were fairly equal on the starved and fed sides of the partitions so that a direct comparison of their produce is allowable :—

Ratio between the number of starved and fed plants	100 : 101·2[1]
Ratio between weights of the plants *exclusive* of flower-stems	100 : 121·5
Total number of flower stems	100 : 164·9
Sum of the heights of the flower stems ...	100 : 159·9[1]
Total weight of flower stems	100 : 231·9
Total number of capsules	100 : 194·4
Average number of seeds per capsule ...	100 : 122·7
Average weight per seed	100 : 157·3
Total calculated *number* of seeds produced	100 : 241·5
Total calculated *weight* of seeds produced	100 : 379·7

[1] In all cases "starved" = 100.

The most important feature in the general result is that the advantage gained by the fed plants is far more conspicuously shown in all that relates to the seeds and flower-stems than in any other part. Thus the ratio between the weights of the plants, exclusive of flower-stems were as 100 to 121·5; while the weights of the flower-stems, including seeds and capsules, were as 100 to 231·9. The highest ratio is seen to be between the total weights of seed produced, namely 100 : 379·7; and this is intelligible, because a store of nitrogen is laid by in the albuminous seeds.

Another point is that the difference between the starved and fed plants is more clearly shown in the comparison of weights than of numbers or heights. It is clear that increase of weight is a better proof of increased assimilation than any other character.

It may fairly be said that the above experiments prove beyond a doubt that insectivorous plants are largely benefited by a supply of animal food, and it can no longer be doubted that a similar benefit is gained in a state of nature by the capture of insects.

ALBERT VON HALLER

On December 12 last the republic and city of Berne celebrated the centenary of the death of one who is universally recognised as their greatest citizen. The important part played in science by Albert von Haller last century is a sufficient excuse for us, profiting by the occasion of the recent celebration, to enable our readers to appreciate the marvellous aptitude of this eminent man for every kind of work, theoretical and practical; he was at once a statesman, theologian, and poet, as well as a physiologist, anatomist, and botanist.

Albert Haller was born at Berne in October, 1708, of a family originally of St. Gall, one of whose members fell by the side of Zwingli in 1531. Very weak in body, like Isaac Newton, in his infancy, he exhibited, like him, an extraordinary precocity, and his avidity for books was something indescribable. Having finished his classical studies brilliantly and rapidly, he went to Tübingen at the age of fifteen years to study medicine, then soon after to Leyden to follow the clinic of the illustrious Boerhaave, on whose works he at a later time published a commentary which greatly contributed to his renown. Albinus taught him anatomy and J. Gessner botany. At eighteen and a half years he obtained the degree of doctor, and afterwards attended, in London, the teaching of Dr. Winslow. After a sojourn at Paris he returned to Switzerland and studied mathematics with Jean Bernoulli, and that with such ardour that his friends were constrained to look after him.

In 1728 he made, with Gessner, his first great Alpine excursion, which, many times repeated, made him, in an eminent degree, master of the Swiss flora. His most celebrated poem, entitled "Die Alpen," was another result of his mountain journeys, which contributed to diffuse among those far away the magic charm of that magnificent scenery.[2]

[1] Therefore the average height of the fed stems is slightly less (100 : 99·9) than that of the fed. But since equal numbers of *plants* are taken, the total yield of flower stems is the fair criterion.

[2] Prince Radzivil, Commander of the Polish Confederates, having at a later period become acquainted with the poem, could not think of anything better to signify to the author his satisfaction, than to send him a commission of Major-General.

His first anatomical instruction was obtained at Bâle, and was continued during five years, after which Haller returned to his native country, where an active medical practice did not hinder him from ever and ever reading to increase the field of his already vast knowledge. He read at table, in journeying on foot or on horseback, during his visits and consultations, which made those shake their heads who could not understand his marvellous clearness of perception.

His botanical labours were then very extensive, and brought him his first encouragement from abroad. In December, 1733, the Royal Academy of Sciences of Upsala received him among the number of its members, and proposals were made to him to become a professor there. At Berne his success was not easy; in 1734 he obtained the modest position of librarian. This was the epoch when, while carrying on his work as a practitioner, he gave himself especially to poetic composition, but which came to an end in 1736.

It was at this time he received a call from the newly-founded University of Göttingen, to go there as Professor of Anatomy and Botany. This call was accepted, and although it was for him the occasion of a great grief, in the death of his wife soon after their arrival, he displayed in this new centre a remarkable activity and capacity. His desire and his plans for the foundation of an anatomical theatre were soon realised. Measures were taken that subjects should not be wanting for dissection; and at the same time conformably to his proposals, a botanical garden was created which soon became one of the most important in Germany. He was the soul of his faculty and of the entire university, and his reputation caused students to flock to Göttingen from all countries, whom he encouraged in every way, prescribing to them various works in connection with his own and for the prompt development of the physiological sciences. He founded at Göttingen the Royal Academy of Sciences, of which he was appointed president, a position he retained to the end of his life, notwithstanding his return to his own country.

It was at this time he published his commentaries on the work of Boerhaave, when he commenced his "Elementa Physiologiæ," his "Anatomical Plates," his "Flora of Switzerland," and other works. In 1749 the King of England appointed Haller his private physician, and confirmed the titles of nobility which had been conferred on him by the Emperor Francis I. The Royal Society of London, the Academy of Stockholm, those of Berlin and Bologna, enrolled him on their lists of members. Frederick the Great of Prussia attempted to get him to Berlin, but Haller would only leave Göttingen to return to Berne, and he decided to do so in 1753. His zeal for public affairs caused him to accept in his native country official functions in which his aptitudes of every kind found their application. Appointed Bailiff of the district of Aigle, near the eastern extremity of the Lake of Geneva, he explored and worked the sources of salt; at Berne he contributed to the creation of an orphanage and a large hospital, upon which he inscribed the beautiful device, "Christo in pauperibus." In 1754 he received from the French Institute the great distinction of being nominated one of its foreign associates; of the eight then existing, three were Swiss—Jean Bernoulli, Euler, and Haller. He regretted that his administrative occupations absorbed much of the time he would have wished to devote to science; and yet even during this period of his life his productiveness was enormous. Besides a large number of monographs and dissertations on subjects in the domains of botany, medicine, anatomy, and physiology, he published more extensive works, such as: Two parts of anatomical plates in folio, a quarto volume of surgical dissertations, four volumes "Disputationes practicæ selectæ," and six volumes of his "Elementa Physiologiæ Corporis humani." He occupied himself more especially with the anatomy of the eye, the formation of the bones, and the comparison of the brains of birds and fishes. He was chiefly original in his experiments on the movement of the blood, in his researches on the development of the chicken in the egg, and on that of the fœtus of quadrupeds, as well as in his teratological studies.

In his physiology he introduced the dominant idea, which was his principal discovery, of irritability considered as a force peculiar to muscular fibre, independent of sensibility properly so called, and differently distributed. In his hands this force became a new law, with which he connected nearly all the animal functions. He can only be blamed, perhaps, for having distinguished it too absolutely and in too decided a manner from the nervous force on which it always depends. As to generation, Haller maintained the doctrine of the pre-existence of germs, and he gave it the most solid support in his studies on the fœtal development. Not knowing the chemical action of the air on the blood he was unable to understand the exact idea of respiration.

All his writings show immense erudition, the fruit of his extensive reading, with the assistance of a prodigious memory. In four "Bibliothecæ," published under his auspices at Berne, Zurich, and Bâle, he spoke of 52,000 different scientific works or treatises all known by him and annotated by his hand to make known the text, the sources, and the authors.

A similar erudition rendered him eminently apt at bibliographical work. Thus we have from him in his "Methodus Studii Medici" of Boerhaave a classification of works, in which their degree of merit is distinguished by one, two, or three asterisks. But few living authors were content with the number of asterisks which he accorded to their works, and this attempt made him numerous enemies. He had collected for his use about 20,000 volumes, which were bought after his death by the Emperor Joseph II. and given to the University of Paris.

On many occasions attempts were made to bring Haller back to Göttingen. In 1770 King George III. personally made overtures for this purpose; but the republic of Berne valued too highly his presence to consent to a new departure. The Council, while assuring the king of its friendship and its desire to please him, was opposed to this departure, not being able to be deprived of a man so necessary to the public weal in a place for life created expressly for him, and in view of the general service of the state. The passionate love which he had for his country made him respond in the most efficacious and the most varied manner to the hopes which his fellow-citizens had placed in his activity, more especially in the great start which agriculture took in his time and under his influence.

However, in the midst of so many matters, for which Haller was always of easy access, his health was constantly delicate. With advancing age many infirmities presented themselves which would have arrested a man of less energy, and which led to very painful crises. Gout and insomnia tormented him more and more, and he did not conceal from himself that the use of opium, by means of which he combated them, had serious drawbacks. One of his friends advising to change the *régime*, he replied in Italian:—

"Sono venti tre ore e mezza."

Haller died December 12, 1777, in his seventieth year, observing till the last moment the ebbing of his life, and indicating at last by a sign the moment when his pulse stopped. But he saw the approach of death with the calmness of a confirmed Christian, having all his life preserved a sincere faith, without fearing more than Newton, Euler, or Linné, that that faith could be contradicted or compromised by the scientific researches which he had pursued with a zeal which has scarcely been surpassed.

E. G.

THE MODERN TELESCOPE[1]

IV.

THE next point to which Mr. Grubb refers is one to which much interest attaches. It is now a long time ago since Sir J. Herschel investigated the effects of differently shaped apertures upon the images of stars. The figure shows the effects they produce due to diffraction.

An effect is also produced on the image if a round, or triangular, or square patch is placed in the centre of the object-glass. With the former the discs of the stars are smaller, and the position of the diffraction rings is changed, so that double stars can thus be measured, while in ordinary circumstances the companion is hidden by one of the rings.

Now in a reflector, unless, indeed, we use the front view, the central patch is always present, and it is to this and to the arm which supports it that the peculiar look of a star in a reflector is due. Mr. Grubb does not hesitate to ascribe to this the great difference of opinion that exists as to the performance of the two classes of instruments, and adds :—

"A veteran and well-known worker with refractors declared 'he never looked into a reflector without drawing away his eye in disgust;' and workers with reflectors cannot understand how the refractor workers can bear that dreadful fringe of colour from the secondary spectrum. The same applies to other matters. Newtonian observers cannot understand how those who observe with refractors or Cassegrain reflectors can bear to strain their neck so in looking up through the tube; while the refractor and Cassegrain workers cannot understand how the Newtonian workers will break their backs sitting or standing bolt upright when they might be reclining comfortably on an easy chair as they do. After all, when this comes to be investigated it resolves itself into but little more than a question of to which telescope the observer has been most accustomed. Each observer becomes in time *wedded to his own instrument;* he has done his work with it, the credit of his discoveries is due to it, and he naturally falls into the idea that no other can be as good."

We next come to those points in which the reflector is stated to be superior to the refractor. These are absence of secondary spectrum, superior applicability for physical work, possibility of supporting mirrors irrespective of size, and handiness of reflectors due to their short focal length, and especially if the Cassegrain form be employed. With regard to the first point, the experiments of Mr. Vernon Harcourt and Prof. Stokes, in which they attempted to produce two kinds of stars with rational or nearly rational spectra, have failed to lead to any great hopes being formed as to ultimate success, and the superior advantage

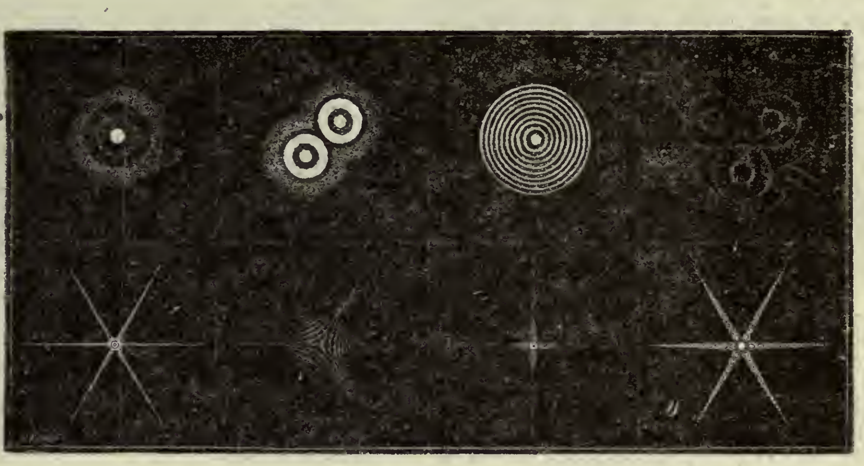


FIG. 10.—Diffraction effects produced by apertures and stops of different shapes (Herschel).

of the reflector in the fact that there is no colour will doubtless long remain. The superior applicability for physical work is much more doubtful. At present we know too little about reflection from metals and many other points to lay down the law with certainty, and in my own opinion Mr. Grubb's dictum is far too absolute with regard to spectroscopic work.

In another part of his valuable paper Mr. Grubb measures the advantage of the reflector with regard to the question of support; he shows that an object-glass may be supported by a central arm without loss of definition. and even that the tube may be filled with compressed air. He says: "The pressure required would be very small. Suppose the objective to be forty inches aperture, and 600 lbs. weight, and that it was purposed to lift $\frac{2}{3}$ of its weight on the air cushion, a pressure of about $\frac{1}{3}$ of a pound to the square inch, or say $\frac{1}{50}$ of atmosphere would suffice, even when the telescope is at its maximum elevation."

The remarks of Mr. Grubb on the practical difficulties which supervene when increased aperture is required, are best given in his own words :—

"It may be said that the difficulty of manufacture is a question for the instrument-maker alone, and not to be discussed by those whose business it is to decide on the form of instrument employed; but it should be remembered that any advance in the size of telescopes, refractors, or reflectors, over those at present in existence, must be considered to be to a certain extent, an experiment, and the nature of the difficulties which will be encountered can at present only be speculated upon, even by the most experienced; and therefore it behoves those whose province it is to decide on the matter to inquire diligently into the relative practicability of the various forms of telescopes in order that they may not decide on a form which might be, if ever accomplished, of great usefulness, but which on trial would be found to be, in the present state of art, impossible to manufacture.

"With respect to refractors, the first great difficulty to be met with is that of procuring suitable discs of glass. Of our glass manufacturers only two firms seem to possess the secret of manipulation of optical glass, viz., Messrs. Chance, Brothers and Company, of Birmingham, and M. Feil, of Paris, a descendant of the celebrated Guinand. Of these one at least speaks confidently of producing discs up to one metre in diameter; but when I consider the difficulty which I know was experienced in moulding the 27-inch discs for the Vienna objective I cannot say that I feel the same confidence. These 40-inch discs would require to be obtained in one single

[1] Continued from p. 189.

piece, just three times the quantity of homogeneous glass that the Vienna discs required, and though I am not of course in the secrets of the glass manufacturers, it appears to me that the chances of obtaining 40-inch discs in the present state of the art are remote.

"The other difficulties of manufacture of refractors consist in the nicety of the operation connected with the calculations of the curves, the manipulation of such extremely costly material, and the enormous labour and trouble of the figuring and perfecting of the objective. All these, however, I have no doubt will be overcome by the optician for any size which the glass-maker is at all likely to produce.

"Now, as to the difficulties connected with the manufacture of reflectors, whether metallic or silver on glass.

"First, as to the difficulty of producing the metallic or glass disc to work upon.

"Lord Rosse has succeeded years since in casting, annealing, and perfecting discs of six feet in diameter, and any difficulties he met with were not such as to lead me to the belief that the limit of possible size has been by any means reached. As regards glass mirrors, the question has never been discussed, for in any sizes that have been made up to the present time, it was only necessary to go to the plate-glass manufacturers and say, 'I want a disc of crown glass of such a diameter and such a thickness,' and forthwith the glass disc was delivered without any trouble; but, when we come to these extraordinary sizes, it is quite a different matter. For the 4-foot disc of glass for the Paris reflector, in place of that which has so recently resulted in failure, the St. Gobain Glass Company require twelve months' time to perfect (although, be it remembered, the quality of the glass is here of no consequence whatever); and I have been myself in correspondence with the principal glass manufacturers here and on the Continent, and not one of them is willing to undertake even a 6-foot glass disc; so that it would appear that, above that size, the silver-on-glass mirrors are out of the question.

"This much, however, is to be said: If anyone were to go to a brass- or bell-founder's and ask them to undertake a speculum of six feet in diameter, he would almost certainly be met with a refusal; and yet Lord Rosse has proved the feasibility of it. And so, reasoning by analogy, might the manufacture of a six- or eight-foot glass mirror be possible, if undertaken in the same scientific spirit in which Lord Rosse undertook his. I answer to this—Yes; perfectly true; but this is too purely a speculative matter to be considered at the present day in the choice of telescopes.

"The other great difficulty in the manufacture of reflectors is the annealing of the disc, and I believe it is this difficulty which limits to so narrow an extent the production of glass discs for silver-on-glass mirrors."

We can abundantly gather from this paper of Mr. Grubb's that our opticians are doing all that lies in their power to give us increased power in the future. The fact that in the last few years one refractor of 25 inches, and two of 26 inches, have been acquired to science, leads us to hope that for the present progress will lie in increasing the dimensions of that instrument. Mr. Grubb, indeed, has already in hand one of 27 inches for the Austrian Government. The *contretemps* to the four-foot Foucault in Paris will also help to set the tide in the same direction.

From what has preceded it will be seen that each increase in the power of the telescope is of little avail unless we use it in purer and purer air. It is quite true that in the telescope much of the injury to definition arising from currents in the tube may be got rid of by the employment of lattice-work; but this, of course, will not lessen the atmospheric effects of the column of air ever increasing in diameter between the telescope and the object.

Prof. Piazzi Smyth's astronomical experiences on Teneriffe will still be in the minds of many of our readers. He showed that an enormous advantage was secured from observations so soon as half the atmosphere was below the observer. A more recent experiment by Dr. Draper, however, has shown that it will not do to go blindly and put the telescope on any high mountain. The conditions of each place from this single point of view must be carefully studied. Summing up his experiences of the Rocky Mountains up to heights of 10,000 feet, Dr. Draper says:—

"On the whole, it may be remarked of this mountain region that the astronomical conditions, especially for photographic researches, is unpromising. In only one place were steadiness and transparency combined, and only two nights out of fifteen at the best season of the year were exceptionally fine. The transparency was almost always much more marked than at the sea-level, but the tremulousness was as great or even greater than near New York. It is certain that during more than half the year no work of a delicate character could be done. . . . Apparently therefore, judging from present information, it would not be judicious to move a large telescope and physical observatory into these mountains with the hope of doing continuous work under the most favourable circumstances." J. NORMAN LOCKYER

(*To be continued.*)

ELECTRICAL ANALOGIES WITH NATURAL PHENOMENA

WITHIN the last few years M. Gaston Planté has at intervals described a series of very curious phenomena produced by electric currents of high tension, and has pointed out numerous analogies which they present with several atmospheric and cosmical phenomena. Without committing ourselves to the belief that these analogies are real, the phenomena described are so interesting that we are glad to be able, by the kindness of M. Planté, to reproduce some illustrations of them.

To obtain electric currents of high tension M. Planté has employed secondary batteries of sheets of lead, which, as is known, constitute powerful accumulators of voltaic electricity. By associating a very great number of batteries uniting from 400 to 800 of these secondary couples, a discharge is obtained equivalent, according to M. Planté, to that of from 600 to 1,200 Bunsen couples arranged in tension.

Fig. 1 represents the arrangement of 400 secondary elements divided into ten batteries. This is the source of electricity employed for some of the earlier experiments which we are about to describe. The more recent ones have been made with 800 secondary elements arranged in twenty batteries of forty couples. A second series of batteries similar to the first is arranged in another room, and the current which it furnishes is joined to that of the first series by conducting wires suitably adjusted. These batteries, associated at first in simple circuit by means of commutators, do not require to be charged all at once like two Grove or Bunsen couples. When they have not been out of use for too long a time a few hours suffice to charge them. We may then, by turning the commutators, unite all the secondary elements in tension and use at will, either in a few seconds or in a longer time, the enormous quantity of electricity resulting from the chemical work accumulated during two hours by Grove or Bunsen batteries.

Such was the powerful means adopted by M. Planté in making his late experiments. In his earlier experiments he used a much simpler apparatus.

The gyratory movements accompanied with luminous effects which M. Planté had observed with a powerful current of electricity, and the spherical and annular forms manifested by bodies submitted to that action, suggested to M. Planté the probability of the electric origin of the forms of some of the nebulous masses of matter which

are not resolvable, and particularly of those which assume a spiral form.

He describes an experiment in which a cloud of metallic matter attracted to an electrode by the electric current assumes in the centre of the liquid a gyratory spiral movement under the influence of a magnet.[1] A glance at Figs. 2, 3, and 4, which represent this experiment, is sufficient to enable us to recognise their similarity to the forms of spiral nebulæ described by Lord Rosse. Some of these have the curvature of their spirals tending in a direction opposite to that of the hands of a watch, like those in Fig. 3, such as in the nebula

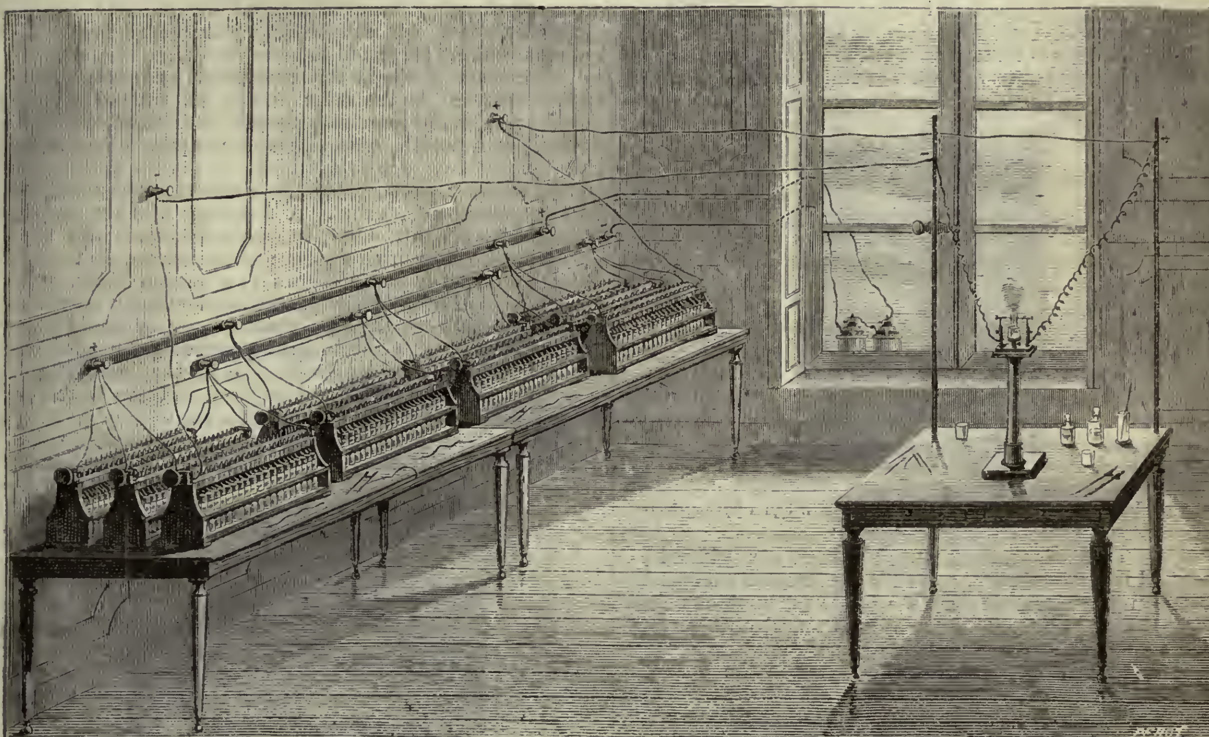


FIG. 1.—Arrangement of 400 couples in ten batteries for experiments with electric currents of high tension.

in Berenice's Hair; others have their spirals in the same direction as the hands of a watch, like that of Fig. 4, as in the nebula in Canes Venatici. M. Planté is inclined to believe that, in presence of an analogy so striking, we are authorised to think that the nucleus of these nebulæ may be constituted by a true centre of electricity; that their spiral form may be determined by the near presence of celestial bodies strongly magnetic, and that the direction

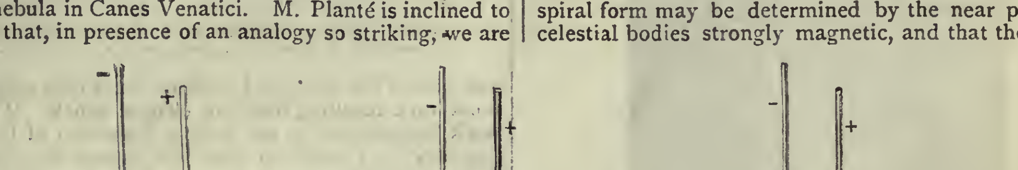


FIG. 2. FIG. 3. FIG. 4.

FIG. 2.—Cloud of metallic oxide formed in a voltameter before the approach of a magnet. FIGS. 3 and 4.—Gyratory movement communicated to the cloud of metallic matter by the action of a magnet.

of curvature of the spirals may depend on the nature of the magnetic pole turned towards the nebula. He

[1] It is easy to reproduce this experiment and even throw the effect on a screen, by means of an electric current equivalent to that of fifteen Bunsen elements. The electrodes are copper wires; the liquid is acidulated with 1-10th of sulphuric acid. From the extremity of the positive wire escapes, with a slight hissing sound, a thick cloud of the protoxide of copper or of finely-divided copper, and this wire takes the form of a very sharp point. The arrows around the spirals in the figures indicate the gyratory movement which this cloud assumes under the influence of a magnet; and the arrows around the magnet represent the direction of the electro-magnetic currents; B is the north and A the south pole.

suggests that if we had powerful enough telescopes, the neighbourhood of the nebulæ should be searched to discover stars capable of exercising such a magnetic influence. If such a star was found likely to act thus on any nebula, then the line passing through the centre of the nebula and the star should be searched to discover if, at the other magnetic pole of the star, a second nebula did not exist, with its spirals in a contrary direction to those of the former.

M. Planté states that with a much more intense source of electricity he has observed small luminous rings, composed of incandescent particles, altogether detached from the electrode. These rings, the centre of which was agitated by a small liquid whirlpool, moved in the interval comprised between the electrode and a very large luminous ring formed round about by the shock of the electric current against the sides of the voltameter.

Fig. 5.—Luminous globule formed at the surface of a liquid by an electric current of high tension.

The Formation of Hail.—In a paper in the *Comptes Rendus*, t. lxxxi. p. 616, M. Planté had shown the influence which atmospheric electricity in a state of discharge must have in the formation of hail, not by producing the cold necessary to congelation, as is sometimes supposed, but by exercising, on the contrary, a powerful heating action, capable of rapidly vaporising the moisture, and of projecting the vapour into the cold regions of the atmosphere. To succeed in explaining the part played by electricity in this natural phenomenon, it is necessary to point out the mechanical action which may result from the passage of the electric fluid into the midst of aqueous masses, and thus to project into the air liquid globules susceptible of being transformed into hailstones.

Fig. 6.—Shower of aqueous globules produced by a current of double the tension of the preceding.

In previous experiments M. Planté showed that with an intense source of voltaic electricity, the immersion of the positive wire in a conducting liquid, such as salt water, determines the aggregation of aqueous molecules around the electrode in the form of a luminous spheroid, in consequence of a double simultaneous effect of flow and aspiration, or of passage in two directions which seems peculiar to the electric current (Fig. 5).

But by employing a current still more intense, resulting from the discharge of a battery of 400 secondary couples, he obtains, by the immersion of the positive wire, instead of a single globule, a shower of innumerable ovoid globules, which succeed each other with excessive rapidity, and are projected to more than a metre distance from the vessel in which the experiment is made. The spark produced at the same time at the surface of the liquid presents the form of a corona or aureole of many points, from which burst forth the aqueous globules (Fig. 6).

The metallic property of the electrode is not necessary to obtain this effect. A fragment of filter paper, moistened with salt water, in communication with the positive pole, also produces the phenomenon, and constitutes a humid mass analogous, to a certain point, with that of a cloud from which proceeds an electric current. If, instead of encountering a deep layer of liquid, the current meets with a moist surface such as the sides or the inclined bottom of a basin, the heating effects predominate, the aureole is very brilliant, and the water is rapidly transformed into vapour (Fig. 7).

The action of the current then differs according to the resistance which is opposed to it, and we find here a new example of the reciprocal substitution of heat and mechanical work resulting from the electric shock. When the work represented by the violent projection of the liquid appears, there is neither heat nor vapour developed, and when no visible work is accomplished, when the liquid is not projected, heat is engendered and vapour disengaged.

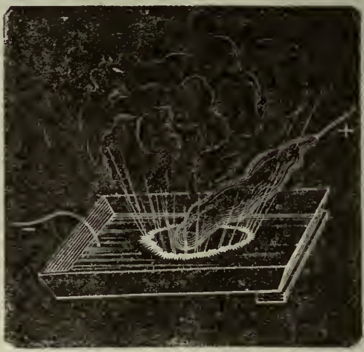


Fig. 7.—Jets of vapour and luminous streaks produced by an electric current of high tension on meeting with a moist surface.

M. Planté sums up the results of these experiments thus:—

1. Electric discharges produced in the midst of clouds may, according to the greater or less density of these moist conductors, determine their reduction into vapour, or their instantaneous aggregation into globules of a volume much greater than that of the globules of the cloud, and the liquid bombs thus formed may be projected to great heights, where the temperature is notably lower than that of the medium in which the discharges are produced.

2. The formation of hailstones, in the case where they do not present a series of opaque and transparent layers, but a structure radiating from the centre, is also explained by such a mechanical action; they must be produced by a single jet, and congealed under the same volume which they had at the moment when they were shot forth by the electric current.

3. The ovoid or pyramidal form of these hailstones, as also their angular parts, aspirates or protuberances, are due to their electric origin.

4. The gleam sometimes emitted by hailstones is due also to electricity; for although in the experiments described, it could not be distinguished whether the globules were self-luminous or shone by the reflection of the spark, it is probable that they were also rendered phosphorescent by the electric current which they contained.

M. Planté admits the action of other agents, such as wind currents, in the formation of hail, but only as accessories to the action of electricity. They are concurrent causes which only prepare the conditions favourable to its production, while electricity is the efficient cause which, by its very presence in the clouds and by the instantaneous power of its discharges, determines the sudden formation and the fall of the meteor.

M. Planté is still prosecuting his researches on this subject.

(*To be continued.*)

ENTOMOLOGY IN AMERICA

THE U.S. Entomological Commission which was organised and placed under the auspices of Prof. Hayden's Geological Survey for the purpose of investigating and reporting the entire subject of insect ravages throughout the western regions of our continent, have completed their field labour for the present season.

The members of the Commission have been busily engaged in the preparation of the several parts of their Annual Report, and will soon meet in Washington, where they will have a protracted sitting to get everything ready for the printer. This report is looked for with much interest by the farmers of the west, and the character of the commissioners is a guarantee that it will be creditable from the scientific, and valuable from the practical, standpoint. The Report will contain sixteen chapters, under the following heads:—Introduction, Riley; Chronological History, Packard; Statistics of Losses, Thomas; Classification and Nomenclature, Thomas; Geographical Distribution, Thomas and Packard; Migration and Meteorology, Packard and Thomas; Original Permanent Breeding Grounds, Riley, Packard, and Thomas; Habits and Natural History, Riley; Embryology, Packard; Metamorphoses, Riley and Packard; Invertebrate Enemies, Riley; Vertebrate Enemies, Thomas; Remedies and Devices for Destruction, Riley; Prairie Fires *versus* Locust Injury, Riley; Agricultural Bearings of the Subject, Thomas; Ravages of other Locusts, Packard and Riley; Locust Ravages in other Countries.

These chapters will embrace many sub-chapters, and the Report will be as exhaustive as the limited time for its preparation will permit.

In Chapter IV. the western extension and the northern and eastern limit of the species' range are fully given.

In Chapter V. the laws governing its migrations are for the first time defined. A very large number of data have been collected in reference to the subjects of this chapter. Not only are the general laws governing the movements of the insect now defined, showing a regular migration southward and return migration northward, which may be counted on and foreseen; but many important and highly interesting facts in reference to their local flights are brought to light, which will henceforward form a part of the history of the insect.

In Chapter VII. several other laws governing the species are also adduced; and the importance of the discovery of the laws which regulate the doings and movements of the pest, cannot be over-estimated. In said Chapter VII. many new facts will for the first time appear, and all that is definite and accurate be made known.

In Chapter X. many new discoveries will be recorded, some of them of great scientific interest and importance. Of these may be mentioned the transformation of the silky mite (*Trombidium sericeum*). This is an eight-legged creature, which preys on the locust eggs. It is proved to be the mature form of the little six-legged mite (*Astoma gryllaria*) which is parasitic on the locust. Insects described under different genera are thus proved to be specifically identical. The life-history of the blister-beetles will also be given, their larvæ feeding upon locust eggs, and undergoing singular changes called hypermetamorphoses. The interest attaching to this discovery among entomologists, as well as among farmers, is best appreciated when it is considered that absolutely nothing has heretofore been known of the larval habits of these blister-beetles, notwithstanding the fact that for half a century much attention has been given to the subject by scientific men, on account of the commercial value of Cantharis, or Spanish fly, and of the great injury to potatoes and other plants committed by several of our American species.

In Chapter XI. are given the locust feeding habits of many western animals not heretofore known to have that habit, and the good offices of birds are especially made manifest, examinations of the stomachs of over ninety species and 630 specimens having been made with special reference to their locust-eating habits. The record in reference to these examinations is very full, giving the dates, the locality, the common and scientific names of the species, and the number of locusts and other insects found in each. The value heretofore placed on these aids by entomologists is fully sustained by this record.

In Chapter XII., which will be one of the most extended and most important practically, it is clearly shown that the young locusts may be controlled, and by what means; while the way is pointed out how to better control the winged insects. Many valuable devices for destruction will be illustrated, among them one invented by Prof. Riley, which gave great satisfaction, and will, it is believed, supersede all others as a cheap and practicable remedy applicable at any season, whether the plants or the insects be small or large.

In Chapters II. and IV. are given statistics showing the immense losses inflicted on western agriculturists by the locust. These chapters also show what crops are most liable to injury and what are most easily protected; also the best methods of cropping in order to reduce the injury to a minimum. A chemical analysis of the dead locusts has been made, and is unusually interesting. The insects furnish a new oil, which will be christened Coloptine, and a very large per-centage of pure formic acid. Though this acid exists in the ant and some other insects, it is with difficulty obtained in large quantities, whereas by the action of sulphuric acid upon the locust juices it passes off with great readiness and in remarkable quantity and gravity. The various uses of this acid whether as a therapeutic agent or as laboratory re-agents, &c., are capable of great and valuable extension when it can once be obtained so readily and in such quantity.

The Report is expected to make about 500 pages, and will, it is hoped, be published in February or March. Although the Commissioners have divided the labour among them, the Report will form one complete whole, as the work of each will be discussed and revised by the Commission as a whole.

The Annual Report, which is intended more particularly for the practical farming public, will be followed by memoirs of a more purely scientific nature—one by Dr. Packard on Anatomy and Embryology; one by Prof. Riley on the Natural History of other Locusts, and one by Prof. Thomas on the classification of the *Acrididæ*.

While it has been the object of the Commission to cover as much ground as possible so as to make the Annual Report as full and valuable as the time would permit, there yet remain several important subjects that it has so far been impossible to properly and exhaustively study. The territory affected is so vast, embracing over

a million square miles, that much of it was imperfectly explored, especially in the north-west. Mr. Riley had to cut short his investigations in British America both for want of time and want of funds. For similar reasons, and on account of Indian troubles, Montana, Wyoming, and Dakota, have been but superficially explored.

The year 1877 was an abnormal year, *i.e.* the insect had, the previous year, overrun a large section of country in which it is not indigenous, hatched in such country in the spring. This was most fortunate for many reasons, as it enabled the Commission to carefully study the insects in this their unnatural condition, and to carry on experiments with a view of learning how to control them. Much of the work of the Commission was with these young insects. The losses sustained through the devastation of the pest by young and struggling frontier populations, ill able to bear them, were immense; and there was so much discouragement that hundreds and thousands of persons were on the point of abandoning their new homes last spring. At this juncture the Commission went into the field, and by its encouraging predictions (which were all verified) and recommendations, imbued the people with hope and confidence, and drew westward again the emigration that had almost stopped. All this work, however, interfered with needed investigations into the proper range, the native home and breeding grounds, the source of swarms, and many other important questions which can only be properly studied during a normal year. It is, therefore, very important that the investigations be continued until every question is settled that human investigation can settle.

For the proper settlement of some of the questions the co-operation of the Dominion Government is desirable, and has been promised by the Canadian authorities if the work of the Commission should continue.

It will be unwise to stop the work of the Commission before completed. The work should be made so thorough as to obviate any necessity in future years of creating another commission for the same purpose. After careful estimates it is concluded that the work can be satisfactorily completed only with two more years' investigation and experiment. The Commission therefore ask for a continuance of the appropriation of 25,000 dols. asked for a year ago.

There are various other injurious insects of national importance, of which much has yet to be learned, and in addition to completing the locust investigation, the Commission contemplate, during the coming two years, studying and reporting on some of these worst enemies to American agriculture. They are especially desirous of reporting on the cotton-worm of the south, which, though often so disastrous to the cotton crop, has never been fully studied, and in the mere natural history of which there are yet many mysteries and conflicting theories.

Much has yet to be done in giving practical form to the conclusions arrived at and plans proposed by the Commission to enable the work already done to bear proper fruit. To bring about the needed co-operation of the two Governments, to cause proper laws to be enacted in all the states interested, and to enforce the truths that alone will make man master of the situation, is largely the work of the future.

SOUNDING APPARATUS

THAT Sir William Thomson's recent application of the pianoforte wire to sounding in small depths for the ordinary purposes of navigation is of great value, will be admitted readily by those who are familiar with the present process. But it occurs to me that a formidable objection to its general introduction into naval or mercantile vessels is to be found in the necessity of using chemically-prepared tubes for determining the depth of water. Sir William's latest device is (I believe) a straight glass tube two feet long, open at one end and inclosed in a brass tube attached above the sinker, in which air is compressed by the pressure of the water, the amount of compression being determined by the height to which the water rises in the tube. This height is marked by the decolorisation of a coating of chromate of silver on the inside of the tube, effected by the sea-water. A number of such tubes, properly prepared, must therefore be kept at hand, and when once used they must be coated anew, an operation of no little difficulty.

I have suggested a form of sinker in which these objections are obviated, while the principle is retained. The sinker is of iron three inches in diameter at the bottom, five inches at the top, and 26·5 inches long. It is cast with a cylindrical cavity, two inches in diameter, extending from the top to within an inch of its base. This cavity contains the glass tube by which the depth is determined. A tube about forty-eight inches long is taken, closed at one end and bent back on itself at its middle point, so as to make two legs each twenty-four inches in length. This is placed inside the sinker (the bend upward) and a screw tap, carrying a swivel-link for the sounding line, is screwed over it. Holes in the bottom of the sinker and through the screw tap allow the water access to the tube. As the sinker descends, in sounding, the air within the tube is compressed and the water rises in the open leg. When the column of water reaches the highest point of the bend, the pressure then corresponding to a depth of about five and a half fathoms, any further descent of the sinker will cause the water to pass over into the lower end of the closed leg. The compression of the air will then take place in the *upper part of the closed leg*, the maximum compression being indicated by the length of the column of water remaining in that leg when the sinker is lifted again to the surface. As the sinker is being raised, the air, expanding under the diminished pressure, drives the water out of the open leg. The inside and outside pressures are therefore equal at any instant. The tube may be graduated in inches and tenths, and a table will give the depth from the reading of the tube. The tube is then easily emptied and is ready for another cast. The form of the sinker is such that the bend of the tube is kept at a higher level than the open end in case the sinker should fall over when it reaches the bottom—the entrance of surplus water is thus prevented. An ordinary cup attachment for a bottom specimen can be applied to the end of the sinker.

The tube described will not indicate a depth less than five and a half fathoms. If it is desired to obtain casts in shoaler water a tube with the open leg shorter than the closed leg may be used. One in which the length of the open leg is one-fourth that of the closed leg will indicate depths of two fathoms and upwards.

I am aware that Sir Wm. Thomson has a tube for bringing up the column of water, but it requires the use of valves, which can never be kept tight under such enormous pressures as those to which the sounding-tubes are exposed.

I inclose a sectional drawing of the above-described tube and sinker.

THEO. F. JEWELL, Lieut.-Com. U.S. Navy

OUR ASTRONOMICAL COLUMN

THE VARIABLE STAR R AQUARII.—Harding notified his discovery of variability in this star in 1811, in the first volume of the *Zeitschrift für Astronomie*. The earliest attempt to determine the period appears to be that of Westphal, in the *Zeitschrift für Astronomie*, vol. iv. p. 218; he used Harding's observations between October 20, 1811, and January 19, 1817, which, though not numerous, sufficed to give an approximate value, while they also indicated that the star at times was as bright as 6·7 m., and at others was invisible in Harding's telescope. Westphal's period is 382·5 days. Although the variability of the star has thus been long known, it would seem that few of these objects have been less observed, and it may be recommended to the attention of those who are interested in this branch of astronomy, and whose positions enable them best to command a star at 16° south declination. In vol. vii. of the Bonn observations, Argelander deduces the following formula for the maxima:—

1843, September 4·7 + 388^d·011 E.,

which is adopted in Prof. Schönfeld's second catalogue (Manheim, 1875); the maximum of the present year would therefore fall on September 25, and may be well observed. In the same catalogue the degree of brightness at minimum is set down as "11 m (?)." Harding estimated the star 6·7 m. on October 20, 1811, and on January 24, 1812, it was not visible in his telescope, being then below what he called a tenth magnitude, so that observations for determination of the minima should probably be commenced not later than seventy days after the maxima, but it is hardly necessary to remark that in the actual state of our knowledge of the variations of this star, continuous observations through as long a period as its position allows, will possess much interest. The best determination of the place of R Aquarii will be that of the Greenwich Catalogue of 1864, giving for the beginning of the present year—

R.A. 23h. 37m. 30s·35, N.P.D. 105° 57′ 37″·3.

ϵ INDI.—When may we hope that some southern observer will find opportunity of attacking the parallax of this remarkable star, the large proper motion of which was first pointed out by the late Prof. D'Arrest, and confirmed by Moesta from the Santiago observations of 1856? Mr. Gill, who allows nothing to escape him, during his brief visit to the other hemisphere, wherein Lord Lindsay's heliometer enables him to do an astronomical service, states that he has measured the distance and position-angle of ϵ Indi relative to five surrounding stars, and hopes "that this may serve as the foundation at some future day of a determination of its parallax and proper motion," but it is obvious that the shortness of his stay at Ascension does not permit of an attempt to measure the amount of parallax—a very interesting undertaking in the case of this star, which, had time allowed, we do not think that Mr. Gill would have hesitated to attempt. And ϵ Indi is not the only star which holds out prospect of success in parallax investigations in the southern hemisphere.

THE SATELLITES OF MARS.—In the last number of the "Monthly Notices of the Royal Astronomical Society" is a communication from the director of the Observatory at Melbourne, giving the results of a search made for the satellites of Mars, in consequence of a telegram notifying their discovery, and received from Sir George Airy on August 22. At that time, from an accident to the declination movement, the large reflector was not available, but observations with it were commenced on September 26. Mr. Ellery states his search to have been fruitless, except that on one occasion it was believed that one of the satellites was seen. This was on the night of October 16, when Mars having occulted a star of the thirteenth magnitude at 22h. 15m. sidereal time, after its emergence a very faint point was seen half a diameter from Mars s.p.; "this was watched for nearly an hour, when its position indicated a motion with Mars," but the sky becoming cloudy, no measures could be made, and, it is added, "no other signs of satellites have been observed since."

If we use the elements of the exterior satellite employed for the ephemerides which have appeared in this column, and which agree precisely with measures of position-angle made by Mr. A. Common, of Ealing, with his eighteen-inch silver-on glass reflector on the date in question (October 16), we have the following angles and distances:—

Melbourne Sidereal Time.	Position.	Distance.	Distance from Limb.
h. m.	°	″	″
22 15	216·0	34·6	25·2
23 15	196·7	27·1	17·7
0 15	167·6	23·5	14·1

Therefore, although the satellite would be in the south-preceding quadrant up to about 23h. 45m. sidereal, its distance would be greater than that estimated at Melbourne, and it is doubtful if this satellite was seen.

As regards the inner satellite, it is not practicable from the measures hitherto published to form so close an estimate of the positions as late as October 16, but on calculation from elements which represent sufficiently well the measures to September 20, it would appear that the satellite was in the south-preceding quadrant after about 23h. 30m. sidereal time, and its distance from the limb at that time would be approximately a semi-diameter of the planet. Thus if either satellite were really observed, it was most probably the interior one—which, indeed, we are assured, is intrinsically the brightest. But the want of better success with the great Melbourne reflector would rather imply that however well adapted for delineation of nebulæ and similar purposes, the instrument fails with observations of such objects as the satellites of Mars.

NOTES

WE understand that on the representation of the Professors of the Royal School of Mines and of the Director-General of the Geological Survey as to the want of proper accommodation for geological teaching in the School of Mines in Jermyn Street, the Lords of the Committee of Council on Education have transferred the instruction in that subject to the Science Schools at South Kensington. As Prof. Judd is supplied with a complete collection of specimens for teaching purposes, and as a laboratory is now provided for him, he will be in a position to give that practical instruction which it is so desirable should be within the reach of geological students.

STUDENTS of pleistocene geology will be gratified to learn that the well-known very fine collection of Ilford fossils, formed by the late Dr. Richard Payne Cotton, F.G.S., has been bequeathed to the Museum of Practical Geology, Jermyn Street. The collection contains 246 specimens of vertebrate remains, consisting of bones belonging to species of mammoth, rhinoceros, ox, aurochs, hippopotamus, horse, deer, Irish elk, lion, bear, beaver, water-rat, wolf, and several kinds of birds. A very perfect lower jaw of the beaver (*Castor europæus*), with some well-preserved bones of the *Elephas primigenius*, the *Rhinoceros leptorhinus*, and the *Bos primigenius*, are among the gems of this private collection, which will form a welcome and most valuable addition to the contents of the National Museum of British Fossils—the more so as the series of late tertiary vertebrates in that collection is by no means so large and complete as could be desired. Every one interested in the geology of the metropolitan area is aware that the Cotton collection, together with that made by Sir Antonio Brady, which has lately been acquired by the British Museum,

have formed the basis of those interesting researches which have been carried on by several distinguished palæontologists concerning the nature of the vertebrate fauna which inhabited the Thames valley during post-pliocene times. The nation is very fortunate in thus having secured for the use of students in the future both of these remarkably fine collections.

IT is with much regret that we have to record the death, on the 10th inst., of Andrew Murray, F.L.S., the eminent entomologist. In the field of scientific botany and forestry and in aid of our scientific intercourse with foreign countries he has done good service, and in his own special line as a practical entomologist, the Government collection at Bethnal Green stands as a monument of patient labour, as well as profound knowledge of his subject. A keen observer and unflinchingly truthful, the records of his original observations, or his careful compilations, given in his peculiarly terse and condensed style are a valuable legacy. A valued naturalist and assiduous worker he continued at his post till within a few days of his decease, and sunk away quietly and gently after some months of failing health. In private life he was greatly esteemed as a true-hearted friend, unwearied in aiding wherever he could be of assistance, and also for his high intellectual powers.

THE death is announced, on the 7th inst., of Prof. William Stokes, M.D., F.R.S., of Dublin.

ONE of the oldest, best known, and most useful of American naturalists, Dr. J. P. Kirtland, died on December 10. In 1848 Dr. Kirtland received the appointment of zoologist to the Geological Survey of Ohio, the duties of which he discharged with great fidelity, and his publications connected therewith constitute in a measure the bases of subsequent similar investigations in the West. His most prominent work was that in connection with the fishes of Ohio, for the accurate knowledge of which he laid the foundation, establishing and identifying many of the species of Rafinesque, which up to that time had been considered entirely mythical. He described many new species of western fishes, and the discovery of one species of bird of Ohio is due to his zeal.

AT a meeting held last week in Sheffield, a resolution was unanimously passed to invite the British Association to visit that town next year. Committees were appointed, and it was decided to raise a guarantee fund of 2,500*l*.

DR. SCHLIEMANN has been elected an honorary member of the Deutsche anthropologische Gesellschaft. The diploma of membership is a handsome specimen of artistic work, being encircled by allegorical representations of the excavations at Troy and Mycenæ, and bears the signatures of Virchow, Kollmann, Fraas, Schaafhausen, and Weismann.

THE new New York Natural History Museum was opened by President Hayes on the 22nd ult. The museum is in Manhattan Square, on a plot of land opposite Central Park, and the plan of the entire structure contemplates a colossal enterprise, which cannot be completed within the present century. It consists of buildings arranged in a parallelogram, of 850 by 650 feet, and of two lines of buildings which divide the interior space at right angles, thus forming four equal courts. At the centre of each side of the parallelogram, and at its four angles, lofty towers will be erected. At the intersection of the cross in the centre of the parallelogram, a dome is designed to cover a space of 120 feet diameter. The portion of this great structure which is now completed is a four-story building with a double attic; it has a length of 200 feet; it will form the southern one of the arms of the interior cross. The walls are three feet thick at the top. The whole building is of brick, and is regarded as strictly fire-proof. The city has appropriated $700,000 for it, besides giving the land.

MR. STANLEY will probably arrive in England this week. He has been received with enthusiasm at Rome, Marseilles, and Paris. The Chamber of Commerce and the Geographical Society of Marseilles presented Mr. Stanley with medals. No doubt our own Geographical Society will take the lead in the warm reception which will certainly be accorded in this country to one of the foremost of explorers.

THE Commission for reorganising the Observatory of Paris has ended its sittings, as we have already reported. The commissioners recommend no change in the present organisation of the International Meteorological Office; but, taking into consideration the actual wants of meteorology, it has advised the Minister of Public Instruction to appoint a Meteorological Commission, in order to suggest any measures which might be likely to promote the interests of meteorology at large, without interfering with the working of telegraphic weather forecasts sent by the International Office to the sea-ports and more than 1,200 parishes all over France.

THE Scientific Association of France, created by M. Leverrier, after having elected M. Milne-Edwards for its president, has decided upon organising a series of lectures at the Sorbonne, to describe important new inventions and discoveries. The first meeting will soon take place, when M. Cailletet's experiments will be exhibited and explained by M. Henri St Claire Deville. M. Dumas will also deliver an address summarising the history of the Association and reviewing the services rendered by it to science. It is stated that M. Dumas will propose to the Association to initiate a subscription for erecting a monument to the great astronomer who founded it.

IN several cities of Holland committees have been formed with a view to collect subscriptions to defray the expenses of the proposed expedition to the Arctic regions, to which we referred some time since.

MACMILLAN and Co. are about to publish "A Monograph on the Development of Elasmobranch Fishes," by Mr. F. M. Balfour, M.A., Fellow and Lecturer of Trinity College, Cambridge. The work is divided into twelve chapters, and contains the results of much original research on the part of the author, who, on certain points, as on the spinal and cranial nerves, advances views which are a modification of those previously accepted.

IT is announced that the *American Naturalist*, which has had so long and useful a career in Salem and Boston, will hereafter be published in Philadelphia by Messrs. M'Calla and Stavely, under the editorship of Dr. A. S. Packard and Prof. E. D. Cope.

REPORTS from the Island of Sylt, on the west coast of Schleswig-Holstein, state that the storm-flood, which caused such serious damage along the Continental shores of the German Ocean last autumn, has laid bare some remains of the village of Eidum, which perished in the year 1436 by the sea suddenly breaking over it and covering it up. Stone foundations of former dwellings, garden-walls, and wooden remains of various kinds are now seen there, also numerous well-openings, built of massive pieces of dried and baked peat. It is also stated that numerous old coins and utensils have been found there, as well as a well-preserved, carved, and engraved metal bracelet.

RECENT excavations made at Trèves, in the so-called Roman imperial palace, have yielded rich results in Roman antiquities.

ON December 26, at 8 A.M., two meteorolites fell near the village of Höhr (in the Prussian province of Hessen Nassau),

close to the high road leading from Neuwied to Coblenz. It is stated that the noise caused by the fall was very characteristic.

THREE earthquakes were observed at Kirlibaba, in the Bukowina, on December 28 and 30 respectively. A fourth phenomenon of this nature was felt at Innsbruck, in the Tyrol, on the 3rd inst., at 8 46 P.M.

BERLIN is listening to an interesting series of lectures delivered under the auspices of the Society for African Exploration. On the programme we notice Dr. Nachtigal, "Ancient Darfour;" Dr. Güssfeldt, "The Arabian Desert;" Dr. Hildebrandt, "Pictures from Equatorial Africa," Prof. Hartmann; "Fauna of the Swedish Islands," &c.

THE two African Societies at Berlin, which have hitherto existed independently of one another, have now finally resolved to unite into a single society.

WE have received the first number of *The Midland Naturalist*, the journal of the associated societies and clubs of the Midland counties, the union of which we referred to some time since. It is a neat and well-printed journal, containing seven good papers, besides miscellaneous matter. Besides the opening address, explaining the formation and objects of the Union, there are papers on abnormal ferns, by Mr. E. J. Lowe, F.R.S.; on an improved aneroid, by Mr. W. J. Harrison, F.G.S.; on the marine zoology of Arran, by Mr. W. J. Hughes (giving the results of an excursion by the Birmingham Natural History Society last summer); Lepidoptera in the Midland Counties, by the Rev. C. F. Thornewill; Entomostraca, by Mr. Edwin Smith; and a paper on some new features in the geology of East Nottingham, by Mr. J. Shipman. This is a very good start, and we hope *The Midland Naturalist* will fill a useful place in our scientific literature. Hardwicke and Bogue are the London publishers.

REPORTS from the Bernese Alps state that the amount of snow fallen during the present month is much greater than has been experienced for a number of years.

THE wolves in Eastern France have become unusually bold during this winter, and reports are constantly received of their depredations in various parts of the country. In one instance a letter-carrier was driven back by them from his regular route.

AN interesting experiment was lately carried out in the neighbourhood of Emmendingen, on the River Danube, to show its subterranean connection with the valley of the Rhine. The river is separated here by a range of Jura limestone from the district drained by the Rhine, and it has long been suspected that the Aach, which has its source in this range and flows into Lake Constance, was really supplied by the Danube. In order to solve the problem, recourse was had to fluorescin, the phthalein of resorein, a compound which yields with alkalies magnificent green fluorescent solutions capable of imparting this property to enormous masses of water. A solution of this substance was introduced into the Rhine at Emmendingen, and two and a half days later the bright green fluorescence was visible in the Aach, the source of which is about five miles distant, and lasted for thirty-six hours. This experiment shows most decisively that the Upper Danube shares its water between the Black Sea and the North Sea, and affords a most interesting explanation of the close similarity in the finny inhabitants of the two great European rivers.

WE notice the appearance, in Paris, of a French translation of the "Organic Chemistry" of Prof. Fittig, of Strassburg. This work, which in its present form is the tenth edition of the textbook originally issued by Prof. Wöhler, has long been a favourite with the German chemist on account of the scrupulous care and fidelity which have been exercised in preparing each successive edition. An English translation was prepared a short time since by Prof. Remsen, of Johns Hopkins University, formerly one of Fittig's assistants, the circulation of which is confined, however, chiefly to America.

ARRANGEMENTS are being made for the holding of an International Exhibition at Sydney in 1879, under the auspices of the Agricultural Society of New South Wales. It is anticipated that many of the articles shown at the coming Paris Exhibition will be trans-shipped to Sydney.

A propos of the remarkable relation established by Dr. Kerr, a short time ago, between light and electricity, an interesting experiment has been made by Mr. J. Mackenzie, in Berlin, at the instance of Prof. Helmholtz (*Pogg. Ann.*, No. 11). A glass plate, 161 mm. long, 12 mm. thick, and with tin foil on its opposite sides, from which proceeded copper wires to a Ruhmkorff coil (with six Bunsen elements), or a Holtz machine, and to earth, was supported and covered with larger glass plates, and placed between two Nicols, as in Dr. Kerr's experiment, the light-source being a lamp. The electric action gave no perceptible increase of brightness, nor was any such obtained when polarised sunlight was used to give greater sensibility, and a leaf of mica thick enough to give the violet colour was interposed between the glass plate and the analyser. Experiments with oil of turpentine likewise gave negative results. (The high sensibility of the polariscope is demonstrated by distinct experiments.) It is therefore concluded that the phenomenon observed by Dr. Kerr is not produced by electric tension itself, but possibly in a secondary manner, through the heating thus caused. Confirmation of this is found in the fact that in Dr. Kerr's experiments it was only after about thirty seconds from closure of the circuit that the action reached its maximum; it also disappeared slowly.

IN a paper in the *Bulletin* of the Belgian Academy of Sciences (Nos. 9 and 10), Prof. Van der Mensbrugghe discusses the causes of the seemingly spontaneous movements of bubbles of air in levels and of vaporous bubbles in the microscopical cavities of minerals, these researches being part of those into the tension of surfaces of liquids. Prof. Mensbrugghe explains these movements, as Mr. Hartley also does, by changes of tension in the surfaces of liquids produced by changes of temperature; when the temperature of the liquid at one end of the bulb becomes, for some reason, higher or lower than at the other end, however small the difference, the tension of the surface decreases at the warmer end, and the bubble moves towards it. But, a thin film of water remaining on the glass, the surface of the liquid is enlarged at the warmer end, and diminished at the opposite end, and this, according to experiments of the author, lowers the temperature and increases the tension at that end; so that if the temperature now ceases to rise the motion of the bubble is not only stopped, but the bubble also returns backwards. Thus each displacement of the bubble immediately gives rise to such forces as tend to produce a motion in an opposite direction; and the variations of tension produce the more obvious motions the smaller the masses of liquid in which the bubble is swimming. The same explanation may be applied also to the movements of bubbles in microscopical cavities of minerals filled with liquids. In that case, the bubble being produced by the vapours of the liquid, its movements are yet more rapid, as every change of temperature is followed by further evaporation of the liquid, or by condensation, both which alter the dimensions of the surfaces of the liquid and their tension. The author supposes also that the Brownian motions of powders suspended in liquids may be explained in the same way, and that those powders which absorb most gas will best display this kind of motion.

PROF. EMILIO CORNALIA, an eminent naturalist and Director

of the Museum of Natural History at Milan, has been decorated by the Emperor of Russia with the order of St. Ann for his efficient co-operation in the foundation of the institute for "Bachicoltura" at Moscow and Tashkend.

THE scientific expedition to Lake Lob-Nor, sent out by the St. Petersburg Geographical Society, under command of Col. Prjewalski, and to which we have already referred, has yielded most interesting results in every direction and is of particular importance with regard to the exploration of Kashgar. The new details obtained in reference to Lake Lob-Nor are remarkable. The expedition continued its way from Korla, following the course of the Tarim River down to its confluence with the Rokala Darja. On their way to the Lob-Nor the travellers passed the ruins of three cities. Lake Lob-Nor is of a marshy nature; its length is some 100 kilometres, by only 20 kilometres breadth. Col. Prjewalski explored the western and southern shores, and through the current of the Tarim River reached the middle of the lake. There the shallowness of the water and impenetrable vegetation prevented further progress; almost the whole surface of the lake is thickly covered with reedy vegetation. The inhabitants of the Kara Kurchintz district, on the shores of Lake Lob-Nor, are on the lowest step of civilisation. They live along the shores as well as on islands in the lake, in miserable huts constructed of reeds and branches twisted together. The whole of their possessions are their clothes, which barely cover their nakedness and are made of the fibres of a kind of lake weed, their nets, and their canoes, which are hollow trunks of trees. Metal objects, such as knives, hatchets, &c., are extremely rare among them. Col. Prjewalski, besides his ethnographical results, has collected rich materials for ornithological investigations. He reports that it is impossible to conceive the enormous number of migratory birds which, on their journey from southern countries to the north, or *vice versâ*, select Lake Lob-Nor as a halting place. At present the Russian traveller has wended his way southward and is engaged in the exploration of Tibet.

IN an interesting paper, published by M. Ph. Plantamour in the December number of the *Archives des Sciences Physiques et Naturelles* (Geneva), regarding the earthquake experienced in the immediate neighbourhood of the Lake of Geneva on October 8 last, the author proves most conclusively that the phenomena known under the name of "Seiches," and consisting in occasional and sudden alterations in the level of the lake, have nothing whatever to do with upheavals or depressions in the bed of the lake. During the earthquake referred to, not the least movement of the surface was perceptible, and had an alteration of only one millimetre taken place in the level, the instruments employed by MM. Plantamour and Forel, which continually register these alterations, would have most certainly shown them. The explanation of these "Seiches," therefore, is still a matter of considerable uncertainty, and it even remains to be seen whether barometrical pressure has any influence upon them or not.

TWO enterprising men in Paris, a merchant and a doctor of medicine, whose names will be surely blessed by future generations, have made the valuable discovery that the different elements contained in sea-water are infallible preservatives against all possible diseases, and at the same time are never-failing remedies against existing illnesses. These two philanthropists have therefore not only issued a seductive prospectus and widely circulated it in France and abroad, but have also prepared a large quantity of hygienic products, such as bread, biscuits, dry cakes of all descriptions, liqueurs, &c., which are all prepared with sea-water, and are endowed with the most marvellous healing properties. In the prospectus it is stated distinctly that the use of these preparations renders all other medicines or medical treatment unnecessary. There is only one little point which requires explanation. The "inventors" state that their preparations are made with distilled sea-water; we would ask them what becomes of the mineral and organic matter contained in sea-water during this distillation? But *mundus vult decipi!*

THE additions to the Zoological Society's Gardens during the past week include a White-handed Gibbon (*Hylobates lar*) from the Malay Peninsula, a Brown Monkey (*Macacus arctoides*) from Burmah, presented by Mr. W. H. Newman; two Black Francolins (*Francolinus vulgaris*) from India, a Chukar Partridge (*Caccabis chukar*) from North-West India, presented by Major Newton Pauli; four Common Marmosets (*Hapale jacchus*) from Brazil, deposited; two Rough Terrapins (*Clemmys punctularia*), a Scorpion Mud Tortoise (*Cinosternon scorpoides*) from Trinidad, purchased.

CERTAIN MOVEMENTS OF RADIOMETERS[1]

THIS morning (Dec. 20) I received from Mr. Crookes an account of the behaviour of a kind of radiometer which he was so good as to construct at my suggestion. The consideration of an experiment mentioned in a paper of his presented to the Royal Society, which will shortly be read, and which he has kindly permitted me to refer to, suggested to me the desirability of investigating the effect of mere roughness of surface, all other circumstances being alike, and the disc of the radiometer being metallic, so that the two faces may be regarded as practically at the same temperature. Mr. Crookes's experiment, above referred to, led me to suspect that mere roughness might increase the efficiency of a surface, and I suggested to him some experiments with heated glass shades, or with a hot poker presented to the radiometer, the bulb being covered with a cool tumbler to defend it from being heated by the rays easily absorbed by glass. The result in every case answered my expectation; and it may be stated shortly that the law of the motion is that when the fly is hotter than the bulb the rough surface is repelled, or, say, the motion is positive; when cooler, negative.

I subjoin Mr. Crookes's memorandum of the results of experiment:—

"*Aluminium Radiometer* (1326), *one side of the vanes being ruled closely with a sharp knife.*

"1. Exposed to standard candle three inches off. Continuous *positive* rotation (ruled side repelled) at rate of $3\frac{1}{4}$ revolutions a minute.

"2. Exposed to non-luminous flame of a Bunsen burner three inches off. Continuous *positive* rotation at the rate of $7\frac{1}{2}$ turns a minute.

"3. The Bunsen burner removed. The positive rotation gradually diminished till it stopped. No negative rotation.

"4. The bulb heated with Bunsen burner. Good *negative* rotation; then stopped, and rotated *positively* till quite cold.

"5. Bulb covered with a cold glass shade, and a large red-hot ring applied round equatorlally. *Positive* rotation, but not very strong.

"6. On removing the shade and ring the positive movement soon comes to rest.

"7. Covered with a hot glass shade, *negative* rotation, with *positive* rotation on cooling (the same as 4).

"8. Plunged into hot water. *Negative* rotation.

"9. Removed from the hot water, and immediately plunged into cold. *Positive* rotation."

Results nearly identical were obtained with another radiometer described as "silver radiometer (1327), one side coated with finely divided silver, electro-deposited."

We must accordingly recognise three distinct conditions under which motion may be obtained in a radiometer, namely, (1) difference of temperature of the two faces, as in a pith radiometer, coated on one face with lampblack; (2) more favourable presentation of one face than the other, as in a radiometer with curved disks; (3) roughness of surface on one face (if this be really different from 2). These three conditions may be variously combined so as to assist or oppose each other, as the case may be, in producing motion.

[1] Paper read at the Royal Society, December 20, by Prof. G. C. Stokes, Sec. R.S. Continued from p. 175.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

LONDON.—At a meeting of Convocation of the University of London held on Tuesday, the supplemental charter empowering the granting of degrees to women was considered. After a long and warm discussion a resolution appoving of the draft of the supplemental charter was carried by 242 against 132.

EDINBURGH.—A letter has been received from the Treasury intimating that 20,000*l*., the first instalment of the grant by the Government for the buildings of the University of Edinburgh, will be inserted in the estimates for this year.

LEEDS.—A course of ten lectures in connection with the Gilchrist Educational Trust will be delivered in the Albert Hall, Mechanics' Institution, Leeds, on Friday evenings, commencing Friday, January 25, by Prof. A. H. Green, M.A., on "The Geology of Coal;" Prof. L. C. Miall, F.G.S., on "Coal Plants and Animals;" Prof. T. E. Thorpe, Ph.D., F.R.S., on "The Chemistry of Coal;" Prof. A. W. Rücker, M.A., on "Coal as a Source of Power;" and Prof. J. Marshall, M.A., on "The Coal Question." An extra lecture will be given by Dr. W. B. Carpenter, F.R.S., on the "General Results of the *Challenger* Expedition." The admission is one penny.

HALLE.—The winter attendance at the University is 887, including, under theology, 189, law, 112, medicine, 106, philosophy and science, 480. Prussia is represented by 711. The attendance of foreigners is unusually small—England, 2, America, 5, Russia, 11, Austria, 20, &c. The corps of instructors numbers at present 90. The University library, one of the most valuable in Germany, possesses over 100,000 volumes.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, December 13, 1877.—On electrostriction, by Prof. Mills, D.Sc., F.R.S. If the bulb of an ordinary thermometer be coated chemically with silver, and then electrically with a metallic deposit, the mercury will traverse some portion of the scale, and finally take up a definite position independently of temperature. Of the metals hitherto worked with, copper, silver, iron, and nickel, constrict the bulb; zinc and cadmium distend it. The author shows that if y be the total obtainable effect after a time x; D the portion of it due to diametral constriction; L the portion of it due to longitudinal constriction; $d\,l$ two geometrical factors, we have, in the case of the cylindrical thermometer—

$$y = Dd^x + Ll^x,$$

D being always greater than L. For a spherical thermometer receiving more metal on its equatorial region than on its poles,

$$y = Dd^x - Ll^x.$$

For a spherical thermometer, with uniform deposition,

$$y = Dd^x.$$

The author determines in atmospheres pressure the total electrostrictive effect; and points out that, since the deposited metal can be removed by a chemical solvent, we are thus able to measure chemical effect in atmospheres pressure.

Linnean Society, December 20, 1877.—Prof. Allman, F.R.S., president, in the chair.—Dr. Maxwell Masters made some remarks on an interesting specimen of *Colletia cruciata* received from Sig. Fenzi, of Florence. In this case, from the same branch there proceeded shoots with broad, flattened, deltoid spines characteristic of *C. cruciata*, but also others with slender or cylindrical spines very similar to, but more cylindrical than, those of *C. spinosa*. It would thus seem this interesting specimen may tend to clear doubts which have arisen respecting the relation of these two species and that of *C. bictonensis*, Lindl. = *C. cruciata*, Hook.—Mr. Worthington G. Smith made some remarks on a fossil fungus, its zoospores being shown under the microscope. He also exhibited drawings, among others sections of *Boletus subtomentosus*, stating that in a specimen five inches in diameter there are 17,000 pores or tubes. Each pore, when cut across, shows 2,000 cells on the surface. The number of surface cells on the underside of a specimen is 36,000,000. The cells in an entire plant are calculated to be 61,500,000,000, and the number of spores produced by the same specimen, 5,000,000,000.—Mr. S. W. Silver exhibited a series of vegetable products, arrows, and other weapons, &c., from the Fiji Islands and New Caledonia, collected by Consul Edgar Layard. Among the specimens was a mass of the poison said to tip the native arrows with. The composition of this is supposed to be identical with that described by the Rev. Thomas Powell in the Society's *Journal* of last year.—A paper was read on the anatomy of the Elk (*Alces machlis*) by Prof. M. Watson and Dr. A. H. Young. In this a full account of the organs of digestion, generative system, myology, &c., was given, preceded by remarks on the literature, &c., of the subject.—An abstract of a communication, "Descriptions of new genera and species of phytophagous coleoptera," by Dr. J. S. Baly, was read by the secretary in the absence of the author.—The Algæ of the Arctic expedition, by Prof. Dickie, was a paper dealing with the collections made by Capt. Feilden, Dr. Moss, and Mr. Hart, who accompanied Capt. Sir G. Nares. It is noted that of fresh-water species there are representatives of fourteen genera, many of which are common to Europe. Of Diatomaceæi thirty-one genera and seventy species have been identified, most being marine. Seven species of olive-coloured Algæ are given, but it seems no marine examples belonging to the red series were obtained. The collection embraces an area between 78° and 83° north latitude. Then followed a memoir on the minute structure of *Stromatopora* and its allies, by Prof. A. Nicholson and Dr. J. Murie. This interesting form (or group) has long been a puzzle; different writers assigning it a place respectively among Corals, Hydrozoa, Foraminifera, Sponges, and Polyzoa. The authors treat the subject by discussing at length history and literature, the general and minute structure of a typical stromatoporoid, mode of occurrence and original constitution, classification, affinities, and systematic position. The following genera are defined:—*Stromatopora*, *Caunopora*, *Clathrodictyon*, *Stylodictyon*, *Stromatocerium*, *Pachystroma*, and *Dictyostroma*, and a number of new species described. They believe it (or them) to have been originally calcareous and not siliceous, as has been maintained by some, substantiating this by weighty facts and reasons. They discard the notion of its alliance with the Nullipores or belonging to the Corals, Hydrozoa, or Foraminifera, showing wherefore in absolute essentials it is deficient and therefore untenably associated with either. To certain of the Polyzoa some examples hold a striking resemblance in many respects (as likewise is specially the case with certain of the corals), and possibly further research may bridge difficulties in the way of classing it with the former group, but their researches do not completely justify this step. Neither, strictly, does it belong to the horny, siliceous, or calcareous sponges, as at present understood, though the tendency of the data point to the probability of sponge organisation predominating. In this case, however, by absence of spicules, &c., the existing group of *Calcispongiæ* could not contain the stromatoporoids which, under negative evidence, would form a new order of calcareous sponges—Stromatoporoidea.—Messrs. A. S. Bicknell, E. A. Floyer, and Capt. Legge were elected Fellows of the Society.

Meteorological Society, December 19, 1877.—Mr. S. H. Eaton, M.A., president, in the chair.—Commander E. G. Bourke, R.N., J. A. Douglas, W. H. La Touche, B.A., G. J. Pearse, W. S. Rogers, and W. Tyrer were elected Fellows.—The following papers were read:—Notes on the meteorology and physical geography of the West Coast of Africa from Cape Verd to the Cape of Good Hope, by Commander E. G. Bourke, R.N. This paper gives the results of the observations which the author made during the five years he was stationed on the above coast.—On the meteorological observations made by the Norwegian Deep Sea Exploring Expedition in the North Atlantic in 1876 and 1877, by Prof. H. Mohn. This expedition has been organised in order to carry out for the North Atlantic and the Arctic Ocean an inquiry similar to that conducted by the *Challenger* Expedition. The vessel employed was the *Vöringen*, of 400 tons burthen, and the period the summer months of 1876 and 1877. The barometrical observations were taken at first with a mercurial barometer and afterwards with an aneroid which was compared daily with the mercurial barometer on board. The temperature was obtained by a special screen hoisted up on the fore-stay. It was found that this gave very satisfactory resnlts. The experiments conducted with a screen similar to that used by our Meteorological Office on ship-board gave readings too high when the sun shone on it. The sling thermometer was also tried, and gave a temperature on the mean a shade below the screen in the rigging. The wind observations were taken with an anemometer, and Prof. Mohn

describes his own anemometer at length, and deals with its corrections in detail. The speed of the ship was determined by a special logging machine, and by this means and the anemometrical observations, the true motion of the wind was ascertained. The part of the paper which presented most novelty was that referring to the evaporation of the sea-water. Two different forms of atmometers were described, both of them devised by Prof. Mohn, and the theory of their action and of the errors to which the experiments were exposed are carefully considered. The paper concluded with tables of the diurnal range of the various meteorological elements for the period of observation.—Report on the phenological observations during 1877, by the Rev. T. A. Preston, M.A. As a rule, the same order of flowering of plants is observed this year as in 1876, viz., that plants came into flower first in the south-west of England and then in regular order to the north of Lincolnshire, where plants were latest in coming into flower. From the tables accompanying the report may be deduced the general state of the weather as regards temperature, and to a certain extent moisture. There is no doubt but that damp acts more powerfully than cold in retarding the flowering of some plants and this has been particularly evident this year. The year, as a whole, has been very unfavourable to vegetation; the bitter cold of May checked the growth of plants, and by the autumn there was comparatively little new wood, and that not properly ripened.—Note on a peculiar fog observed at Kew on October 18, by G. M. Whipple, B.Sc., F.R.A.S.

Royal Microscopical Society, January 2.—Dr. J. Millar in the chair. A paper was read by Dr. Bartlett on the detection of toxic matter connected with typhoid and other enteric diseases, in the course of which he gave an account of his attempts to trace to its ultimate source the cause of a recent outbreak of typhoid fever, and showed that whilst chemical analysis had failed to discover any impurity either in the water or milk, he had been able, by means of microscopical examination, to detect in the water certain bodies, presumably of a fungoid character, which were identical with those found in the bowels of persons who had succumbed to the disease.—Mr. Slack brought before the notice of the meeting a section of bone of *Megalosaurus bucklandii* and its remarkable resemblance to the structure now identified as peculiar to birds, was pointed out by Mr. Charles Stewart.

Paris

Academy of Sciences, January 7.—M. Fizeau in the chair. —M. Daubrée was elected vice-president, from the Section of Physical Sciences (the other candidates being MM. Wurtz, Chevreul, and Blanchard).—M. Peligot reported on volumes just published, or being published, by the Academy. Vol. xxxix. of the *Mémoires* is devoted chiefly to researches by M. Chevreul, vol. xli. to researches by MM. Becquerel; a second volume on the transit observations, relating those at Pekin and St. Paul's Island, has been published; a memoir on *Phylloxera vastatrix*, by M. Cornu, appears in vol. xxvi. of the *Mémoires des Savants Étrangers*. The Academy lost by death, in 1877, one member, M. Leverrier, and five correspondents, MM. Santini, Hofmeister, Braun, Weddell, and Gintroc.—M. Faye presented, in the name of the Bureau des Longitudes, the first volume of its *Annales*. In these *Annales* will be inserted, with additions, the memoirs which the Bureau formerly published in the *Connaissance des Temps*, its circle of activity having been enlarged.—On persulphuric acid, a new oxygenated acid of sulphur, by M. Berthelot. This is obtained pure and anhydrous, by making the electric *effluve* act, with strong tension, on a mixture of equal volumes of dry sulphurous acid and oxygen; it is got in the dissolved state by electrolysis of concentrated solutions of sulphuric acid, or by mixing with care a solution of oxygenated water with sulphuric acid, concentrated, or diluted with less than one equivalent of water. At a temperature near zero, it crystallises, and resembles, in its general aspect, anhydrous sulphuric acid, only it has longer, and thinner, and transparent needles. The formula, determined variously, is S_2O_7. Heated in a flame, the substance is immediately decomposed into oxygen and anhydrous sulphuric acid. In air it gives off thick fumes. In concentrated sulphuric acid it dissolves without liberating oxygen. In water it dissolves, giving thick fumes and effervescence, &c.—On a new flat regulating spiral for chronometers and watches, by M. Phillips. The theory of this is explained.—On some new modifications in the telephone, by M. Breguet. According to indications by MM. Garnier and Pollard, a thin plate of sheet iron is arranged with the end of a blacklead pencil pressing slightly on the central part; plate and pencil are connected by wires of ordinary lines with the two ends of the bobbin wire of a Bell telephone, which has, instead of the magnetic bar, a bar of soft iron. A battery of two Laclanché elements is placed in the circuit. The plate, vibrated by the voice, causes variations in the blacklead, and so in the resistance of the circuit and the intensity of the permanent current, which produces alternative attractions and non-attractions in the electro-magnet of the receiving telephone; thus the voice is reproduced. M. Breguet is hopeful of an increased intensity of effect by such a method.—On the production and properties of a new suction-ram without air-reservoir, capable of drawing water from all depths, by M. de Caligny.—Density of liquid oxygen, by M. Pictet. The author experimentally confirms M. Dumas' view, who obtained the expression $\frac{16}{16} = 1 = \delta$, for the solid, and probably the liquid state also. The jet of oxygen showed a strong polarisation of the electric light, indicating the presence of solid dust, probably small crystals of solid oxygen.—On the quartic of Steiner, by M. Amigues.—On a single principle containing the whole theory of curves and of surfaces of any order or class, by M. Serret.—On a theorem of M. Villarceau; remarks and consequences, by M. Gilbert.—On phenomena of dispersion in metallic reflection of polarised luminous or calorific rays, by M. Mouton. The greater the wave-length the longer is the interval during which mirrors act like glass on light, simply impressing a certain rotation in the original plane of polarisation, and the shorter therefore is that in which the original rectilinear polarisation of the incident ray is changed by the fact of reflection into elliptic polarisation.—On normal ethyloxybutyric acid and its derivatives, by M. Duvillier.—Researches on the intracellular alcoholic fermentation of plants, by M. Muntz. Plants kept in air give no trace of alcohol; those kept in nitrogen give a quite appreciable quantity, and they continue to live and grow. These facts are a confirmation of M. Pasteur's views.—On the inversion and alcoholic fermentation of cane-sugar by mouldiness, by M. Gayon.—Some remarks on the origin of alcoholic yeast, by M. Trécul.—Verbal response of M. Pasteur.—On a new gorilla from Congo, by MM. Alix and Bouvier. This seems, like chimpanzees, to have more arboricolar habits than the *Gorilla gena*. The name of *G. Mayema* is given it from that of the negro chief of the village near which it was killed.—On the formation of fibrine of the blood studied with the microscope, by M. Hayem.—On a process for obtaining recomposition of the light of the solar spectrum, by M. Lavand de Lestrade.

CONTENTS

THURSDAY, JANUARY 24, 1878

RAYLEIGH'S "THEORY OF SOUND"

The Theory of Sound. By J. W. Strutt, Baron Rayleigh, F.R.S. Vol. I. (London: Macmillan and Co., 1877.)

THE author, who already, by a series of interesting treatises belonging to different branches of mathematical physics, has acquired a respected name in the domain of science, undertakes to give a complete and coherent theory of the phenomena of sound in the work above mentioned, the first volume of which has recently been published; and he does this with the application of all the resources furnished by mathematics, since without the latter a really complete insight into the causal connection of the phenomena of acoustics is altogether impossible. We must confess that, even in spite of the most intense exertion of the powers of mathematical analysis, in the present state of its development several problems remain unsolved, for which, indeed, the conditional equations are known, but for which it has not yet been found possible to carry out the calculation.

The author will merit in the highest degree the thanks of all who study physics and mathematics if he continues his work in the manner in which he has begun it in the first volume. The separate treatises in which the acoustic problems that have been solved hitherto are discussed, are for the most part dispersed in the publications of academies or of scientific societies, which can be found only in larger libraries, and which frequently are not at all easily traced. But even if one has found a treatise of this kind and reads it, it happens often enough that the author refers in his quotations to other works quite as difficult of access, the knowledge of which is necessary for understanding his treatise. Thus the zeal of the student is paralysed by a number of purely external difficulties, and the ordinary result at which an intelligent student arrives after a few attempts in this direction, is that for problems in which he takes great interest he prefers starting anew to find the solution, rather than trying to hunt for it in libraries. Even if we must admit that the insight into the essence of a problem for which one has found the solution oneself is much deeper and clearer than when one has obtained the solution from some other author, yet an enormous amount of time is thus lost, and the survey of the whole extent of solvable problems remains incomplete. A survey of this kind, however, is necessary for all who wish to work at the progress of science themselves. For in order to obtain decisive results by new scientific investigations it is necessary above all things to be quite clear with regard to the question for which forms of experiment or of observation the theoretical deduction from principles can be carried through as purely as the experiment itself. I know by experience that a number of young physicists lose their time and their zeal by trying to solve problems which, taken by themselves, are very interesting, but for which at present the deductions from the theoretical principles for the given case can only be drawn in coarse approximation, and where the experiments cannot be freed from important sources of error.

While praising Lord Rayleigh's book as a means of overcoming the difficulties described, I do not at all wish to designate it as a mere compilation. On the contrary, it is a perfectly coherent deduction of the special facts from the most general principles, according to a uniform method and in a consequent manner. The mechanical principles of the doctrine of minute oscillations are contained in the present volume and are developed in greater generality than in any other book known to me. For this purpose the author in the first chapter explains the general physical principles of sound, of its propagation, of pitch and its dependence on the rapidity of vibration, of the musical scale, of the quality of sound and its dependence on the harmonic over-tones; and in the second one the doctrine of the composition of harmonic motions of either equal, or nearly equal, or consonant numbers of vibrations, and further illustrates them by the description of the physical phenomena and methods in which the principles developed are applied, and to which belong the doctrines of musical beats and of the physical methods to render the forms of vibrations visible.

Then follows the development of the most general peculiarities of oscillating motions, first, in the third chapter, for mechanical systems to the motion of which only one degree of freedom is allowed, and then, in the fourth chapter, for systems with a finite number of degrees of freedom. There is a great multitude of peculiarities common to the most heterogeneous sounding bodies, which up to the present have mostly been found in certain instances only, but which can also be deduced from the most universal form of the motion-equations of systems of one or more degrees of freedom of motion. The author in the form of the equations and in the manner of denotation, closely follows the "Natural Philosophy" of Thomson and Tait; in fact the whole manner of treatment of the mathematical problems corresponds so closely to that adopted in the work just mentioned, that Lord Rayleigh's book may be looked upon as the acoustic part of the excellent handbook of the two celebrated physicists named.

With all systems of this kind if there are no exterior forces acting upon them, we find, on the whole, a number of proper tones equal to the number of degrees of freedom, and the pitch of which does not depend on the amplitude of the vibrations as long as this one remains small enough. Exceptionally, however, several of these proper tones may be of equal pitch. If there is no friction or dissipation of energy the amplitude of every kind of oscillation remains constant. To each separate proper tone a certain form of motion of the whole system belongs; so that the directions and magnitudes of the displacement of the separate points of the system are different in each case. Each arbitrary motion of the system produced in any arbitrary manner, may be regarded as a superposition of these forms of vibrations belonging to the various proper tones of the system. In order to find the amplitude and phase of these different vibrations for a given original displacement and of given velocities of its different parts, quite similar methods are adopted as those which are employed to develop a given periodical function into one of Fourier's series; only the whole method here becomes far more intelligible and has a thoroughly certain foundation, because we have to do with a finite number of unknown factors instead of with the infinite number of continuously

succeeding values of a function, with finite sums instead of with integrals or with infinite series. Of course for Fourier's series as well as for the developments of Laplace by means of spherical harmonic functions the proof for the correctness of their values can also be furnished in the case of continuous functions. For a large number of other functions which are given by differential equations of the second degree this proof results, under certain suppositions regarding the continuity of the functions and the limit conditions, from the theorems of Sturm and Liouville, which Lord Rayleigh explains when speaking of the vibrations of strings of unequal thickness. Yet in mathematical physics we are still compelled to employ a great number of series-developments of functions which do not belong to this class; and even the vibrations of rods and plates are cases in point. In this respect the treatment of the problems mentioned with a finite but arbitrarily large number of degrees of freedom of motion is interesting also with regard to analysis.

For vibrating systems of one degree of freedom, the oscillations of which are subjected to damping, the doctrine of the laws of resonance is developed in the third chapter. The author calls the vibrations which are continuously maintained by the influence of a periodical force acting externally, *forced vibrations*. In all cases their intensity is greatest when their period of vibration, which equals the period in which the force changes, is also equal to the period of the system vibrating freely and without friction. For the relations between the intensity and the phase of the co-vibration, between the breadth of the co-vibration in case of small alterations in the pitch and the degree of damping, which I had myself proved for certain instances and used for certain observations, the general proof is given here. The author has further employed these chapters to set up certain general maxims respecting the direction and magnitude of the corrections which must be made in cases where one cannot completely solve an acoustic problem, but can only find the solution for a somewhat altered vibrating system. These are like the outlines of a "theory of perturbations" applied to acoustic problems. The author illustrates these maxims by many various examples. Thus, for instance, he replaces a string by an imponderable stretched thread which carries weights either in the middle only or at certain distances from each other; or a tuning-fork by two imponderable springs with weights at the ends.

For vibrations of very small amplitude, the forces which tend to lead the moving points back to their position of equilibrium may always be considered proportional to the magnitude of their distance from the position of equilibrium. As long as this law holds good, the motions belonging to different tones are superposed, without disturbing one another. But when the vibrations become more extensive, so that the law of proportionality just named no longer applies, then perturbations occur which become manifest by the appearance of new tones, the combination tones. In my book on acoustic sensations ("Die Lehre von den Tonempfindungen") I have myself explained this manner of origin of the combination tones, only for the motion of but a single material point. In Lord Rayleigh's book this explanation is given with reference to any compound vibrating system of one degree of freedom, and it is further amplified with regard to the manner in which the forces deviate with the displacements from the law of proportionality.

Certain laws of reciprocity, of which I had given single instances in my investigations on the vibration of the air in organ pipes, may be proved in a general way for all kinds of vibrating elastic systems. If on the one hand at point A an impulse is given, and the motion at point B is determined after the time *t* has elapsed, and if on the other hand an impulse is given at point B in the direction of the motion which occurred there, and, after the time *t*, the motion-component falling into the direction of the first impulse is examined at point A, then the two motions in question are equal if the impulses were equal.

Chapters VI. to X. of Lord Rayleigh's book treat of the vibrations of strings, rods, membranes, and plates. The vibrations of strings have played an important part in acoustics; their laws are simple, and the physical conditions which the theory demands are fulfilled with comparative facility, different modes of producing the tones may be employed, and a number of various motions may thus be produced. It is just because the physical phenomena in connection with strings were well known, that the observation of the way in which the ear is affected by their various modes of vibration has materially facilitated the solution of the problems of physiological acoustics. The musical importance of strings rests on the circumstance that the series of their proper tones corresponds to that of the harmonics, the vibration-numbers of which are entire multiples of those of the fundamental tone. For this reason, if the motions of many proper tones are superposed on one string, a periodical motion again results, and this is the cause why on strings we can produce notes of the most varied quality. We need only remember how differently the same string sounds according to whether it is plucked with the finger or with a metallic point, whether a violin bow is drawn across it or whether it is caused to vibrate by means of a tuning-fork

In this chapter less new work remained to the author; however, this example shows how much easier it is to understand all these separate problems if they are not treated separately but developed in coherent representation, after the most general principles, the validity of which is independent of the special peculiarity of the case, have been first explained.

The short chapter VII. gives the laws for the longitudinal and torsional vibrations of rods; the laws are simple and resemble those of the open and stopped organ pipes. The lateral vibrations of rods, during which these bend, give more complicated analytical expressions; their proper tones do not form a harmonic series, but are given by the roots of a transcendental equation. The tones are different according to whether one or both ends of the rod are free to rotate and to move, or free to rotate, but hindered from moving (supported), or hindered from rotating and moving (damped). With this more complicated problem the advantage of first treating of the general principles becomes clearly apparent. The forms of the simplest vibrations are calculated and represented graphically. The mode of vibration of a stretched rod, for which Seebeck and Donkin have already given the solution, is also treated here in order to determine the influence of rigidity upon the vibrations of strings.

Then the vibrations of a uniformly-stretched membrane are investigated. This investigation is of more theoretical than physical importance, since it shows in a case which may be treated in an easier way, the peculiarities of vibrations which are capable of spreading in two dimensions. Unfortunately we have not yet succeeded up to the present in obtaining good membranes which would be fit for experiments of measuring in order to investigate, with some degree of exactness, how far theory corresponds with the experiment.

On the contrary, in the case of elastic plates, the vibrations of which the author treats in the last chapter of the present volume, the experiments can be made with more accuracy, while the analytical difficulties are so great that, on the whole, only few cases permit of a solution of the problem. Indeed, even the formulæ expressing the conditions which must be fulfilled at the edge of the plate have given rise to discussions. Poisson had thought that three conditional equations were necessary for the edge; Kirchhoff has shown that in reality only two are required. Lately M. Mathieu opposed this view. Lord Rayleigh has adopted Kirchhoff's views, and no doubt with perfect right. He gives the analysis of the latter of the vibrations of a circular plate, and has made an important addition of his own to the solvable cases, by teaching us how to deduce theoretically a series of vibration forms of square plates, at least for that case where they consist of an elastic substance the resistance of which to change of volume may be neglected; and these theoretical deductions sufficiently correspond with the forms observed. Also for elastic rings and for cylinders vibrating in the manner of bells, he has improved the theory in an essential point, by proving theoretically and experimentally, that the node lines of such plates execute vibrations in a tangential direction. These tangential vibrations are the ones which are first produced if the edge of a drinking-glass is rubbed with the wet finger.

The above survey will give an idea of the numerous contents of the book. As in the treatment of the separate problems it touches everywhere the limits of our present knowledge, it cannot but demand sound mathematical knowledge on the part of the reader. Yet the author has rendered it possible, by the very convenient systematic arrangement of the whole, for the most difficult problems of acoustics to be now studied with far greater ease than hitherto. He thus proves himself to be a philosopher who does not lose the liberty of intellectual supervision, even when he is occupied with the most abstruse calculations. H. HELMHOLTZ

HINDUISM, BUDDHISM, AND ISLÁM

1. *Hinduism.* By Monier Williams, D.C.L. 2. *Buddhism.* By T. W. Rhys Davids. 3. *Islám and its Founder.* By J. W. H. Stobart, B.A. (London: Society for Promoting Christian Knowledge, 1877.)

IT is a sign of the times that a "Society for Promoting Christian Knowledge" should undertake a series of works on "non-Christian religious systems." Nor is it a less striking characteristic of our day that it should entrust the work to scholars of so liberal a faith as Prof. Monier Williams and Mr. Rhys Davids. Dr. Monier Williams's volume on Hinduism forms a model of a popular exposition of a religious system. He explains with great clearness the historical *catena* of the sacred writings on which the Sanskrit religion was based. He delineates the various movements, from the Buddhistic reformation 2,500 years ago, down to the modern revivals among the Sivaite and Vishnuvite sects, which have developed the Vedic worship into that complex structure of ritual, dogma, and social institutions, which we call Hinduism. To the ordinary English reader, this little volume will reveal a different aspect of Hindu faith and morals from that which he has been accustomed to hear from the pulpit or missionary platform. He will find that the great questions of how a man should rule his life, and what prospect lies before him after he has done with this world, have formed the subjects of religious thought and practical experience, not less anxious nor less deep among the people of India than among the western races. The ethical replies which they have given to those questions differ more in form than in spirit from the higher beliefs of Christendom. The hard and narrow judgments of the elder Mill and the zealous statements of missionaries, have too long possessed the popular mind. Prof. Williams does not appear as the advocate or admirer of Hinduism. In fact he tacks on to the end of his book a proselytising page or two, which, however in accord with the object of the Society for which he writes, form a strange contrast to the scholarly tone of his volume.

Dr. Monier Williams's tours in India have enabled him to deal with the modern phases of Hinduism from a practical, as well as from the professorial point of view. Three features of Hinduism must be distinctly realised in order to understand its vitality and influence on the people. Hinduism represents, in the first place, a very ancient growth of worship and belief; and is invested with all the authority of age and unquestioned prescription. In the second place, it is a very modern religion, whose later developments have neither been reduced to, nor are restrained by, any systematic theology; which is, therefore, plastic, sensitive to every change in the popular beliefs or modes of thought; and which, in each province of India, takes on a local colouring adapted to the necessities or customs of the local population. In the third place, Hinduism is not only a religious system, but an all-powerful social institution. It forms the outcome of religious and philosophical thought in India during several thousands of years; it also represents the organised modes of life at which a great variety of tribes and races have slowly arrived. This threefold source of strength makes itself felt in every detail. To take one instance: Caste is enforced alike by the sanctions of ancient prescription, modern religion, and social utility. It articulates the population into communities, each bound together by ties of a useful, not less than of a doctrinal sort. Caste, with the feelings of kindred and family on which it rests, forms the substitute for a Poor-Law in India; it supplies a bond something like that which in America is felt by people who attend the same meeting-house or chapel; and it discharges many of the functions of the mediæval guilds of Europe, together with others which are effected less smoothly by modern Trades' unions. It has its disadvantages—some of them very serious ones; but it curiously resembles, in several of its judicial, social, and charitable aspects, the ekklesia of the early Christians.

Prof. Monier Williams might not accept this view, but we recommend his book as at once a scholarly and a practical exposition of Hinduism, in a cheap and popular form.

Mr. Rhys Davids has done his work well, but with a difference, in his little volume on Buddhism. He has rightly separated the facts (so far as we can ascertain them) of the history of the founder from the modern legends regarding him. He gives a careful and interesting narrative of the life, explains the doctrines which Gautama Buddha taught, and the system of morals which was subsequently based upon his precepts and example. Nothing could be better than some of the passages which bear upon the aspects of Buddhism in Ceylon, China, and Tibet. But it is to be regretted that the plan of the work permitted of so little space for its influence upon the mediæval forms of Indian ritual and belief. One of the most interesting pictures which we possess of a struggle between two great faiths is to be found in Hiouen Thsang's itinerary through India in the seventh century. The narratives of the Chinese travellers form, indeed, the first historical evidence of eye-witnesses with regard to Indian manners and beliefs. They supply a key to the subsequent religious developments among the Hindus, and well merit a fuller notice. Another point of deep interest on which Mr. Rhys Davids' volume is, perhaps necessarily, silent, refers to the industrial aspects of Buddhism. It is well known that architecture in India began with the requirements of Buddhism, and that those requirements profoundly affected its whole subsequent history. Moreover, the Buddhist monks were not only missionaries; they were artists, or at any rate artisans, who carried a new civilisation as well as a new faith to the Asiatic races. Thus it was a Buddhist monk of Corean ancestry who, between 662 and 672 A.D., published the secret of making translucent pottery in Japan. The ritual of Buddhism stamped its influence on the characteristic national industry both of Japan and China; and as late as 1212 we hear of a celebrated Japanese potter, accompanied by a Buddhist monk, going on a mission to the mainland to acquire the deeper mysteries of ceramic art. The vast number of Buddhist records did much to develop the art of writing, while the circumstance that its theology centres around a single human life, gave a biographical and historical impulse to the nations who adopted it, which is unknown among the followers of the older Bráhmanical faith. Mr. Rhys Davids' book is silent on these points. But it is only just to him to add that he has managed to compress a vast amount of thought and information, of a kind perhaps more important from the Society's point of view, into his 250 pages.

Mr. Stobart's Islám is conceived in a less philosophical spirit. "Light and darkness," he says, "are not more opposed than the loving doctrines of the Gospel and the vengeful spirit of the Koran." "Darkness and retrogression are engraved on every page of the Preserved Book." This is his conclusion of the whole matter, but it fails to explain the secret of one of the great historical movements which has deeply influenced mankind. Scraps of piety are scattered throughout the book, sometimes with a curious effect. Here is Mr. Stobart's conception as to how a chapter on the Ancestry of Mahomet should begin:—"We have the assurance that Noah was 'a perfect man and walked with God' (Gen. vii. 9); and as a 'preacher of righteousness' (2nd Peter, ii., 5), having with his sons been witness of the flood, handed down to his posterity the worship of the True God." Further quotation is unsuitable. Mr. Stobart's book will supply a convenient but misleading compendium for those who wish to know a little about the subject. It reproduces the bigotry which disfigured Sir William Muir's "Life of Mahomet," on which it is chiefly based, without the scholarship which rendered that Indian civilian's four volumes the standard English work on Islám.

OUR BOOK SHELF

Physical Chemistry. By N. N. Lubavin. Part II. 8vo. 460 pp. (Russian). (St. Petersburg, 1877.)

We are glad to notice the appearance of the second and last part of M. Lubavin's most valuable work, which is devoted to the most important departments of physical chemistry. In this part the author deals with chemical reactions in general and discusses under this head some of the various theories advanced as to the distinctive characters of chemical processes; the stechiometrical laws of Dalton, Gay-Lussac, Faraday, Dulong and Petit, &c., all figures relative to these laws being given in a tabular form; chemical combinations, *i.e.*, the formation of compounds by heat, light, and electricity, and under the influence of other bodies; the development of energy during chemical processes, this chapter containing nineteen very useful tables; changes of properties of bodies when entering into chemical combinations; the decomposition of bodies by heat, electricity, and light; mutual decomposition; and chemical isomerism. Under each of these heads we find a considerable amount of most valuable information, skilfully selected from the already immense literature of that subject, and always giving the last results of recent investigations. The work will be thus of a great value for the student, giving in one volume of 800 pages of compact print a reliable and often very complete *exposé* of the results reached by science in this most important department.

Elementary Theorems Relating to the Geometry of a Space of Three Dimensions, and of Uniform Positive Curvature in the Fourth Dimension. By Simon Newcomb. (From the *Journal für Mathematik*, Band lxxxiii., Heft 4, 1877.)

This is an interesting contribution to the subject treated of by Riemann, Helmholtz, and others, and in this country by Prof. Clifford. The question is considered from the standpoint of elementary geometry instead of by the analytic method which has been commonly employed by writers on non-Euclidian Geometry.

Quatre Modèles, représentant des Surfaces développables, avec des Renseignements sur la Construction des Modèles, et sur les Singularités qu'ils représentent. Par V. Malthe-Bruun et C. Crone; *avec Quelques Remarques sur les Surfaces développables et sur l'Utilité des Modèles.* Par M. le Dr. H. G. Zeuthen. (Copenhague, 1877.)

In the third edition of Salmon's "Geometry of Three Dimensions" there is (p. 289) a description of a simple way of making a model of a developable surface, attributed by Prof. Cayley to Mr. Blackburn. This suggested to Dr. Zeuthen the idea of drawing on the same model curves having contact of different orders with the edge of regression (*l'arête de rebroussement*) and of constructing new models of a very elementary nature, showing the principal singularities of developable surfaces.

Full accounts are given in a pamphlet (15 pp.) and direc-

tions for putting the models together, which consist of flat cardboard marked in accordance with the printed descriptions. The whole is contained in a neat quasi-envelope (nine inches by seven).

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Glass for Reflectors

YOUR last number (vol. xvii. p. 226) contains a very interesting paper by Mr. Norman Lockyer, in which that gentleman quotes the following passages from Mr. Grubb's paper :—

"For the 4-foot disc of glass for the Paris reflector, in place of that which has so recently resulted in failure, the St. Gobain Glass Company require twelve months' time to perfect (although, be it remembered, the quality of the glass is here of no consequence whatever); and I have been myself in correspondence with the principal glass manufacturers here and on the Continent, and not one of them is willing to undertake even a 6-foot glass disc; so that it would appear that, above that size, the silver-on-glass mirrors are out of the question." . . . . "The other great difficulty in the manufacture of reflectors is the annealing of the disc, and I believe it is this difficulty which limits to so narrow an extent the production of glass discs for silver-on-glass mirrors."

It may be interesting to your readers to know that an attempt is now being made to entirely overcome the apparently insurmountable difficulties so clearly pointed out by Mr. Grubb, and to obtain at any time without delay, and at a very small cost, discs of glass suitable for making silvered reflectors from 6 to 8, or even 10 feet in diameter.

It is almost impossible to over-rate the difficulty of producing massive discs of glass such as the one employed for the 47¼-inch reflector of the Paris Observatory, weighing, as it did, no less less than 1,546 lbs. in the rough, for, however carefully annealed such a mass of brittle, slow-conducting material may be, it will always be liable to unequal expansion, deflection, and fracture.

Fortunately, however, we have commercial plate glass to fall back upon; plates of 1 to 1½ inch in thickness can be readily made and perfectly annealed, and it is to the substitution of these large and comparatively thin sheets of glass, in lieu of thick cast masses, that my attention has been chiefly directed.

It is perfectly well known that plates of 1 to 1½ inches in thickness, if of large area, are subject to a great amount of deflection and consequent distortion of the image, which no ordinary support or backing can prevent. Several modes of converting such thin discs of commercial plate glass into efficient reflectors are about to be put to the test of practical experiment for the 50¼-inch silver-on-glass reflecting telescope which I am making and erecting at my residence on Denmark Hill.

Any attempt to support a disc of this diameter of 1½ inch in thickness against a cucheon of any kind, or loosely against a plane, must end in failure; nor can we hope to escape the difficulty by cementing the glass to any foreign substance whose power of conducting heat and rate of expansion differs from that of glass, as a giving way of the cement would be only a question of time, while distortion would result from unequal expansion of the two different materials. An intermediate course has therefore been adopted.

A strongly ribbed hollow cellular casting is made of iron 52½ inches in diameter, and 13 inches in thickness, weighing 1,400 lbs.; after slowly cooling in its mould, it will be again heated to about 900° F., and then be again slowly cooled; the whole of the external skin of the casting will be turned off in the lathe, and its face made into a true plane, less the final process of scraping; it will then be thrice annealed in oil, each time slowly raising the temperature from 60° up to 600° F., and each time slowly cooling it again. When all undue tension has thus been got rid of, its face will be finally scraped to a true plane, and a small spiral channel $\frac{1}{16}$ of an inch deep, and the same in width, will be formed on the flat face, the channels being about ¼ an inch apart from each other, and extending from the centre nearly to the outer edge of the metal surface. One side of the glass disc having been previously ground flat by the plate-glass manufacturer, will have a second grinding on the grooved plane, so as to insure perfect contact all over its surface; the emery having been all carefully removed, the surface of the iron plane is to be slightly moistened with olive oil, and the disc of glass replaced upon it. A flanged iron ring will then be placed around the glass disc, and screwed firmly to the iron surface, leaving a clear annular space of about ⅝ of an inch wide between the periphery of the glass disc and the ring; a permanently tenacious viscous matter (of the character of soft marine glue) will then be poured into this annular space, forming an air-tight junction between the iron plate and the glass surface, and at the same time admitting the glass to expand or contract freely. A partial vacuum will then be formed beneath the glass by exhausting the air through a central hole communicating with the spiral groove; the glass disc will then be held firmly in contact with the entire surface of the iron plane, which, however, is free to slide under the glass when undergoing expansion or contraction. I have found by repeated experiments (many years ago) that plate glass (say of ¼ of an inch only in thickness) so held on to an unyielding plane, may be repeatedly struck by the rounded face of a heavy wooden mallet, with the greatest violence, without producing a single fracture, so complete is the support thus afforded.

It is important to bear in mind that a glass disc so held does not rest on its lower edge when placed in a vertical position, nor are the upper portions of the plate allowed to press on, and be supported by the lower ones, as is inevitably the case with a mirror loosely suspended in a sling in the ordinary manner, but on the contrary, every portion of the glass disc is sustained and supported in position by atmospheric pressure, and held flatly and firmly against a corresponding portion of the unyielding iron plane, free from any accumulated downword pressure.

The expansion by heat of plate-glass and cast-iron are in round numbers as 19 is to 22, and the differential amount of this expansion between the extreme range of summer and winter temperatures, would cause the iron to exceed the diameter of the glass by about $\frac{1}{100}$ of an inch;—this minute sliding motion of the two smooth planes upon each other would not in the slightest degree alter the curved face of the mirror.

The glass disc having been thus finally and permanently attached to the iron plane, the latter would be supported in its cell by bands passing round it as usual, and with a system of triangular supports at the back. The weight of this strong-ribbed hollow cellular plane, of 13 inches in thickness, is only 1,400 lbs., while a disc of equal diameter in speculum metal, if only 4¼ inches in thickness, would weigh about 2,700 lbs.; hence such a compound metal and glass reflector is lighter than a solid cast glass one, and but little more than one-half the weight of a reflector made of ordinary speculum metal, while its thickness being three times as great as the latter, it would, when in use, and also while undergoing the polishing operations, be perfectly free from deflection.

Hitherto I have spoken only of the mode of mounting the glass disc on its iron support; it now remains to convert the flat surface of the glass disc into a shallow concave reflector. For this purpose I have made experiments in turning glass concaves with a diamond-cutting tool mounted on a slide-rest, and I have found that in this way glass affords nearly the same facilities for shaping in the lathe that iron or brass would do under similar conditions; it therefore follows that lenses of all shapes and sizes may be brought approximately to a true figure with very great ease and rapidity.

Satisfied with this result, I am now erecting in my laboratory a lathe of peculiar construction and specially adapted to this purpose with a bed fifty feet in length, and having a fifty-four inch diameter face plate at each end of the mandril. A massive radius-bar or frame of double the intended focal length of the reflector, moves on an adjustible pivot attached to the lathe-bed, while the other end of the radius frame carries a slide-rest in which a diamond-cutting tool is mounted, and by means of which a spherical concavity is rapidly and truly turned over the whole face of the glass disc, and of any desired radius, while a second plate of glass or metal is turned into a convex surface on the other face-plate of the lathe, thus furnishing a convex grinder of the exact same radius as the concave reflector. Special arrangements are made to neutralise any difference in the length of the radius-frame by expansion or contraction during the turning operation, and provision is also made for gauging to the

$\frac{1}{100}$th of an inch the focal length of the convex and concave surfaces under operation.

Although I have heretofore described the cellular casting as having a flat face, it will be obvious that if made into a concave corresponding with the intended focal length of the reflector that much thinner sheets of glass than those before named may be employed by first bending them to the required curve and fitting them by grinding to the concave iron surface, so that a glass reflector can on this principle be made just as large as a plate-glass manufacturer can produce an ordinary thin plate.

A description of the novel arrangements which I employ for grinding and polishing the spherical concave reflector, and its conversion into a paraboloid of revolution would carry me far beyond the already too lengthy remarks I have made, and which had for their primary object simply to show that we may still have good reason to hope that silver-on-glass reflectors of large diameters are within our reach. HENRY BESSEMER

Denmark Hill, January 21

A Telephone Without Magnetism

FOR some time past I have been experimenting with the view of transmitting articulate sounds through wires without the aid of electricity or magnetism.

I have now been quite successful, my experiments proving that the sounds of the human voice can be carried by *vibrations* through considerable lengths of wire.

Last night conversation was carried on with ease between *four* individuals, situated in different rooms. Piano music, singing, laughing, and breathing, were all clearly transmitted to the ear.

The whole distance would be about fifty yards.

The communication was effected by means of a mouth-piece with a vibrating disc in connection with the wire.

Glasgow W. J. MILLAR

Change of Habits in Toads

WHILE prosecuting my field-work as Palæontologist of the United States Geological Survey of the Territories, under the direction of Prof. F. V. Hayden, in Colorado, during last season, I had the opportunity to make some very interesting observations in relation to a change of habits in the common toad (*Bufo americana*). The district referred to is that portion of the great plains which lies immediately adjacent to the eastern base of the Rocky Mountains, and which is traversed by the South Platte River and its tributaries there.

The valleys of these streams are broad and shallow, and the streams heading in the immediately adjacent mountains have an abundant flow of water; so that large tracts of land in all those valleys have been brought under cultivation by irrigation. Irrigation is necessary in all that region, for it lies within that portion of the United States domain upon which the annual rainfall is insufficient for the purposes of agriculture.

With the irrigation of the land came increased and perennial vegetation; with that came increased insect-life, and with that an increase of birds and toads. The irrigating ditches are everywhere numerous, and during the season of growing crops they are frequently visited by men to regulate the flow of water to the land.

This and other circumstances disturb the toads that frequent the shades of the herbage which grows upon the borders of the water. It is no uncommon thing for toads as well as frogs, to jump into the water when disturbed, but the habit of the former is to make a shallow dive, rise immediately to the surface, and swim upon it by a sweeping curve to the shore again, not resting until the brink is gained, upon which they tarry a while before coming upon the land.

Frogs, on the contrary, when disturbed, make a strong dive directly to the bottom, upon which they lie prone, with the legs flexed against the body, and into the mud of which they settle themselves a little. Here they remain and exhaust the patience of one who may attempt to wait for them to rise. Now the toads in this irrigated region have adopted precisely these common habits of the frogs when disturbed upon the borders of the ditches, as I repeatedly witnessed. I regard this as the resumption of an instinctive trait that has been potentially transmitted from a former race of Anourans that were less differentiated than frogs and toads are now from each other; and that the lately introduced change of physical conditions in the region has caused the toads to resume habits which the frogs have never abandoned

Washington, D.C., January 6 C. A. WHITE

Talking Photographs

THE article from the *Scientific American* on the phonograph which is quoted in NATURE, vol. xvii. p. 190, concludes as follows:—"It is already possible, by ingenious optical contrivances, to throw stereoscopic photographs of people on screens in full view of an audience. Add the talking phonograph to counterfeit their voices and it would be difficult to carry the illusion of real presence much further."

Ingenious as this suggested combination is, I believe I am in a position to cap it. By combining the phonograph with the kinesigraph I will undertake not only to produce a talking picture of Mr. Gladstone which, with motionless lips and unchanged expression shall positively recite his latest anti-Turkish speech in his own voice and tone. Not only this, but the life-size photograph itself shall move and gesticulate precisely as he did when making the speech, the words and gestures corresponding as in real life. Surely this is an advance upon the conception of the *Scientific American!*

The mode in which I effect this is described in the accompanying provisional specification, which may be briefly summed up thus: Instantaneous photographs of bodies or groups of bodies in motion are taken at equal short intervals—say quarter or half seconds—the exposure of the plate occupying not more than an eighth of a second. After fixing, the prints from these plates are taken one below another on a long strip or ribbon of paper. The strip is wound from one cylinder to another so as to cause the several photographs to pass before the eye successively at the same intervals of time as those at which they were taken.

Each picture as it passes the eye is instantaneously lighted up by an electric spark. Thus the picture is made to appear stationary while the people or things in it appear to move as in nature. I need not enter more into detail beyond saying that if the intervals between the presentation of the successive pictures are found to be too short the gaps can be filled up by duplicates or triplicates of each succeeding print. This will not perceptibly alter the general effect.

I think it will be admitted that by this means a drama acted by daylight or magnesium light may be recorded and reacted on the screen or sheet of a magic lantern, and with the assistance of the phonograph the dialogues may be repeated in the very voices of the actors.

When this is actually accomplished the photography of colours will alone be wanting to render the representation absolutely complete, and for this we shall not, I trust, have long to wait.

WORDSWORTH DONISTHORPE

Prince's Park, Liverpool, January 12

Sun-spots and Terrestrial Magnetism

I BEG to direct Prof. Piazzi Smyth's attention to an article in the *Annuaire du Bureau des Longitudes* for 1878 by M. Faye, entitled "La Météorologie Cosmique," in which this distinguished astronomer and meteorologist says:—"La période des taches, portée à 11^{ans}·1 par M. Wolf n'étant pas égale à celle des variations magnétiques (10^{ans}·45), ces deux phénomènes n'ont aucun rapport entre eux." It thus appears rather premature to suppose that the sun-spot cycle and the terrestrial magnetic diurnal oscillation cycle are intimately connected. A. W. DOWNING

Greenwich, January 21

Great Waterfalls

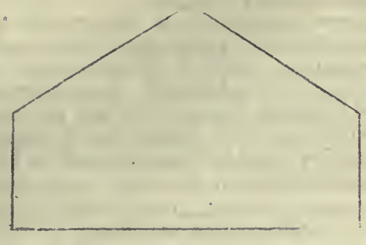
IN reply to Mr. Guillemard's inquiry in NATURE (vol. xvii. p. 221) he will find some account of the Kávari or Cauvery Falls in the "Mysore Gazetteer," recently compiled under orders of the Indian Government, vol. ii. pp. 271–273 (Bangalore, 1876). A copy is doubtless to be seen at the India Office Library.

Edinburgh, January 21 W. W. HUNTER

Mechanical Analysis of the Trevelyan Rocker

ALMOST every physical cabinet possesses one of Trevelyan's rockers, and yet it is rare to find one which always works well and gives complete satisfaction. Some two years ago having experienced this difficulty in New York, where I was then Professor of Physics, I requested Mr. Robert Spice, F.C.S., of 230, Bridge Street, Brooklyn, U.S., a very skilful constructor of acoustic instruments, and a thorough physicist, to make for me several of these rockers and ascertain, if possible, the conditions

of success. After many experiments with rockers of different sizes and angles, Mr. Spice obtained a formula by which a perfectly satisfactory rocker can be constructed, as several trials since then, both in America and Europe, have convinced me. Be-



lieving that there are many other professors who feel interested in this matter I communicate to the readers of NATURE, at Mr. Spice's request, his analysis of the rocker.

Let A B C D be the principal section of the rocker. Draw an indefinite base-line through the points C and D. From the point B let fall the perpendicular B E, and from F the perpendicular F D.

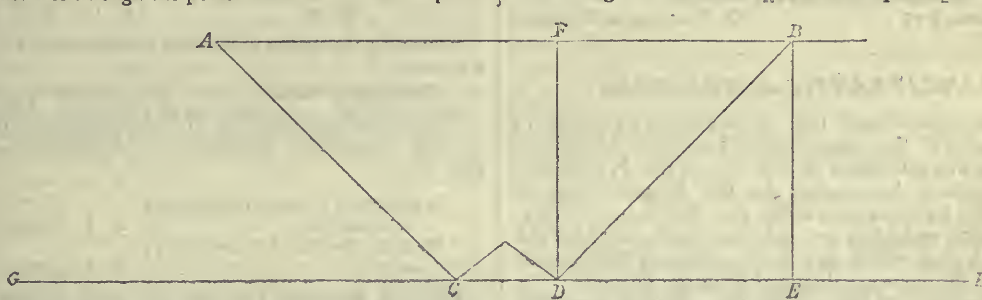
When the lead support raises (by expansion) the point D the point C becomes the fulcrum, and the line D E represents the complimentary arm of an imaginary lever C D E of the third order. In proportion as the distance C D is very small in comparison with the distance D E, in a like proportion will greater force be required to raise the rocker, and *vice versa*.

By experiment on a right-angled prismatic rocker (*i.e.* if the lines A C and B D be produced the angle at their intersection would be a right-angle) it was found that the most certain and pleasing effect was obtained when the distance C D was to the distance D E as 2 : 5.

In the case of a right-angled rocker as above, of course the distance D E = the distance D F.

By making the rocker-angle less than a right-angle, the distance D F would exceed the distance D E. This, it is believed, would be an advantage, as the leverage would remain constant and the additional weight would have the effect of raising the note.

The length of the rocker should be equal to twice A B. The



length of the handle should be four times A B. Finally, in practice, the angles C and D are slightly flattened, by filing, to prevent adhesion to the lead by sinkage, also to gain a larger heating surface.

The lead should have the form shown in the section below, and should weigh from three to four pounds.

SAMUEL H. FRISBEE

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No Butterflies in Iceland

A FEW months ago, at a meeting of the Linnean Society, Mr. McLachlan, when speaking of the various species of butterflies brought to England from the far north by the last English Arctic expedition, mentioned incidentally that there were no butterflies in Iceland.

On looking up some old books on the subject, in which I had the most able assistance of Mr. Erickr Magnussen, of Cambridge, we found at folio 602 of a book entitled, Olaffson (Eggert) Reise giennem, Island. Sorö, 1772.

LEPIDOPTERA.
L. phalænæ.
,, *maxima.*
,, *fluctuata.*
,, *geometra.*
,, *tota aurea.*

Again, in a work by R. Mohr, 1786, folios 90–91, under the head "Lepidoptera," we have—

L. phalænæ.
,, *graminis.*
,, *betularia.*
,, *olivacea.*
,, *lucerina.*
,, *vaccinii.*
,, *fluctuata.*
,, *pratella*, &c., &c.,

all of which are named as butterflies of Iceland.

Mr. McLachlan is a very high authority, and not at all likely to assert as a fact that there are *now* "no butterflies in Iceland," unless it were true.

The only possible way in which these perfectly opposite authorities can be reconciled (unless we throw aside those of a hundred years ago as worthless), is to suppose that in the interval the butterflies and their larvæ have been destroyed—not an impossible circumstance in Iceland, which has been almost, if not wholly, covered with poisonous volcanic ashes from time to time.

JOHN RAE

Kensington, January 18

The Great Pyramid

I HAVE been reading in Mr. Piazzi Smyth's book on this subject ("Our Inheritance," &c.). From the measurements made or cited by the author it appears tolerably clear that if the vertical height of the pyramid, as originally built, be taken as 1·, the total length of the four base lines will be twice 3·14159, &c., the number which expresses the circumference of a circle whose diameter is 1. At first sight this statement seems startling, but I think it may readily be acceded to, and that neither Mr. P. Smyth nor anyone need believe that by inspiration or otherwise, the architect knew the above relation of diameter to circumference, or was a circle-squarer in any special sense. I conceive the architect to have done something like the following :—Deciding first upon the vertical height of his intended pyramid, he took a cord, equal in length to that vertical height, and with it as a radius described a circle on level ground. Along the circumference of this circle he laid another cord, the ends of which met and were fastened together. The circle being thus formed, he drove four pegs, at equal distances inside the cord, so as to stretch it out into a square. The square thus formed gave the lines for the base of the pyramid; and it is obvious that thus the ratio of diameter to circumference would necessarily be built into the pyramid, however ignorant the architect might be. Working drawings (actual size) of surfaces, angles, chambers, passages, and other things would easily be laid out on the ground. The dimensions of the so-called King's chamber, and of a coffer or stone chest therein, which appear to involve the above ratio of 1 to 3·14159, &c., were, I think, arrived at by a somewhat similar process of construction.

Now as to the religious aspect of the case and an easy bit of "development." A cone is a well-known ancient religious symbol (of the kind denounced by Mr. P. Smyth as unclean),

and no doubt cones had been erected before the time of Cheops, and had their meaning and uses. Probably they were first made of earth, and the circular base would no doubt be set out by a cord, as above described. Cheops, by his architect, squared the circular base, getting thereby lines much better adapted to stone work, whilst still keeping the old sacred emblem, though in a developed form. It may, I believe, be traced down to many modern forms not often suspected of bearing any relation to it.

Belper, January 17 J. G. JACKSON

Acoustical Effects of Atmospheric Pressure

ON tapping an ordinary bell-jar receiver after exhaustion, the following was noticed. The note derived from percussion after exhaustion was sensibly of a higher grade than that obtained from the glass containing air. On gradually letting the air in, the note sank directly as the amount of air so introduced. We conclude that the phenomena here recorded are connected with the atmospheric pressure, and that the note yielded is a function of the atmospheric pressure. Can any of your readers suggest a method for the investigation of the observed facts, if no investigations have been before made on the subject.

Rugby, January 19 G. RAYLEIGH VICARS

TRANSATLANTIC LONGITUDES[1]

IT will be remembered that a preliminary account of the results of the transatlantic longitude determination of 1872 was published in Vol. xxii. of the *Proceedings* of the American Association for the Advancement of Science. We have now received the final report of Mr. Hilgard, which embodies not only the results, but also the observations, and which sets forth concisely the manner in which the former were deduced from the latter.

The importance of fixing with the greatest precision achievable, the longitude of some point in the coast survey triangulation with reference to Greenwich, led the U.S. Government promptly to make use of the means afforded by the completion of the Atlantic telegraph cable from Ireland to Newfoundland.

The first telegraphic longitude determination through it, made under the direction of Dr. B. A. Gould, in 1866, although it surpassed in exactness all results obtained by different methods was subject to a small but indeterminate correction, the "personal equation" between the American and the standard Greenwich observer.

Use was therefore made of the French cable in 1870 to make another determination under different circumstances, and under the charge of Mr. Dean the longitude difference between Brest and Cambridge, U.S., as before, was obtained; but as at that time no cable was in operation between Brest and England, the connecting link, Brest-Greenwich, remained undetermined until 1872, when Mr. Hilgard took charge of the work necessary to supply this deficiency, and since the opportunity was afforded, to repeat the Transatlantic determination. This time an intermediate station, St. Pierre, on the American side, was introduced, thus varying still more the conditions under which this third determination was made.

The general plan of operations was to unite at Brest time signals from St. Pierre, Greenwich, and Paris. The co-operation at the last-named stations of the Astronomer-Royal, Sir G. B. Airy, and of M. Delaunay, and the generosity of the telegraph companies, enabled Mr. Hilgard to finish the work successfully in September of that year.

We can only advert briefly to one or two points of interest. The accordance of the results appears to have been due in a great measure to the attention given to the accurate determination of the relative personal errors of the observers, which gave also indirectly the "personal equation" correction, lacking in the longitude determination of 1866.

[1] Final Report on the Determination of 1872, with a Review of Previous Determinations. By J. E. Hilgard. From the United States Coast Survey Report for 1874.

Incidentally, the "wave-time" of the cable signals was deduced, and on the assumption of equality in time in either direction, the resulting wave-time from Brest to St. Pierre, through a length of cable equal to 2,979 statute miles, is given as 0s.·351 ± s.·003.

The final results are given as follows, and the author remarks "that the close agreement of the three independent determinations made in different years is no less surprising than it is satisfactory." Even if we assume, as Mr. Hilgard evidently does, the identity of the results as accidental within the limits of the probable errors assigned, the determination must be characterised as being of the highest order of precision.

Longitude of Cambridge (*Harvard College Observatory dome*) *west of Greenwich* (*meridian*) :—

	h.	m.	s.	s.
1866	4	44	30·99	± 0·10
1870			30·98	± 0·06
1872			30·98	± 0·04
Mean	4	44	30·98	± 0·04

Referring this mean value to Paris (meridian of France) we have :—

Cambridge—Paris ... 4h. 53m. 51s·95 ± 0s.·06

These results, combined with elaborate determinations of the longitude difference, Washington-Cambridge, give :—

	h.	m.	s.	s.
Washington (Naval Observatory) —Greenwich	5	8	12·09	± 0·05
Washington (Naval Observatory) —Paris	5	17	33·06	± 0·07

We may, therefore, consider the geographical position of the Washington Observatory as one of the best determined in reference to others.

One of the incidental but highly important results of this expedition is the longitude difference Greenwich-Paris, the accuracy of which was checked by the conditions involved in the closing of the longitude triangle Greenwich-Paris-Brest. The result, 9m. 20s.·97 must now supersede the value obtained by Mr. Leverrier in 1854, which it exceeds by nearly half a second.

ANTOINE CÉSAR BECQUEREL

IT is with regret that we record the death of the noted French physicist, Prof. Becquerel, which occurred on January 18, in Paris. Antoine César Becquerel was born at Châtillon-sur-Loing, in the Loiret department, March 8, 1788. After completing a course in the Paris Polytechnic, he entered, in 1808, the Imperial Engineer Corps. It was no time of idleness for young officers, and he was shortly in active service, taking part in the entire Spanish campaign under General Luchet. Here he was present at the sieges of Torbosa, Tarragona, Lagonte, and Valencia, and manifested such marked abilities that in 1812 he returned to Paris to receive the rank of captain, and be presented with the Cross of Chevalier of the Legion of Honour, from Napoleon's own hands. In the following year he was sent by the Emperor to complete the fortifications on the German frontier. At the fall of the empire, in 1815, he resigned his position as chief of battalion in the Engineer Corps, and devoted himself exclusively to physical and chemical research, accepting a position as teacher in the Musée d'Histoire Naturelle, of Paris. In 1837 he was made professor in this institution and occupied this position up to the time of his death. Shortly after entering upon his scientific career he commenced the remarkable series of investigations in electricity and magnetism which have been uninterruptedly continued during the past half-century, and have linked his name closely with every branch of these two leading departments of physics. In thermo-electricity Becquerel carried out a large number of experiments on the

currents caused by heating both a single metal and two metals in contact, and formulated the well-known thermo-electric series, bismuth, platinum, lead, tin, gold, silver, copper, zinc, iron, and antimony. In his studies on atmospheric electricity he proved that the water of the ocean and the solid crust of the earth are in opposite electrical conditions, a fact which explains the positive state of the air immediately above the sea, while at a distance from the ocean the positive change is noticeable only at a certain height above the earth. The physiological effects of the electric current formed likewise the subject of numerous observations, and by means of delicate apparatus he was able to demonstrate the development of minute currents by the various operations of life, the movement of the muscles, &c. In view of the purely chemical character of these operations these observations harmonised perfectly with the theory which he advanced that electric currents were produced by all chemical unions and decompositions.

The effects of electricity on the colours of flowers, he showed to consist chiefly in a mechanical bursting of the cells containing colouring matter, and not in a chemical change. The conductive powers of a number of elements and compounds for the electric current, as well as the thermal phenomena in bad conductors, formed likewise the subject of numerous investigations. In magnetism Becquerel's researches were confined chiefly to the demonstration of the ability of all bodies to be magnetised, and to the phenomena of terrestrial magnetism. His favourite field of discovery, and that in which he obtained the most brilliant results, was electro-chemical action; in the variety and value of his contributions in this department he is certainly surpassed by no other physicist, while he was the first to grasp and sum together the scattered observations, and fairly mould them into a science. In 1834 he observed the deposition of metal on the negative electrode when the two poles of a pile were introduced into solutions of the salts of various metals. Shortly after he discovered that by using feeble currents the metal could be deposited very evenly and equally on the surface of the electrode, and that the two solutions required for the purpose could be kept from mingling by the use of goldbeater's skin or animal membranes, without hindering the current. These facts were at once made use of by De la Rive, of Geneva, who based on them his technical process of gilding in 1840. Although not the first to make the practical application of his discoveries, Becquerel rapidly improved the methods derived from them, and contributed in swift succession an enormous number of facts which serve as the fundamental principles of the art of galvano-plastic. These are to be found in a compact state in Smee's Elements of Electro-metallurgy. Becquerel's famous *Oxygen-circuit*, discovered at this time, made his name known at once to a large circle, on account of its simple, practical quantities. It consists of a glass tube covered at one end with linen, which supports a layer of kaolin, and designed for the solution of the metallic salt to be reduced. This is placed in a vessel containing a dilute acid, and the object to be electro-plated is immersed in the solution after being connected by a wire with a platinum plate in the acid. The action begins instantaneously, and is both rapid and regular. Another well-known apparatus is his *depolariser*, an arrangement designed to obviate the reverse currents produced by the gaseous deposits on platinum electrodes, and consisting essentially in a continuous shifting of each of the plates to the liquid of the other, so that they have no opportunity to become polarised. The oxygen-circuit, with its gentle regular current, was used by Becquerel for the decomposition of a large variety of chemical compounds. Among the more noteworthy preparations by its action can be mentioned aluminium, silicium, beryllium, sulphur, and the various earthy and metallic phosphates. Equally extensive were the preparations of crystalline salts, notably those occurring in nature, by the action of the electric current on mixed solutions or on solutions of soluble salts in contact with insoluble substances. During the past ten years his attention has been almost exclusively devoted to the novel and remarkable electro-capillary phenomena first observed by him in 1867. These can be observed in their simplest form when a cracked test-tube containing a solution of cupric sulphate, for example, is immersed in a solution of sodic sulphide. A deposition of metallic copper takes place at once on the crack. This elementary fact has been elaborated in a variety of directions with numerous solutions, and the laws regulating the development of electric currents by capillary action partially enunciated. The study of these phenomena is, however, still in its infancy. Becquerel regarded them as explanatory of the deposition of metals in veins in the rocks and of many physiological reactions taking place in the vegetable and animal tissues. A very detailed account of the experiments is to be found in vol. xxxvi. of the *Mémoires de l'Institut.*

Despite his manifold experimental investigations, Becquerel was an indefatigable author, and contributed a most valuable series of standard works to the physical literature of the past forty years. In the seven volumes of his "Traité expérimental de l'Électricité et du Magnétisme, et de leurs Phénomènes naturels," 1834-40, he presented these two sciences with a completeness and systematic arrangement which has been hitherto wanting in physical literature. This work was followed by "Éléments d'Électro-Chimie appliquée aux Sciences naturelles et aux Arts," 1843; "Traité de Physique considérée dans ses Rapports avec la Chimie," 1844, 2 vols.; "Éléments de Physique terrestre et de Météorologie," 1847; "Traité de l'Électricité et du Magnétisme; leurs Applications aux Sciences physiques, aux Arts, et à l'Industrie," 1856, 3 vols.; Résumé de l'Histoire de l'Électricité et du Magnétisme," 1858; and "Des Forces physico-chimiques et de leur Interprétation dans la Production des Phénomènes naturels," 1875.

In 1829 Becquerel was elected a member of the French Academy, and received in 1874 the Medaille Cinquantenaire, although he had been but forty-five years a member. His scientific communications are to be found in the *Comptes Rendus* of the Academy and in the *Annales de Chimie et Physique.* The Royal Society elected him as a corresponding member a number of years ago, and he was one of the three French *savants* who have been recipients of the Copley Medal. In 1865 Napoleon III. decorated him with the Cross of Commander of the Legion of Honour.

Prof. Becquerel leaves behind him a son, Edmond Becquerel, Professor of Physics in the Conservatoire des Arts et Métiers, who has assisted his father for a long series of years in the compilation of his numerous works, and whose researches in electricity fairly rival those of the latter. The funeral ceremonies took place on Monday in the church of St. Medard, at Paris.

DAVYUM [1]

ABOUT the middle of this year (1877) I succeeded in isolating a new metal belonging to the platinum group. I named it Davyum, in honour of Sir Humphry Davy, the eminent English chemist.

The platiniferous sand from which it has been extracted [2]

[1] From an article by Sergius Kern in *La Nature.*

[2] The sand treated bad the following composition:—

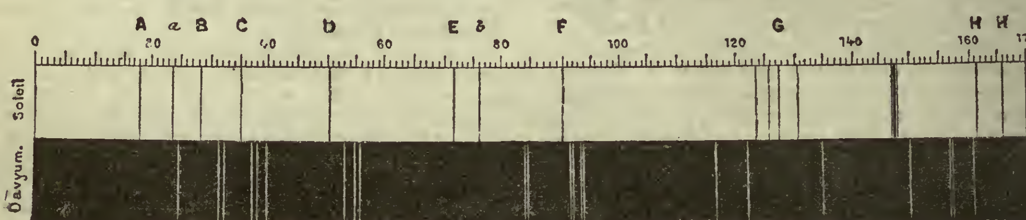
Platinum	80·03
Iridium	9·15
Rhodium	0·61
Osmium	1·35
Palladium	1·20
Iron	6·45
Ruthenium	0·28
Copper	1·02
	100·09

was treated, for the separation of the metal, by the analytical method of Prof. Bunsen. The mother-liquor obtained after the separation of the rhodium and iridium was heated with an excess of chloride of ammonium and nitrate of ammonium. A dark red precipitate was obtained after calcination at red heat. It yielded a greyish mass resembling spongy platinum. The ingot resulting from 600 grammes of mineral weighed 0·27 kil.

The metal was dissolved in aqua regia, in order to examine the action of different reagents on the solution. Potash gave a clear yellow precipitate of the hydrate of davyum, which is easily attacked by acids, even by acetic acid. The hydrate of davyum dissolved in nitric acid gave a brownish mass of nitrate of davyum; by calcining this salt a black product is obtained, which is probably the monoxide.

The chloride of davyum, dissolved in a solution of potassic cyanide, gave, by gently evaporating the solution, beautiful crystals of a double cyanide of davyum and potassium. In this salt the potassium may be replaced by several metallic elements. The cyanodavic acid is very unstable; it is isolated by passing a current of sulphuretted hydrogen through a solution of the double cyanide of lead and davyum. Sulphuretted hydrogen produces, in the acid solutions of davyum, a precipitate of sulphide of davyum, which is easily attacked by the alkaline sulphides, yielding probably a series of sulpho salts.

A concentrated solution of chloride of davyum yields, with potassic sulpho-cyanide, a red precipitate, and when gently cooled, produces large red crystals. If the same precipitate is calcined the sulpho-cyanide of davyum takes the form of a black powder. These reactions show that this salt is allotropic.



Spectrum of davyum according to the data of Sergius Kern.

The chloride of davyum is very soluble in water, alcohol, or ether; the crystals of this salt are not deliquescent. The calcined salt gives the monoxide as a residue. Chloride of davyum forms double salts with the chlorides of potassium and ammonium. They are insoluble in water and very soluble in absolute alcohol. The double salt of sodium and davyum is almost insoluble in water and alcohol; this reaction is very characteristic, because many sodic salts of the platinum group are very soluble in water.

This chloride of davyum is the only one which exists, as the second product, containing more chlorine, is decomposed during the evaporation of the solution, disengaging chlorine.

I have made some new researches on the density of melted davyum; three experiments gave the following numbers :—9,383, 9,387, 9,392 at 24°. These results agree very sensibly with those of my first researches; the density of davyum given in my first note to the Academy being 9,385 at 25°.

M. Alexejeff has undertaken the determination of the equivalent of davyum; but as the quantity of davyum which I possess is very small, exact researches are difficult. Preliminary experiments have shown that the equivalent is greater than 100, and probably about 150-154.

Some new platiniferous sands, which are to be placed at our disposal, will yield a sufficient quantity of the new metal for additional experiments. We hope to have in time nearly 1·2 gr. of davyum.

Finally I have investigated the spectrum of davyum by vaporising the metal in powder between the carbons of the electric lamp. The spectroscope at my disposal is not powerful enough to show precisely all the secondary lines. This is why I have only indicated the principal lines easily visible in my spectroscope.[1]

THE GREAT DETONATING METEOR OF NOVEMBER 23, 1877

HAVING fully discussed the whole of the accounts of the great meteor that have reached me, consisting of some ninety direct communications and forty or fifty newspaper cuttings, I have the pleasure to forward to NATURE a condensed description of it.

The points of most importance to be determined are—1. The true orbit which is obtained from a knowledge of the radiant and velocity of motion. 2. The height at which it first became luminous, as our knowledge of the real extent of the earth's atmosphere depends exclusively upon such determinations. 3. The height at which it exploded and came to an end. That this last is connected with the physical condition and constitution of the body cannot be doubted. The brightness of meteors seems always to depend upon the distance they penetrate into the air. Generally, when they get below 30 or 40 miles, they are very remarkable.

The Greenwich mean time was 8h. 24m. 30s. on November 23.

There are but few descriptions of the path of the great meteor in question from which to derive the radiant point. Five of the fully-described tracks meet almost exactly in R.A. 62°, N.P.D. 69°. The others tend to support this position rather than to alter it, but many are, as is usual, extremely wild, passing 20°, and even 30° from it. To an observer situated near the middle of the north coast of Wales, this radiant would bear south 74° E., at altitude 37°.

The meteor first came visible to Mr. T. B. Barkas, at Newcastle-on-Tyne, to another observer at Tynemouth, to the Rev. G. Iliff, at Sunderland, and Mr. E. Pikard, at York, at the great height of 96 statute miles. The observers agree very closely. It is probable, of course, that had any one been actually looking in the right direction, it might have been seen a little earlier when it was still higher. A height exceeding 90 miles is certain. The meteor was then vertically over a point 13 miles north of Derby, and its appearance was that of an ordinary shooting star. Descending in the air at the inclination of 39° to the surface of the earth, when 48 miles exactly over Liverpool, it became intensely brilliant, so suddenly, that many observers speak of this as the first explosion.

It was at this instant that it attracted universal attention. People as far distant as Essex, Roscommon, Edinburgh, Bristol, and Queenstown, 200 miles from it,

[1] *Comptes Rendus* and *Chemical News*.

describe it as being nearly as large as the full moon and greatly exceeding it in brilliancy. An observer at Ashby-de-la-Zouch first noticed his shadow, and those of neighbouring trees thrown *towards* the moon, then shining brilliantly in the east. Persons much nearer the scene, sitting in rooms with the blinds down, were frightened by the flood of light that suddenly found its way in. The meteor exploded with great violence at the height of 14 miles over the Irish Sea, 20 miles N.N.W. (true) of Llandudno. The total length of path was 135 miles, which was traversed in about 8 seconds of time, or with a velocity of $17\frac{1}{2}$ miles per second, as determined from twenty-three estimations of its duration.

The streak left in the air extended for 40 miles along the track, and was not less than 2,000 feet in diameter.

The violence of the explosion was such, that at Bangor, Beaumaris, Conway, and Llandudno, doors and windows rattled, and people ran out to see what was the matter. As far as Chester the sound resembled "thunder not very far distant," or "a salvo of artillery."

It is a fact worthy of thoughtful consideration that the body which was capable of producing this convulsion, probably exceeding the discharge of the 81-ton gun in the proportion of a hundred to one, was converted into impalpable powder in eight seconds of time, merely by the rapidity of its transmission through very attenuated air. After the explosion nothing remained but dully incandescent dust or ashes, which slowly fell a short distance *vertically* downwards. That is to say, there was not one remnant sufficiently heavy to continue in the same direction, or to retain the original velocity, because such a remnant would have been visible itself as a bright meteor. A momentum which, estimated in foot-tons, would reach some enormous figure, was instantaneously reduced to nothing, or, rather, converted into atmospheric waves—and dust!

The exact position and height of the explosion is fixed by the singular observation of Mr. Petty, at Llandudno (NATURE, vol. xvii. p. 183), who did not even see the meteor itself, but its light on the hearthrug coming through a chink in the blind.

Mr. J. Ismay, the superintendent of telegraphs at Liverpool, who observed the explosion from the beach at Llandudno, measured the sound-interval, and found it between 2 min. and 2 min. 15 secs. From the spot where I have assumed the explosion took place to his position is 25 miles, which sound would traverse in two minutes exactly.

The orbit deduced from the apparent position of the radiant point is—

$$i = 0$$
$$\pi = 153^\circ$$
$$q = \cdot 47$$

Motion direct.

The relative velocity obtained by assuming a parabolic orbit is 19 miles per second, agreeing very closely with that found by observation.

If the longitude of the radiant be diminished 3° or 4°, the orbit is so far modified as to almost coincide with that of the comet of 1702. The comet was not very well observed. The meteor belonged to the well-known shower of *Taurids*, first discovered by Mr. R. P. Greg, encountered by the earth with great regularity about November 21–23. In 1877 it appears to have been very prolific of bright and of detonating meteors.

G. L. TUPMAN

OUR ASTRONOMICAL COLUMN

THE COMETS OF 1618.—The year 1618 presented a phenomenon which is perhaps unique in the history of the appearances of comets, two of these bodies having been conspicuously visible at the same time in certain parts of he earth, and for several days, at least, in the same quarter of the heavens, with trains of thirty or forty degrees in length, and upwards. Cometographers previous to Pingré had been much exercised with reference to the comets of this year; Comiers, in "La Nature et Prestige des Comètes," had supposed that six comets in all were observed in 1618; this number was reduced by Pingré to three, which appears to have been beyond doubt the correct number, though another cometographer, Struyck, disputed the distinctness of Pingré's second.

The first comet was discovered at Caschau, in Hungary, on August 25, and two days later by Kepler, at Lintz, where it rose in the morning about three o'clock, with a tail directed towards the west. Kepler observed it on several occasions, and for the last time on the morning of September 25, and from his rough indications of its positions Pingré calculated the elements which figure in our catalogues, and which it will be found represent the track of the comet pretty nearly; there can be no confusion between this object and the second or third comets of the year.

The third comet, as Pingré remarks, "eut autant d'observateurs qu'il y avoit alors d'astronomes en Europe." It was first seen in Europe in the last days of November, and was observed by Cysat at Ingoldstat till January 21, he having used optical aid, though other observers lost it at the beginning of the month, or even earlier. The elements, first calculated by Halley, were more accurately investigated by Bessel, whose orbit, published in 1805, agrees with the observations as closely as the errors, with which they are obviously affected, will allow. There is no difficulty, therefore, in fixing upon the position of the third comet, with sufficient approximation in the month of November previous to its discovery in Europe.

It appears to have been one of the finest comets of the seventeenth century, apparently hardly exceeded in the imposing character of its appearance by the celebrated comet of 1680. The tail gradually increased in length until, on the morning of December 10, the Danish astronomer, Longomontanus, estimated its extent at 104°, with marked coruscations.

The second comet of 1618, according to Pingré, to which these remarks are intended more particularly to refer, was discovered in Silesia, and also at Rome, on the morning of November 11, as Kepler tells us (*De Cometis lib. I.*). The nucleus was lost in the twilight, but the tail was visible from 4h. 20m. to 6h. 40m. at Rome. On the following mornings the tail was seen at other places in Europe, and by Kepler himself at Lintz, at 5h. 30m. A.M. on November 20; he then describes it as a train of milky-white light, passing below the stars in the quadrilateral of Corvus, and reaching the extremity of Crater. He saw this train for the last time on the morning of November 29, when "inter atras nubes et ventos vehementes, cum campi essent picti nivula, apparuit tamen tractus iste secundi cometæ, sed valde dilutus nec æquans albedinem nubium a luna illuminatarum." This was at 5 A.M., and an hour and a half later, the clouds having somewhat dispersed, he obtained his first view of the third comet, which was then in longitude 221°, with between 9° and 10° north latitude. Thus we see that Kepler saw both comets on the same morning, though he failed to detect the nucleus of the second in the strong twilight; and it may be added that Blancanus, at Parma, had similar experience.

In more southern latitudes the second comet was pretty favourably situated for observation, and its nucleus was observed. Figueroes, ambassador of Spain, at Ispahan, and the Jesuits at Goa, saw both comets simultaneously, and determined positions of the nucleus of the second. Riccioli mentions that Father Kirwitzer, an Austrian, was sent out to China, and died at Macao in 1626, adding that he wrote of observations made in India on the comets of 1618.

In a communication to Baron de Zach early in 1821, Olbers states that Brandes had sent him a work by this Father Kirwitzer, which it appeared had become very scarce, containing observations of the second comet of 1618, but so disfigured by faults either in copying or printing, that he had found it impossible to deduce from them a tolerable orbit. According to these observations "la comète sautille d'un jour à l'autre ça et là dans le ciel, tantôt en avant, tantôt en arrière, de sorte qu'à peine peut-on reconnaître quelle a été la vraie direction de son mouvement." Kirwitzer, who had observed the comet from November 14, reports that on November 26 he was joined in the observations by Father Schall, and Olbers drew attention to the fact that in Zach's *Monatliche Correspondenz*, vol. xxviii., it had been stated that fourteen volumes of Schall's manuscripts were in existence in the library of the Vatican, and engaged Zach to use his interest towards having them examined. This was soon after effected by Conti, but unfortunately no allusion to the second comet of 1618 was found in them, indeed these manuscripts proved so worthless, that Zach considered them "que de la poudre chinoise jetée aux yeux européens." It does not appear that a more accurate copy of the Goa observations has been found since Olbers wrote on the subject. There are two works by Kirwitzer in the British Museum, but they afford no assistance. It thus happens that there is as yet no orbit of the comet in question.

In a further note we shall briefly recapitulate other circumstances in the history of the comet, and examine one or two points in which the known elements of the third comet assist in establishing the absolute distinctness of the second, notwithstanding the idea advocated by Kepler that a comet had divided into two—and which led Pingré to say of him—*aliquando bonus dormitat Homerus.*

METEOROLOGICAL NOTES

Contribution to the Climatology of the Spanish Peninsula.—An interesting and able contribution to the climatology of the Spanish Peninsula has been made by Dr. Hellmann in a discussion of the humidity and clouds of that region, published in the Dutch *Meteorological Year-Book* for 1876, being one of the results of the author's recent prolonged meteorological tour in the Peninsula. One of the broad results arrived at is this: the small variation in the annual humidity of places on the west coasts of Europe, as contrasted with the large variation in the humidity of the east coasts of Asia, together with the striking climatic contrasts resulting therefrom is essentially, though less intensely, reproduced in the climates of the Peninsula of the west bordering the Atlantic as contrasted with those of the east washed by the Mediterranean. As regards the relative humidity of the air, the climate is moister in May than it is immediately before and after, and it is interesting to observe that thunderstorms, rain, and cumulus, cirro-cumulus, and cumulo-stratus clouds show an increase in May as compared with March and April on the one hand, and June and July on the other. The annual variation in the relative humidity increases from about four to nine per cent. on the coasts, to about forty per cent. at such inland places as Madrid and Campo Maio. Those who are familiar with the weather-maps of Europe are aware how often atmospheric pressure is so distributed as to give rise to winds blowing outwards from the Peninsula to the ocean in all directions, being easterly on the west coast, southerly on the north, westerly on the east, and northerly on the south. They are everywhere dry winds, and are known in the various provinces as the *Terral*, or land-wind. The desert-wind of the Spanish Mediterranean coast is the *Leveche*, and not the *Solano*, as it is almost universally stated to be by non-Spanish writers. The Solano is, as its name implies, a simple east wind which blows everywhere over the east coasts, and is a rain-bringing wind, but in no sense a desert-wind, malignant and prostrating in its effects. The true desert-wind is known by the name of the Leveche, which is usually loaded with fine sand and dust, and is hot and stifling, is productive of violent headaches, and prostrates even the most robust with a feeling as if every member of the body were oppressed under a load of lead. Dr. Hellmann describes the effects of the passage of one over a vineyard in August, 1876, the appearance being as if a scorching flame had passed over it. The Leveche is felt on the coast only from Cabo de Nao, to Cabo de Gata in the south, and in a less severe form as far as Malaga; but it extends inland no farther than from forty to fifty miles.

Climatology of the Fiji Islands.—A valuable contribution to this subject from data collected by the Meteorological Office has appeared in the *Quarterly Journal* of the Meteorological Society for July, 1877. From the position of the Fiji Islands in the South Pacific, the climate is strictly tropical, the year being divided into a hot moist season, extending from November to April, and a cool dry season from May to October. The prevailing winds are S.E. and E., but during the hot season, particularly from January to March, N.E. winds prevail. These N.E. winds are, in Mr. Strachan's opinion, probably due to the heated land of the large island, Viti Levu, giving rise to a wind of aspiration. The annual rainfall on an average of six years was 110 inches. The heaviest falls occur in the summer months of January, February, and March, when thunderstorms are frequent, and in the same months hurricanes occur, though frequently several years pass in succession without the occurrence of any hurricane. In the cool season the rainfall, though considerable, is reduced in amount and frequency, and in all seasons there is a considerable difference as regards moisture and rainfall between the windward and lee sides of the different islands, the effect being strikingly shown by the difference of vegetation. The working out of this question of the distribution of the rainfall by such a multiplication of gauges over the islands as has been so successfully done in the Mauritius and Barbadoes, is most desirable from the scientific and practical importance of the subject. The mean annual temperature is about 77°·5, and the difference between the hottest and the coldest months scarcely reaches 5°. In the wet season atmospheric pressure is about 29·870 inches, and vapour tension 0·860 inch, but in the dry season 30·020 inches, and 0·700 inch, thus showing considerable variation through the year in the pressure and vapour tension of a climate characterised by comparatively so little variation as that of Fiji.

Extension of Volunteer Weather Service in the United States.—We are greatly gratified to see that the marked success which has attended the volunteer weather service in the State of Iowa, so vigorously prosecuted by Dr. Hinrichs, and which now numbers about 100 observers, is leading other states to adopt a similar system. Prof. Francis E. Nipher, of the Washington University of St. Louis, has already secured the services of fifty-five observers, chiefly in the northern and western parts of Missouri, for the regular observations, particularly of rainfall, but also, where possible, of temperature pressure and humidity; and for observations of irregularly recurring phenomena, such as storms, the aim being to collect together as full and accurate an account of the different phases of these phenomena as it is possible to make, particularly their commencement, culmination, and termination. The investigation of the climatology of the state is also to be undertaken. The observations are to be according to local time. Regular reports will be furnished to the newspaper press. The work is under-

taken under the auspices of the university, and it is not intended that it supersede, as regards this State, the work of the central office at Washington (D.C.), but to supplement that work in collecting data for a more satisfactory treatment of the climatology and storms of that state. We strongly commend this scheme, and earnestly hope that Prof. Nipher will succeed in extending his network of stations till all parts of the state be adequately represented, especially since telegraphic stations everywhere are by far too few to meet the requirements of the more important and pressing problems of meteorology. We have the further satisfaction in learning that a similar weather service is contemplated in the State of Kentucky.

HIGH TEMPERATURE OF NOVEMBER LAST.—M. Brounoff, of the St. Petersburg Physical Observatory, publishes in the Russian *Golos*, December 10, an interesting note as to the unusually high temperature of St. Petersburg during November last. The mean temperature of that month was as high as 39°·4, or 10°·3 higher than the mean temperature deduced from ninety years' observations, and 4°·9 above the very high mean temperature of November observed at St. Petersburg in 1851. Throughout the month the thermometer never fell below 32°·0. It is worthy of notice that during all the other months of this year the temperature was lower than the means deduced from ninety years' observations. An unusually high temperature prevailed in November over nearly the whole of Europe and Western Siberia, except North Scotland, Southern Italy, the middle Danube, and the two shores of the Caspian. The highest above the average, 15°·7, was observed at Archangel, and the line of 9°·0 runs from the Upper Volga to Stockholm, and thence straight north. The proximate cause of such unusually high temperature was the abnormal predominance of barometrical minima with south-westerly winds, which passed over Europe during November last. Thus, the number of these minima in November has been forty-two during the last five years, whereas there occurred thirteen during November last, the one thus following the other almost without interruption.

TEMPERATURE OF VIENNA.—Among other points treated in a recent paper by Dr. Hann to the Vienna Academy, "On the Temperature of Vienna, according to a Hundred Years' Observations," is the influence of the frequency of sun-spots on the mean temperature of summer, winter, and the year. Neither in the temperatures arranged according to the separate cycles of sun-spot frequency, nor in the averages of these from all the nine cycles (1775 to 1876) is there recognisable a distinct periodicity of the heat variations, which can be connected with the period of sun-spot frequency. Placing in the individual cycles the averages of every three years' temperatures, corresponding to the minimum and maximum of the spots, opposite each other, it is found that in five cycles out of nine the minimum years have indeed a considerably greater heat than the corresponding maximum years. But in three cycles precisely the opposite is the case, and in one cycle the difference is almost *nil*. Dr. Hann further inquires whether one may with any probability draw inferences from the temperature character of one season with regard to that of the next, and the next again. He finds that if the temperature-anomaly of one season reach a considerable amount (a divergence of 1° C. or upwards), the probability that the following season will diverge in the same sense from the average value is 0·68; the probability that a very cold or warm winter will be followed by a cold or hot summer respectively, is even 0·70. On the other hand, the probability of an agreement of the temperature-anomaly of a winter with that of the previous summer is only 0·45. In his paper Dr. Hann also gives a comparison of the temperatures of the meteorological and astronomical observatories.

GEOGRAPHICAL NOTES

MR. STANLEY.—Mr. Stanley arrived in London on Tuesday. From the time that he emerged at Emboma from his ever-memorable dash into the unknown region west of Nyangwé to his arrival at Folkestone, his journey homewards has been a well-earned ovation. Everyone, from the Governor downwards, at the Cape vied in doing him honour; at Cairo the Khedive conferred upon him two of the highest orders of merit; at Rome he received the Victor Emmanuel Gold Medal of Merit, arriving too late, alas! to receive it from the hands of its donor, though it was accompanied by a sealed letter from the late King, speaking in high terms of Mr. Stanley's discoveries and his services to humanity and civilisation; Turin, Milan, and Naples sent welcomes to him; at Marseilles the Geographical Society, the Chamber of Commerce, and the Municipality presented him each with a medal; at Paris the Geographical Society fêted him in splendid style, the President of the Republic sending his representative the Minister of Public Instruction presenting him with the high honour of the palms of Officier de l'Instruction Publique, and the President of the Geographical Society telling him he should be gold medallist of the Society for 1878. We expressed confidence last week that our own Geographical Society would lead the movement in this country for giving Mr. Stanley a reception worthy of the great work he has achieved, and we rejoice to see that our confidence has been justified. The Society are to invite Mr. Stanley to dinner, and also to read a paper on his discoveries, "at St. James's Hall or elsewhere." We feel sure that St. James's Hall will be quite inadequate for the accommodation of all who will wish to see and listen to the story of one of the greatest of pioneer-explorers; so that, after all, the announcement made in the *Times* last week, that the Albert Hall was to be taken for the purpose, is likely enough to be correct. There will certainly be no difficulty in filling it. Everyone will wait with impatience the publication of Mr. Stanley's work; for although a fair idea of what he has done has been obtained from his occasional letters in the *Telegraph*, there must be many things to tell that could not be set down in the circumstances under which these letters were written.

THE MARQUIS ANTINORI.—From another telegram received by the Geographical Society at Rome, it appears that the Marquis Antinori, contrary to his first intention, does not return to Italy, but has started again with his companions on a new tour southward from Shoa. Signor Martini alone comes home with the scientific collections.

AFRICAN EXPLORATION.—Reports from Berlin state that in the budget for the current year the sum of 100,000 marks (5,000*l.*) is asked for the continuation of the exploration of Central Africa. This is considerably more than in the preceding years; the rise in the sum demanded is justified by reference to the efforts of German private societies and scientific men.

THE NORTHERN PAMIR.—The last number of the *Izvestia* of the Russian Geographical Society contains some new and valuable information on the little-known tracts of the Northern Pamir, which have hitherto been a blank on our best maps. This information has been compiled from notes taken last summer by M. Korostovtseff during his journey to the Alai Valley and the Northern Pamir highlands. The valley of Alai, visited first by M. Fedchenko, runs north-east and south-west for forty-five miles, and is from thirteen to twenty miles wide. It is inclosed between high mountains, the Kaupmann Peak reaching 25,000 feet. Forests are found only in the north-eastern part of the valley (11,000 feet above the sea) which is part of the dominions of the Khan of Kashgar, while the south-western part (8,000 feet high),

watered by the Kyzyl-su River, is covered with luxurious Alpine pasturage, and therefore becomes in summer the feeding ground for immense herds of cattle belonging to the Fergana, Kashgar, Shungan, and Karateghin Kirghizes. A sandy cleft, Tash-kurgan, leads from the Alai Valley to the Pamir Highlands. After a journey of forty-five miles along this cleft, and after having crossed the Kyzyl-art Pass, 14,017 feet high, M. Korostovtseff reached the salt-lake, Kara-kul, twenty-seven miles long and twelve miles wide, 13,194 feet above the sea-level. Its sandy banks are quite bare, and the surrounding stony hills bear no traces of vegetation; it is only close by the deep-blue waters of the lake that the traveller discovers here and there a low and dry bush. Thence M. Korostovtseff turned south-east, entered the cleft Alabaital, and reached, by a gentle slope, the pass of the same name, 15,314 feet high, whence he had to descend on the very steep southern slope, to the valley of the Chan-su River, quite bare and covered with snow-white deposits of salt. The valley of a rivulet, Uz-bel, tributary of Chansu—a sandy desert twenty miles long—and the Uz-bel Pass, 15,195 feet high, were followed east to reach the valley of Sary-kol, 14,300 feet above the sea-level, and covered with a very scarce vegetation; here some small rivulets give rise to the Kashgar-daria River. Thus the general characters of the northern part of the Pamir table-land are high valleys, flat, open, bare, and sandy, never descending below some 13,000 feet, with blue salt lakes and salt deposits on their dry bottom; relatively low mountains, the passes between which are only some 1,000 or 2,000 feet above the bottom of the valleys, the peaks being covered with perpetual snow when they exceed an altitude of 15,000 or 16,000 feet; no inhabitants, and a very scarce vegetation. Such is the hitherto mysterious "roof of the world" (Pamir). From Sary-kol M. Korostovtseff was compelled to return. He died a short time after his return, without being able to publish the results of his most interesting journey or describe the valuable collections he obtained.

THE "NERTHUS" OF TACITUS.—Dr. Michelsen, of Schleswig, has just published a pamphlet in which he discusses that remarkable and often-mentioned Nerthus-island, which, according to the description of Tacitus, with its sacred lake and forest, formed the centre of a divine service of seven closely connected communities. Formerly the island of Rügen, or the so-called "Land Oldenburg," was thought to be the island in question. Dr. Michelsen, however, points out that the island of Alsen is the one meant by Tacitus. He states that the name signifies "sanctuary" or "temple-island," and that the sacred lake and forest still exist in the north-west of Norburg on the Alsensund, under the names of "Hellewith and Hellesö" (*heilige Wald und heilige See*—holy wood and holy sea). The inhabitants of that district still call the village of Hellewith, situated near the forest, Hellod (*heiliges Eigen*—holy own); and in the existing remains of the old forest there is a well-preserved sacrificial altar consisting of enormous blocks of granite. Dr. Michelsen gives a number of other interesting proofs for the correctness of his conjecture, and also remarks that he has partly discovered the names of the seven Nerthus people in villages of the Sundewitt district.

VENEZUELA.—In the January session of the Berlin Geographical Society, Dr. Sachs gave a description of his recent journey to Venezuela, for the purpose of studying the gymnotus in its native haunts. Humboldt's sketch of the Llanos was completed and corrected in some points. This great plain, formerly an inland sea, is 600 feet above the sea in its upper part, and but 200 in its lower part, a difference which accounts for the fact that the grass, but 1 to 2 feet in height in the upper portion, rises above the head of the river in the lower region. The decrease in the number of cattle on the Llanos of late years has led to a rapid extension of the arboreal growth. The Llaneros are a peculiar people, arising from a mixture of the white, red, and black races, and standing on a low grade of civilisation, their religion consisting in the adoration of a few saints, and marriages being rare. Humboldt's familiar description of the capture of the electric eel, by driving horses into the streams frequented by it, as the customary method in the land, is regarded as resting on an error. No one in the region was acquainted with it, and it was found impracticable to carry out. The scientific results of Dr. Sach's observations will be published shortly.

THE INDUS.—The course of the Indus river from the point where it leaves Cashmere down to where it enters English territory, about 120 miles below Darband, has recently been explored in detail by a Punjaub surveyor, and our geographical knowledge of the river has thus been considerably augmented, while valuable topographical material has been obtained. Of course Capt. Carter had previously determined, in a general way, the course of the river in the districts named, by his trigonometrical measurements of the heights of the mountain summits on both banks of the Indus.

NEW GUINEA.—Dr. E. T. Hamy, in the just issued November part of the *Bulletin* of the Paris Geographical Society, describes in considerable detail the results of his examination of an old map of New Guinea, for the purpose of showing how much had been done for its discovery by the Spanish navigators of the sixteenth and seventeenth centuries (1528–1606). The map, which serves as the basis of Dr. Hamy's paper, is contained in the atlas of Pierre Martier, published at Amsterdam in 1700. The data for this and other maps in the atlas had been collected by Frémont d'Ablancourt while in Portugal, and the many names on New Guinea would show that by the sixteenth century its coasts had been pretty well explored all round, though its shape is very inaccurately laid down.

NOTES

WE give some account to-day of the life and work of the late M. Becquerel, and next week we hope to do the same for M. Regnault, who died two days after M. Becquerel, in his 68th year. M. Victor Regnault was born at Aix-la-Chapelle, in 1810. He was Professor of Physics in the College of France, and of Chemistry in the Polytechnic School; he also held for some time the Directorship of the Porcelain Manufactory of Sèvres. His researches in the several branches of physics and chemistry published in the *Memoirs* of the French Academy of Sciences, and many other scientific journals, are numerous, and of the greatest value. Of these perhaps his publications on the expansion of elastic fluids, the determination of the densities of gases, the measurement of temperatures, and the determinations of the specific heats of liquids, solids, and gases, are the most important, and have brought his name most prominently before the world. He has also written many valuable papers on physiological questions. M. Regnault was elected a member of the Academy of Sciences in 1840, and in 1850 was created an officer of the Legion of Honour.

THE Council of the Royal Society of Edinburgh have awarded the Neill medal to Dr. Ramsay Traquair, for his paper on the Structure and Affinities of *Tristichopteris alatus*, Egerton, being one of an important series of contributions to the knowledge of the structure of recent and fossil fishes.

WE are informed that the Pennsylvania Railway Company are disposed to grant very favourable terms to any European astronomers who, in their private capacity, may wish to go to America to observe the approaching eclipse of the sun. It is stated that for less than half the usual fares astronomers will be conveyed from New York, Washington, or Baltimore to Denver. We

hope, however, to be able in an early number to publish definite information on the matter.

THE German Military Department, always on the watch to make use of the latest scientific discoveries, has naturally devoted its attention at once to the telephone. In the last number of the *Militair Wochenblatt* we notice a report on the practicability of its use in warfare for maintaining communication with pickets and outlying posts. The experiments were carried out at a temperature of $-3°$ C., and during a violent wind, and showed most conclusively its availability for the purposes in question.

IT is gratifying to know that at last Cleopatra's Needle has safely reached the Thames. It is proposed to moor the ingeniously-constructed vessel containing the obelisk at a convenient part of the Thames embankment for some days, to enable the public to inspect it.

THE lately formed society for the protection of the interests of chemical manufactures in Germany, begins with the present year the publication, at Berlin, of a monthly organ entitled *Die chemische Industrie*, under the editorship of Dr. Emil Jacobsen. It is intended to make it a complete record of everything of interest in the department of technical chemistry.

THE Academy of Sciences will hold its anniversary meeting next Monday, when M. Bertrand will deliver an *éloge* of Lame, a member of the Academy of Sciences and a physicist, who died twenty years ago. He had travelled in Russia like Becquerel, but not as an officer belonging to an invading force. He had been appointed by the Russian Government to establish the Military School of Odessa.

THE second part of Signor Mantegazza's studies on the Ethnology of New Guinea is published in the December number of his *Archivio*, illustrated by a number of plates.

A GEOGRAPHICAL Society has been formed at Metz, based on the model of those in other German cities.

THE German Patent Office reports that it has received during the past year 6,424 applications, a larger number than any other country can boast of except the United States.

THE Association for the Improvement of Geometrical Teaching held its annual meeting at University College, Gower Street, on Friday, January 11, and at this meeting, in addition to proceeding with the work already taken in hand, it was resolved that sub-committees should be appointed to draw up syllabuses of solid geometry and of higher plane geometry, and also that the Association should take into consideration the subject of geometrical conics, with a view to expressing its opinion on the best order of teaching it. The president (Dr. Hirst, F.R.S.) delivered an address, and subsequently tendered his resignation of the presidentship on the ground of the pressing nature of his other duties; the Rev. E. F. MacCarthy, one of the secretaries, also was obliged, for a like reason, to resign his office. The vacancies were filled up by the election of Mr. R. B. Hayward, F.R.S., as president, and of Mr. R. Tucker as secretary (in conjunction with Mr. R. Levett, the principal originator of the movement). Mr. J. M. Wilson and Dr. Jones were re-elected vice-presidents. Mr. H. C. Watson, Clifton College, was elected Treasurer in the room of Mr. H. Weston Eve.

IT appears that beer is adulterated to a great extent with glycerin. An easy and exact method of its determination in this connection is wanting, and a prize of 3,000 marks has been offered by the *Verein für deutschen Gewerbfleiss* for the best solution of this problem.

THE *Deutsche ornithologische Gesellschaft* was lately requested by the Chancellor of the Empire to express its opinion on a proposed law for the protection of birds. A duly appointed commission under the presidency of Dr. Brehm, has recently presented a report on this subject, in which the contemplated law is regarded as unnecessary. There is at present, according to their information, no general diminution in the number of useful birds, and where a local disappearance has been observed, it is to be traced to the present development of the agriculture and forestry of the land, and is not due to the direct attempts of man.

THE German botanist, Regel, has discovered in the Himalayas a variety of wild onion, which he regards as the original source of our ordinary garden onion. It is called *Allium cepa sylvestre*.

BERLIN is becoming the centre of an extensive system of subterranean telegraphic lines radiating in various directions. Cables have been already laid, or are in process of being laid, on the routes Berlin-Cologne, Berlin-Frankfort, Berlin-Strassburg, Berlin-Breslau, Berlin-Königsberg, and Berlin-Hamburg and Kiel. As a glance at the map will show, the military element plays an important part in the selection of these routes. Most of the lines are buried alongside the substantial roadways which traverse the empire. The work of excavation is carried on rapidly by means of enormous portable engines which dig a trench one metre in depth and half a metre broad, lay in it the cables (generally two in number, containing each seven wires), and cover them by a continuous movement.

A DOG-FISH became entangled in the net of some French fishermen near Cape Agde lately, and after having dragged their boat about during the entire night at the rate of twelve miles an hour, was finally captured and brought to land. It measured over sixteen feet in length and weighed about 2,500 lbs. Its enormous stomach contained the head, feet, and several other portions of a mule, as well as two half-digested tunny-fish.

WE notice in the last number of the *Journal* of the Russian Chemical and Physical Societies (vol. ix. No. 9), two interesting chemical papers by M. Eltekoff, on the regularity of elimination of the elements of the haloid-hydric acids from chlorates of hydro-carbonates, and on the structure of different amylenes which are found in the amylene supplied by trade.

PROF. C. HERMANAUZ, of Vienna, died recently in Japan, while engaged on a voyage round the world, chiefly for th purpose of agricultural observation.

FEW national scientific associations have grown so rapidly e the French Association for the Advancement of Science. Although but in its seventh year, we notice from the recently issued report of the secretary that the number of the members is already nearly 2,400. In this short time the association has accumulated a capttal of 223,000 francs, and has granted 26,000 francs to various scientific objects. Each member pays annually 20 francs, and receives a handsome copy of the report. The last issued (for the Session of 1876) forms a bulky volume of 1,200 pages, illustrated by seventeen well executed plates. According to the statutes, Paris is excluded from the place of session, on much the same ground that London is never chosen by the British Association. The present year forms, however, an exception, on account of the Exhibition, and Paris will welcome the Association far the first time.

IN the eighth number of the *Journal* of the Russian Chemical Society is a paper by Prof. Meorshutkin on the influence of isomerism on the formation of ethers between acids and alcohols (NATURE, vol. xvii. p. 151) (also published separately in French); a note by M. Ziloff, on the influence of the medium on the electro-dynamical induction; a paper by M. Borgmann, on thermo-electricity; and a note by M. Kraevich, on his new portable barometer, which is intended to avoid the usual boiling of mercury in barometrical tubes, and was highly approved some

time ago by officers of the Russian general staff, who have had the opportunity of making use of it on travels.

M. CHIKOLEFF, who has made, at St. Petersburg, several experiments on electrical lights, by order of the Ministry of War, confirms, in the ninth number of the *Journal* of the Russian Chemical and Physical Societies, the results of the experiments of Tyndall. He observes also, that a galvano-plastic copper coating of the carbon proves to be very useful.

AT a recent lecture held at the Rudolphinum, at Vienna, before a large audience, Dr. E. Lewy proved that the human skin is completely impenetrable for the chemical contents of mineral waters, and that therefore the explanation of the effects of baths in these waters, at the numerous bathing-places, has to be sought exclusively in the domain of physics and not in that of chemistry. This important discovery annuls all common views regarding the bathing cures effected by the various mineral springs, and explains in the simplest manner that, from a chemical point of view, the action of the most different waters must be one and the same.

THE French Government has recently appointed a mixed commission of leading scientific men and engineers for the purpose of making a thorough examination into the best means of preventing the explosions of firedamp in coal-pits. Among its members are MM. Daubrée, Berthelot, Thénard, and Hébert, of the Academy of Sciences, Professors Bert, Burat, Haton de la Goupillière, Fouqué, and other well-known names. Although the French mines have suffered comparatively little in this direction, the terrible disasters in our English mines have taught the necessity of throwing about the miner's dangerous occupation the utmost safeguards at the command of modern science, and an active and thorough programme is being prepared by the Commission.

A REQUISITION has been sent to the French Ministry by the Société de Physique, asking that it should be incorporated, or "reconnue comme d'utilité publique." It is stated that a favourable reply may be expected from M. Bardoux.

AT a recent meeting of the French Physical Society, M. Duter presented magnets obtained by subjecting circular steel plates to the action of an electro-magnet terminated with a conical point applied to the centre of the disc. In these magnets the neutral line is a concentric circle of the disc, with radius $\frac{R}{\sqrt{2}}$. To study the free magnetism distributed over them, M. Duter used a small soft iron cylinder (a few centigrammes in weight), fixed in the centre to the rod of an areometer floating in water. The force of detachment of this was estimated by the weight of water which had to be let off from the cylindrical vessel containing the areometer before the contact was detached. The precise instant of contact and detachment was indicated by an electric signal. M. Duter thus demonstrated experimentally that the quantities of free austral and boreal magnetism were equal in the two portions (of contrary name) in the same plate. He sought to represent by an empiric formula, the results relative to forces of detachment for plates of different diameter. These forces depend simply on one specific coefficient variable with the nature of the steel and with the thickness.

THE influence on the animal organism of breathing pure oxygen gas of density corresponding to ordinary atmospheric pressure, has not hitherto been adequately determined. The Royal Society of Göttingen, therefore, offer a prize for new researches on the subject, made both on homoiothermal, and, as far as possible, on poikilothermal animals; in these researches, while certain externally visible phenomena in the animal will have to be considered, special attention is desired to be given to the nature of the blood and the exchange of material (excretion of carbonic acid, and nature of urine). The oxygen used should be carefully freed from all foreign matters apt to occur in manufacture; while a limited (and perhaps hardly avoidable) admixture of atmospheric nitrogen would not compromise the results. In the mathematical class, the Göttingen society desires (and offers a prize for) new researches on the nature of the unpolarised light-ray, "fitted to bring the conceptions of natural light of any origin, near (in definiteness) to those which theory connects with the various kinds of polarised light." (For further particulars see the Society's *Nachrichten*, No. 26, 1877.)

HITHERTO water has been regarded as possessing a greater specific heat than any other body, with the exception of hydrogen. In a recent session of the Vienna Academy M. E. Lecher communicated the results of experiments showing that in this respect water alone is surpassed by various mixtures of methylic alcohol and water, which will accordingly take the second position in regard to hydrogen.

THE Report of the Berlin Academy of Sciences for September and October, which has just appeared, contains, among other papers, "Comparison of the Tidal Heights in the East Sea from 1846–1875," by H. Hagen; "Anatomy of the Appendicularia," by Prof. Virchow and H. Langerhaus; "Atomic Weight of Molybdenum," by Prof. Rammelsberg; "Movement of the Electricity in Submarine and Subterranean Telegraphic Wires," by Prof. Kirchhoff; and "Catalogue of the Fishes and Amphibia from Chinchoxo (Africa), presented to the Berlin Zoological Museum by the *Afrikanische Gesellschaft*," by Prof. Peters.

THE electromotive force produced by the flow of water through capillary tubes has lately been investigated both by M. Haga at Strassburg University, and by Mr. J. W. Clark at Heidelberg (*Pogg. Ann.*, No. 11, 1877). Both observers used a quadrant electrometer instead of a galvanometer (as in former experiments with diaphragms and capillary tubes) to measure the difference of potential. This difference, according to M. Haga, is proportional to the pressure, independent of the length of the tubes, dependent on the nature of the inner surface of the tubes, increases with the resistance of the water, and probably also with the temperature. Mr. Clark finds (1) that the narrower the tube the greater is the electromotive force when liquids are forced through. (2) In very narrow tubes the electromotive force is independent of the length; in wider tubes it decreases with the length. (3) If the inner tube-surface be coated with different substances, different electromotive forces are obtained, whose amounts entirely agree with Quincke's former results with regard to diaphragm currents. (4) The electromotive force decreases with the time; and this whether still water or flowing water occupy the tube between the experiments. If the tube be cleaned anew with sulphuric acid and distilled water, the original electromotive force is re-established. (5) The seat of the electromotive force is the limiting surface of the liquid and the solid tube-wall.

THE Russian newspaper published in Turkestan reports that the scientific explorations in the Semirechensk District were continued uninterruptedly during the year 1877. Special attention was bestowed upon the investigation of the line of coral reefs which remained from the prehistoric Central Asian Sea. This line extends from the Dalashik Mountains over the Tuluk Tau and Temirlik Tau, and further eastward as far as the frontier of the Kuldsha District. Large quantities of the finest corals and beds of fresh-water shells were found; marine shells were discovered only in small quantities. The silurian formation of these districts may now be considered as proved beyond doubt.

THE new ethnological museum opened at the Hôtel des Invalides, Paris, contains a collection of warriors belonging to several nations and tribes, civilised and uncivilised. These models

have been executed in an artistic manner and give a clear idea of the variety of destructive agencies resorted to by mankind for warlike purposes.

WE have received a useful little manual of dates, "Drury's Chronology at a Glance" (Hardwicke and Bogue), containing much well-packed information. In the next edition the author should omit all expression of opinion on events and men, and utilise the space for additional information.

IN the January number of Petermann's *Mittheilungen*, Dr. Mohn describes in detail the results as to soundings and temperatures of the Norwegian North Sea Expedition of 1876. Dr. Oscar Drude has an important article on the geographical distribution of palms, and a detailed programme is given of the new expedition of Gerhard Rohlfs, to which we referred last week. A brief sketch is given of the ten-years' exploration in South America of Doctors Reiss and Stübel, some of the results of which have appeared at various times in *Globus* and elsewhere, but the full details of which will necessarily take some time to publish.

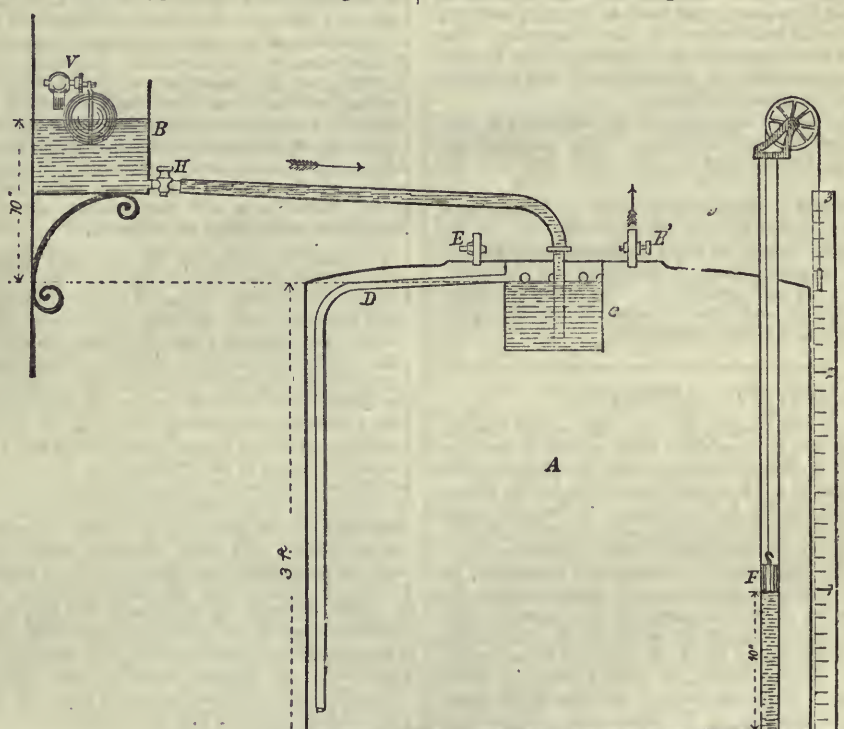
DR. RADDE, in a letter from Tiflis to Dr. Petermann, speaks of the brothers Brotheus, from Helsingfors and Wasa, who spen last summer in botanising in the Caucasus, taking back with them a varied collection of mosses and a rich herbarium of phanerogams.

THE additions to the Zoological Society's Gardens during the past week include a Javan Chevrotain (*Tragulus javanicus*), a Stanleyan Chevrotain (*Tragulus stanleyanus*), from Java, presented by Mrs. Leslie Walker; a Grivet Monkey (*Cercopithecus griseo-viridis*) from North-East Africa, presented by Madame Patey; an Arabian Gazelle (*Gazella arabica*) from Arabia, presented by Mr. Mark Whyley; three Summer Ducks (*Aix sponsa*) from North America, presented by Lord Braybrooke; two Mandarin Ducks (*Aix galericulata*) from China, two White-bellied Storks (*Abdimia sphenorhyncha*) from West Africa, purchased; two Silky Cow Birds (*Molothrus bonariensis*) from South America, a Superb Tanager (*Calliste fastuosa*), two Violet Tanagers (*Euphonia violacea*) from Brazil, deposited.

NEW FORM OF GAS-HOLDER

HAVING found the necessity of a gas-holder which should yield a steady flow of gas and be under control from the lecture-room, at some distance from the only available spot where the gas-holder could be placed, I devised the following plan, which was carried out for me by Mr. Yeates, to whom I am indebted for one or two excellent suggestions. The arrangement is, I believe, novel, it is inexpensive, and it answers admirably; it may, therefore, be of convenience to put before some of your readers the following sketch, which needs but little explanation:—

A is a bell-shaped, gas-tight holder of galvanised iron or stout zinc. B is a water-supply cistern with adjustible ball-cock valve, in fact, an ordinary kitchen boiler supply-cistern, in connection with the water-main through V. C is a small reservoir fixed to



the dome of the gas-holder; when filled, once for all, the water overflows into the holder; to avoid splashing it is better to convey a pipe, D, near to the bottom of the holder. The water-pipe from the cistern, B, passes air-tight into the gas-holder, and is furnished with a cock, H, to shut off the pressure when necessary. The pressure on the gas within the holder obviously depends on the difference of the water-level in the cisterns B and C. To give a brilliant lime-light some ten inches head of water is required. This corresponds to about 2 cwt. on the usual wedge-shaped gas-bag. To give a sensitive flame with a steatite gas-jet having an orifice the size of No. 19 wire, B W G (0·04 inch diameter), a pressure of some nine inches of water is required. The depth of the cistern, B, allows the ball-float a range of adjustment, and hence of gas-pressure, of some six inches.

E E′ are cocks for the entrance or exit of gas, and F is a float marking the quantity of gas in the holder.

When the gas has all been expelled from the holder it is full of water, and hence conveniently ready for refilling with gas. For this purpose the cock H is closed, and G partially turned on; the water escapes as the gas enters E. A delivery tube is carried from E′ to the lecture table, and can, of course, be used as an entrance as well as an exit pipe. After the holder is filled with gas, G is shut off and H and E′ are turned on. All is now ready for use, for as soon as the cock at the burner attached to the lantern or other arrangement in the lecture-room, is turned on, the gas is displaced from the holder by the entrance of a corresponding quantity of water from the cistern B. No weights are required to be taken on and off, an equable flow of gas is secured, the turning on of the gas-cock in the lecture-room puts the whole apparatus in action, and the employment of a single cylinder considerably diminishes the original cost of the gas-holder.

W. F. BARRETT

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UNIVERSITY AND EDUCATIONAL INTELLIGENCE

BERLIN.—The Prussian budget contains provisions for four new professorships in the university, including two in medicine, one in philology, and one in botany. The appropriations for most of the laboratories, &c., in connection with the university have likewise been notably increased. The "Gewerbe-Akademie" is to receive an important addition in the shape of a department for the examination of iron, in which the physical and mechanical properties of the various sorts of Prussian iron—unwrought as well as manufactured—can be thoroughly tested, and officially approved. A similar institute, and the only one hitherto in Germany, has existed for a number of years at Munich, and has been of great value to the iron industry in South Germany. The lectures of Prof. Du Bois-Reymond on physiology have become so popular that no lecture-room in the university is of sufficient size to accommodate his numerous hearers.

INNSBRUCK.—The winter attendance at the university is 605, an increase of 27 on the past semester. The philosophical faculty includes 159, the medical 55, the legal 206, and the theological 185.

JENA.—The university is attended at present by 488 students, a diminution of 102 on the number of the past summer. Of the 219 in the philosophical faculty 64 study philosophy and history, 73 mathematics and natural sciences, 62 chemistry and pharmacy, and 20 political economy and agriculture. The attendance from foreign countries is but 35, and Jena is one of the few European universities where England is not represented.

SCIENTIFIC SERIALS

Annalen der Physik und Chemie, No. 11, 1877.—The residual charge of the Leyden jar, in its relation to the nature of the insulating substance, by M. von Oettingen.—On the electromotive force produced by flow of water in capillary tubes, by M. Haga.—On the same subject, by Mr. J. W. Clark.—On the connection between electromagnetic rotation and unipolar induction, by M. Edlund.—On Dr. Kerr's recently-found relation between light and electricity, by Mr. Mackenzie.—Calorimetric researches, by MM. Schuller and Wartha.—Apparatus for demonstrating the different heat-conducting powers of gases, by M. Kundt.—Observations by Adolf Rosencranz, on the influence of temperature on internal friction of liquids, by M. O. E. Meyer.—On a method of investigating the gliding friction of solid bodies, by MM. Warburg and v. Babo.—Determination of the velocity of sound by the method of coincidences, by M. Szathmari.—Velocity of wave motion in soft string, by M. Abt.—Studies on chemical volumes, by M. Ostwald.—On miargyrite, by M. Weisbach Lime, strontium, and baryta in the crystalline state, by M. Brügelmann.—On numerical determination of the constants of Weber's fundamental law, by M. Voigt.—On a simple experiment for subjectively showing the reversal of the coloured lines of flame-spectra, especially the sodium line, by M. Günther.—On completeness of exclusion of aqueous vapour from air-pumps, by M. Laspeyres.—On the applicability of fatty gases in blowpipe operations, by M. Lohse.

Kosmos, July, 1877.—O. Caspari, on the philosophy of Darwinism.—Prof. Haeckel, in discussing Bathybius and the Monera, is very little inclined to give up the organic nature of Bathybius.—G. Jäger, on Heredity, part 2.—Carus Sterne (Dr. Krause) on the taming of the old by the young, discusses the influence of children from the Darwinian standpoint. He calls Bret Harte "probably the psychologist of deepest insight in our time."—Fräulein von Hellwald writes on the speechless primitive man.—Fritz Schultze on the origin of the culinary art.

August, 1877.—Mr. Darwin's biographical sketch of a little child is translated here.—Fritz Müller contributes observations on Brazilian butterflies on evolutional principles.—A. Dödel-Port writes on the colour and size of Alpine flowers—A. Lang, on Lamarck and Darwin, part 4, considers Lamarck's views on the relation of organic to inorganic nature.—Hugo Magnus treats on the development of the colour sense.

September, 1877.—Otto Caspari continues his discussion of the Darwinian philosophy, dealing with the problem of evil, the idea of individuation, the conditions of pleasure and disgust.—Prof. Jäger treats of colour and the colour-sense.—Prof. Krause discusses the origin of the legend of Iphis (Ovid, "Metam.," book ix.) with regard to its bearings on a morphological question.—Dr. A. Lang, in his fifth paper on Lamarck and Darwin, comes to Lamarck's theory of descent.—A comprehensive notice of Darwinian literature up to the present time is given by Dr. G. Seidlitz.

October, 1877.—Dr. B. Vetter, on design in nature.—H. Müller, on the variation in size of the coloured envelopes of flowers in relation to natural selection; a valuable paper.—Prof. Jäger, the origin of organs. Part III., locomotive organs.—Fritz Müller, on Brazilian butterflies, Part 2.—Dr. F. Weinland, on the language of primitive man.

Zeitschrift für wissenschaftliche Zoologie, vol. xxix., Part 4.—W. Schmankewitsch, on the influence of external conditions on the organisation of animals. This is a long and valuable paper, especially having to do with the influence of different degrees of concentration of salt water and varying temperatures on a number of Crustacea, as *Artemia saliva*, Daphnia, Branchipus.—J. W. Spengel, on the reproduction of *Rhinoderma darwinii* (amphibian), a translation from the Spanish of X. Jimenez de la Espoda; a very remarkable case of a male brood-cavity.—B. Hatschek, on the embryonic history of the budding of *Pedicellina echinata* (polyzoan), forty-eight pages, three plates.—A. Wierzejski, on the crustacea parasitic on cephalopods, twenty-one pages, three plates.—H. von Ihering, contribution to the morphology of the kidneys of mollusca.

THE current number of the *Quarterly Journal of Microscopic Science* commences with a paper by Mr. C. S. Tomes, on the hinged teeth of the common pike, the existence of which, in other than the angler and one or two other fish, was unknown.—The Rev. Thomas Hinks has notes on the movements of the vibracula in *Caberea boregi*, and on the supposed *common* nervous system in the Polyzoa, in which the synchronous movement of the vibracula is shown to render the existence of a common nervous system almost essential.—Dr. A. M. Marshall describes the development of the cranial nerves in the chick, in continuation of his earlier papers in the *Philosophical Transactions* and the *Journal of Anatomy and Physiology*.—Prof. E van Beneden contributes to the history of the embryonic development of the Teleosteans, showing that the germ-layers do not proceed exclusively from the blastodisc: that extra blastodic cells are developed on the deuteroblastic globe, and that there is no segmentation cavity.—Mr. S. H. Vines writes on the homologies of the suspensor of the ovule, showing its unity with the seta and foot of mosses, liverworts, and vascular cryptogams.—Prof. Lankester describes the corpusculated nature of the red vascular fluid of the earthworm.—Dr. F. Darwin describes the contractile filaments of *Amanita* (*Agaricus*) *muscaria* and *Dipsacus sylvestris*.—The last paper is a short one by Mr. Dowdeswell, on atmospheric bacteria.

SOCIETIES AND ACADEMIES

LONDON

Mathematical Society, January 10.—Lord Rayleigh, F.R.S., president, in the chair.—Mr. F. B. W. Phillips was elected a member, and Mr. R. R. Webb was admitted into the Society.—The following papers were read:—Mr. J. Hammond, on the meaning of the differential symbol D^n, when n is fractional. (Prof. Cayley gave a few references to papers on the subject by Riemann, Schrœter, and others, and expressed his

opinion that the matter had not yet been satisfactorily settled.) —Prof. Lloyd Tanner, on partial differential equations with several dependent variables.—Lord Rayleigh, on the relation between the functions of Laplace and Bessel (in § 783 of Thomson and Tait's "Natural Philosophy," a suggestion is made to examine the transition from formulæ dealing with Laplace's spherical functions to the corresponding formulæ proper to a plane). It is evident at once, from this point of view, that Bessel's functions are merely particular cases of Laplace's more general functions, but the fact seems to be very little known.—Mr. Ferrers, in his elementary treatise on Spherical Harmonics, makes no mention of Bessel's functions, and Mr. Todhunter, in his work on these functions, states expressly that Bessel's functions are not connected with the main subject of the book. The object of the present paper was to point out briefly the correspondence of some of the formulæ. The author showed that the Bessel's function of zero order (J_0) is the limiting form of Legendre's function, $P_n(\mu)$, when n is indefinitely great and μ (= cos θ) such that n sin θ is finite, equal (say) to Z. This was proved by taking Murphy's series for P_n (Todhunter, § 23). In like manner Bessel's functions of higher order are limits of those Laplace's functions to which Todhunter gives the name of associated functions. A theorem was found for the general functions corresponding to the relation subsisting between three consecutive Bessel's functions $\left[\text{viz., } \tfrac{1}{2} Z\left\{J_{m-1}(z)+J_{m+1}(z)\right\} = m\, J_m(z)\right]$. Prof. Cayley stated that the results obtained were very interesting).—Mr. S. Roberts gave some results bearing upon his paper read at the December meeting.—Prof. Cayley gave an expression for the surface of an ellipsoid communicated to him by Prof. Tait.—The Chairman, Professors Cayley, Tanner, and Mr. Webb spoke upon the subject.

Chemical Society, January 17.—Dr. Gilbert in the chair. —It was announced that a ballot for the election of Fellows would take place at the next meeting of the Society (February 7).—The following papers were read:—On the luminosity of benzol when burnt with non-luminous combustible gases, by E. Frankland and L. T. Thorne. After many unsuccessful attempts to burn benzol with a smokeless flame, the authors determined the luminosity of benzol vapour after dilution with hydrogen, carbonic oxide, and marsh-gas. These gases were passed through a benzoliser kept at a constant temperature and burnt in a fish-tail burner. The following results were obtained:—1 lb. avoirdupois of benzol gives, when burnt with hydrogen, the light yielded by 5·792 lbs. of spermaceti with carbonic oxide, that of 6·100 lbs. of spermaceti with marsh-gas, that of 7·7 lbs. of spermaceti. The authors point out that this difference is probably due in part to the different pyrometric thermal effects of the gaseous mixtures.—On the action of reducing agents on potassium permanganate, by F. Jones. Hydrogen reduces permanganate, sesquioxide of manganese being formed; ammonia produces in addition a nitrate, a nitrite, and free nitrogen; phosphine, arsine, and stibine give somewhat similar reactions; oxalic acid forms manganese sesquioxide, carbonic acid, and oxygen; strong solutions of permanganate and manganese chloride, when mixed, form sesquioxide of manganese, chlorine and oxygen being evolved.—On the action of sulphuric acid on copper, by Spencer Pickering. According to the author there are only two primary reactions, in one of which copper-sulphate, sulphurous acid, and water are the products, in the other subsulphide of copper, copper sulphate, and water are formed. The author has studied the action at various temperatures and has investigated the quantity of sulphuric acid actually used, the effect of an electric current, the action of impurities in the copper, the variations produced by diluting the acid, &c.—On the analysis of sugar, by G. Jones. The author proposes to estimate sucrose volumetrically by adding a 0·1 per cent. solution to a boiling decinormal solution of permanganate, acidulated with sulphuric acid, until the dirty-brown hydrated peroxide of manganese, which is at first formed, is reduced and dissolved.—On the decomposition products of quinine, by W. Ramsay and J. Dobbie. The authors oxidised quinine with permanganate and obtained a new acid, which they have identified with Dewar's dicarbopyridenic acid, and a red amorphous substance. The same acid was obtained by oxidising Marchand's quinetin.

Geological Society, December 19, 1877.—Prof. P. Martin Duncan, F.R.S., president, in the chair.—Messrs. William Fream, J. G. Hochstätter Godfrey, Herbert Goss, and John Fowke Lancelot Rolleston were elected Fellows of the Society. —The following communications were read:—On *Argillornis longipennis*, Owen, a large bird of flight, from the eocene clay of Sheppey, by Prof. Owen, C.B., F.R.S. In this paper the author described some remains of a large bird obtained by Mr. W. H. Shrubsole from the London clay of Sheppey (already referred to in NATURE), consisting of parts of fractured humeri belonging to the right and left sides of the same species or perhaps individual, and including the head of the bone, with portions of the upper and lower parts of the shaft. The texture of the shaft, the thinness of its bony wall, and the large size of the cavity recall the characters of the wing-bones of the large cretaceous pterodactyles. The author concluded that the bones obtained by Mr. Shrubsole furnished indications of a new genus and species of flying birds, for which he proposed the name of *Argillornis longipennis.* He regarded it as probably a long-winged natatorial bird, most nearly related to *Diomedea*, but considerably exceeding the Albatross (*D. exulans*) in size.—Contributions to the history of the deer of the European miocene and pliocene strata, by Prof. W. Boyd Dawkins, F.R.S. The author commenced by referring to the difficulties attending the study of the European miocene and pliocene deer, and indicated that the majority of the known antlers may be referred to two categories—an earlier or capreoline, and a later or axidine type. To the Capreoli he referred the following species:—*Dicroceros elegans*, Lart. (= *Prox furcatus*, Hemel), *Cervus dicranoceros*, Kaup (including *C. anoceros* and *trigonoceros*, Kaup), and *Cervus Matheronis*, Gerv. (= *C. bravardi*), from the miocene, and *Cervus australis*, Gerv., and *C. cusanus*, Croizet and Jobert, from the pliocene. To the Axeides belong *Cervus Perrieri*, Cr. and Job. (including *C. issiodorensis* and *pardinensis*, of the same authors), *C. etneriarum*, Cr. and Job. (= *C. rusoides*, Pom., and *C. perollensis* and *stylodus*, Brav.), *C. suttonensis*, sp. n., and *C. cylindroceros*, Brav. (including *C. gracilis*, Brav.), all from pliocene deposits. Besides these, the author noticed a species *insertæ sedis* under the name of *Cervus tetraceros*, Dawkins, which he regards as coming nearest to the Virginian deer, or cariacou (*Cariacus virginianus*). From the examination of the antlers of these species he indicates that in the middle miocene age the cervine antler consisted of a simply forked crown, whilst in the upper miocene it becomes more complex, although still small and erect, like that of the roe deer. In the pliocene it becomes larger and more complex, some forms, such as the *Cervus dicranios*, Nesti, being the most complicated of known antlers. The successive changes are analogous to those observed in the development of the antlers of the living deer with increase of age. In the miocene we have the zero of antler-development, and the capreoline type is older than any other. The nearest living analogue of the miocene deer is, according to the antler, the muntjak (*Styloceros*), now found only in the oriental region of Asia, along with the tapir, which also co-existed with *Cervus dicranoceros* in the miocene forests of Germany. The pliocene deer, again, are generally most nearly allied to the oriental axis and rusa deer, the only exception being *Cervus cusanus*, the antlers of which resemble those of the roe, an animal widely spread over Europe and Northern and Central Asia. The alliance of these pliocene deer with those now living in the Indian region is regarded by the author as a further proof of the warm climate of Europe in miocene times, confirmatory of the conclusions arrived at by Saporta from the study of the vegetation.—On the occurrence of *Branchipus* (or *Chirocephalus*) in a fossil state, associated with *Archæoniscus*, and with numerous insect-remains in the eocene freshwater limestone of Gurnet Bay, Isle of Wight, by Henry Woodward, F.R.S. The remains of crustacea and insects noticed in this paper were obtained by Mr. E. J. A'Court Smith from a thin bed of limestone belonging to the Osborne or St. Helen's series at Thorness and Gurnet Bay in the Isle of Wight. The collection is the result of about twenty years' work. The insect-remains comprise about fifty specimens of diptera, including wings of tipulidæ and culicidæ, and the pupa apparently of a gnat, one wing of a hemipterous insect, and a flattened homopterous insect identified by Mr. F. Smith with *Triecphora sanguinolenta*; two specimens referred to the lepidopterous genus *Lithosia*; only three orthoptera, one a *Gryllotalpa*, the other two belonging to a grasshopper; thirty-five hymenopterous wings, thirty-three of which are referred to ants of the genera *Myrmica*, *Formica*, and *Camponotus*; twenty-three examples of neuroptera referred to *Termes*, *Perla*, *Libellula*, *Agrion*, *Phryganea*, and *Hemerobins*; and twelve of coleoptera, including species of *Hydrophilus*, *Dyticus*, *Curculio*, *Anobium*, *Dorcus*, and *Staphylinus*. There were also two spiders. Several species of bivalved entomostraca have also been obtained from

these deposits, and identified by Prof. Rupert Jones. Of the branchipod crustacean both sexes are fossilised and beautifully preserved, the males showing their large clasping antennæ, and the females their egg-pouches, with large and very distinct disc-like bodies representing the compressed eggs. Dr. F. Goldenberg notices a fossil from the coal-measures of Saarbrück which he regards as a branchipod, and describes and figures under the name of *Branchipusites* (rectè *Branchipodites*) *anthracinus*; but this interpretation of it is at least doubtful. The author names his species *Branchipodites vectensis.* The isopods accompanying this species are referred to the genus *Archæoniscus*, M.-Edw., and one of them is identified with the *Palæoniscus brongniarti* of Milne-Edwards. The other is probably a new species, perhaps nearly allied to the existing *Sphæroma serratum.*—The chronological value of the pleistocene deposits of Devon, by W. A. E. Ussher, F.G.S., of H.M. Geological Survey.

Entomological Society, January 16.—Anniversary Meeting.—Prof. J. O. Westwood, M.A., F.L.S., president, in the chair.—The following gentlemen were elected members of the council for the present year, viz.:—Henry Walter Bates, F.L.S., F.Z.S., G. C. Champion, W. L. Distant, J. W. Douglas, Rev. A. E. Eaton, M.A., E. A. Fitch, Ferdinand Grut, F.L.S., George Lewis, R. Meldola, F.R.A.S., F.C.S., Ewd. Saunders, F.L.S., Frederick Smith, J. Jenner Weir, F.L.S., Prof. J. O. Westwood, M.A., F.L.S.—Henry Walter Bates, F.L.S., F.Z.S., was elected president, and Messrs. J. J. Weir, treasurer, F. Grut, librarian, and R. Meldola and W. L. Distant, secretaries.—An address was read by the outgoing president, in which reference was made to many of the less accessible entomological memoirs of the past year. The address was ordered to be printed, and the meeting terminated with a vote of thanks to the officers of the Society.

PARIS

Academy of Sciences, January 14.—M. Fizeau in the chair.—The following papers were read:—On the presence of oxygen in metallic silver, by M. Dumas. He shows that in the numerous experiments where silver has been used in determination of equivalents, the chemists who, after careful purification, converted it into minute grains after fusion in presence of borax, nitre, and air, have made it liable to absorb oxygen varying from 50 to 200 cubic centimetres per kilogramme. Hence much discrepancy.—On the formation of oxygenated water, ozone, and persulphuric acid during electrolysis, by M. Berthelot. The oxidising substance formed in electrolysis of sulphuric solutions is not oxygenated water, as commonly supposed, nor ozone in simple solution, but (as proved by the fact of its not being formed in other solutions, and by its reactions positive and negative) persulphuric acid. It is formed with absorption of heat. The three substances named may be simultaneously formed in electrolysis. The ozone may be changed into oxygenated water by means of ether; the oxygenated water may be changed into persulphuric acid by concentrated sulphuric acid; and persulphuric acid liberates gradually in the cold state the whole of its oxygen at the ordinary state without presenting any finite tension of dissociation.—On the stability of ozone, by M. Berthelot.—Experimental researches on the fractures traversing the earth's crust, especially those known as joints and faults, by M. Daubrée. One end of a long rectangular plate of the substance to be examined was seized between wooden jaws, and the other end by a wrench which gave torsion. The nature of the fractures (in gypsum and glass) are described; geological deductions will follow in another paper.—On the recent tornado of Ercildoun (Chester Co., Pennsylvania), by M. Faye. He finds evidence in it that these great gyratory movements arise in the upper currents and travel with them; they are propagated downwards to the ground.—On a new bed of Adamine, by M. De Cloizeaux. This hydrated arseniate of zinc, found previously only at Chanarcillo, Chili, and in a mine of the Garonne, has now been found among the zinc ores of Laurium.—Note on the official report of last *séance*, by M. Pasteur.—The vibrations of matter and waves of ether; probable consequences of the fact which serves as base of the mechanical theory of heat, by M. Favé. *Inter alia*, the author gives an interpretation of the law of radiation and absorption, slightly differing from that given by Prof. Stokes.—On the liquefaction of gases, by M. Cailletet. Inclosing in the glass tube dry air freed from carbonic acid, he cooled with protoxide of nitrogen the upper part of the tube only. When the pressure was 200 atmospheres, streams of liquid (air) were seen flowing down the lower parts. When they met the mercury they seemed to turn back. At 310 atmospheres, the mercury being in contact with the cooled part of the tube, was frozen, and on quickly removing the refrigerating apparatus it was seen to be covered with what was probably *frozen air*.—Observations of the solar protuberances during the first six months of 1877, by P. Secchi. The figures are simply tabulated.—P. Secchi presented a copy of his new work (in Italian) entitled "The Stars; Essay in Sidereal Astronomy."—On telephony, by M. Breguet. The variations in conductivity of retort carbon have been utilised by M. Salet in a similar way to that of MM. Garnier and Pollard (see last week's report) with graphite, and with better results.—Study of the ultra violet solar spectrum, by M. Cornu. The first part of the memoir presented treats of the ultra violet spectrum from the line h' to the line O, observed with ordinary spectroscopes having glass objectives and prisms; the second part, from O to U, the ultra violet extremity observed photographically with a spectroscope with objectives of quartz and prism of Iceland spar. The limitation of the ultra violet spectrum is found to be caused by, and to vary with, the aqueous vapour in the atmosphere. The *maxima maximorum* of extent is at the summer solstice; but with equal height of the sun the spectrum is incomparably more extensive in winter than in summer.—On the fertility of volcanic soils, by M. Truchot. Phosphoric acid is the chief element of it.—Liquefaction of hydrogen, by M. Pictet.—On the question of the special conditions of contour of elastic plates, by M. Boussinesq.—On an industrial application of Gauss's theorem regarding the curvature of surfaces, by M. Levy.—On the function arising from development of the expression $(1 - 2ax + a^2a^2)^{\frac{-2l+1}{2}}$, by M. Escary.—On a theorem of M. Chasles, by M. Serret.—On the function of Jacob Bernouilli and on the interpolation, by M. Lipschitz.—On the preparation of curare, by M. Jobert.—Palæontological contributions, by M. Meunier.—Effect of a low temperature on a mixture of oxygenated water and sulphuric acid, by M. Boillot.

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THURSDAY, JANUARY 31, 1878

TAIT'S "THERMODYNAMICS"

Sketch of Thermodynamics. By P. G. Tait, M.A., formerly Fellow of St. Peter's College, Cambridge, Professor of Natural Philosophy in the University of Edinburgh. Second Edition, revised and extended. (Edinburgh: David Douglas, 1877.)

THIS book, as we are told in the preface, has grown out of two articles contributed in 1864 by Prof. Tait to the *North British Review.* This journal, about that time, inserted a good many articles in which scientific subjects were discussed in scientific language, and in which, instead of the usual attempts to conciliate the unscientific reader by a series of relapses into irrelevant and incoherent writing, his attention was maintained by awakening a genuine interest in the subject.

The attempt was so far successful that the publishers of the *Review* were urged by men of science, especially engineers, to reprint these essays of Prof. Tait, but the *Review* itself soon afterwards became extinct.

Prof. Tait added to the two essays a mathematical sketch of the fundamental principles of thermodynamics, and in this form the book was published in 1868. In the present edition, though there are many additions and improvements, the form of the book is essentially the same.

Whether on account of these external circumstances, or from internal causes, it is impossible to compare this book either with so-called popular treatises or with those of a more technical kind.

In the popular treatise, whatever shreds of the science are allowed to appear, are exhibited in an exceedingly diffuse and attenuated form, apparently with the hope that the mental faculties of the reader, though they would reject any stronger food, may insensibly become saturated with scientific phraseology, provided it is diluted with a sufficient quantity of more familiar language. In this way, by simple reading, the student may become possessed of the phrases of the science without having been put to the trouble of thinking a single thought about it. The loss implied in such an acquisition can be estimated only by those who have been compelled to unlearn a science that they might at length begin to learn it.

The technical treatises do less harm, for no one ever reads them except under compulsion. From the establishment of the general equations to the end of the book, every page is full of symbols with indices and suffixes, so that there is not a paragraph of plain English on which the eye may rest.

Prof. Tait has not adopted either of these methods. He serves up his strong meat for grown men at the beginning of the book, without thinking it necessary to employ the language either of the nursery or of the school; while for younger students he has carefully boiled down the mathematical elements into the most concentrated form, and has placed the result at the end as a *bonne bouche*, so that the beginner may take it in all at once, and ruminate upon it at his leisure.

A considerable part of the book is devoted to the history of thermodynamics, and here it is evident that with Prof. Tait the names of the founders of his science call up the ideas, not so much of the scientific documents they have left behind them in our libraries, as of the men themselves, whether he recommends them to our reverence as masters in science, or bids us beware of them as tainted with error. There is no need of a garnish of anecdotes to enliven the dryness of science, for science has enough to do to restrain the strong human nature of the author, who is at no pains to conceal his own idiosyncrasies, or to smooth down the obtrusive antinomies of a vigorous mind into the featureless consistency of a conventional philosopher.

Thus, in the very first page of the book, he denounces all metaphysical methods of constructing physical science, and especially any *à priori* decisions as to what may have been or ought to have been. In the second page he does not indeed give us Aristotle's ten categories, but he lays down four of his own:—matter, force, position, and motion, to one of which he tells us, "it is evident that every distinct physical conception must be referred," and then before we have finished the page we are assured that heat does not belong to any of these four categories, but to a fifth, called energy.

This sort of writing, however unlike what we might expect from the conventional man of science, is the very thing to rouse the placid reader, and startle his thinking powers into action.

Prof. Tait next handles the caloric theory, but instead of merely showing up its weak points and then dismissing it with contempt, he puts fresh life into it by giving (in the new edition) a characteristic extract from Dr. Black's lectures, and proceeds to help the calorists out of some of their difficulties, by generously making over to them some excellent hints of his own.

The history of thermodynamics has an especial interest as the development of a science, within a short time and by a small number of men, from the condition of a vague anticipation of nature to that of a science with secure foundations, clear definitions, and distinct boundaries.

The earlier part of the history has already provoked a sufficient amount of discussion. We shall therefore confine our remarks to the methods employed for the advancement of the science by the three men who brought the theory to maturity.

Of the three founders of theoretical thermodynamics, Rankine availed himself to the greatest extent of the scientific use of the imagination. His imagination, however, though amply luxuriant, was strictly scientific. Whatever he imagined about molecular vortices, with their nuclei and atmospheres, was so clearly imaged in his mind's eye, that he, as a practical engineer, could see how it would work.

However intricate, therefore, the machinery might be which he imagined to exist in the minute parts of bodies, there was no danger of his going on to explain natural phenomena by any mode of action of this machinery which was not consistent with the general laws of mechanism. Hence, though the construction and distribution of his vortices may seem to us as complicated and arbitrary as the Cartesian system, his final deductions are simple, necessary, and consistent with facts.

Certain phenomena were to be explained. Rankine

set himself to imagine the mechanism by which they might be produced. Being an accomplished engineer, he succeeded in specifying a particular arrangement of mechanism competent to do the work, and also in predicting other properties of the mechanism which were afterwards found to be consistent with observed facts.

As long as the training of the naturalist enables him to trace the action only of particular material systems without giving him the power of dealing with the general properties of all such systems, he must proceed by the method so often described in histories of science—he must imagine model after model of hypothetical apparatus till he finds one which will do the required work. If this apparatus should afterwards be found capable of accounting for many of the known phenomena, and not demonstrably inconsistent with any of them, he is strongly tempted to conclude that his hypothesis is a fact, at least until an equally good rival hypothesis has been invented. Thus Rankine,[1] long after an explanation of the properties of gases had been founded on the theory of the collisions of molecules, published what he supposed to be a proof that the phenomena of heat were invariably due to steady closed streams of continuous fluid matter.

The scientific career of Rankine was marked by the gradual development of a singular power of bringing the most difficult investigations within the range of elementary methods. In his earlier papers, indeed, he appears as if battling with chaos, as he swims, or sinks, or wades, or creeps, or flies,

> "And through the palpable obscure finds out
> His uncouth way;"

but he soon begins to pave a broad and beaten way over the dark abyss, and his latest writings show such a power of bridging over the difficulties of science, that his premature death must have been almost as great a loss to the diffusion of science as it was to its advancement.

The chapter on thermodynamics in his book on the steam-engine was the first published treatise on the subject, and is the only expression of his views addressed directly to students.

In this book he has disencumbered himself to a great extent of the hypothesis of molecular vortices, and builds principally on observed facts, though he, in common with Clausius, makes several assumptions, some expressed as axioms, others implied in definitions, which seem to us anything but self-evident. As an example of Rankine's best style we may take the following definition:—

"A PERFECT GAS is a substance in such a condition that the total pressure exerted by any number of portions of it, at a given temperature, against the sides of a vessel in which they are inclosed, is the sum of the pressures which each portion would exert if inclosed in the vessel separately at the same temperature."

Here we can form a distinct conception of every clause of the definition, but when we come to Rankine's Second Law of Thermodynamics we find that though, as to literary form, it seems cast in the same mould, its actual meaning is inscrutable.

"*The Second Law of Thermodynamics.*—If the total actual heat of a homogeneous and uniformly hot substance be conceived to be divided into any number of equal parts, the effects of those parts in causing work to be performed are equal."

We find it difficult enough, even in 1878, to attach any distinct meaning to the total actual heat of a body, and still more to conceive this heat divided into equal parts, and to study the action of each of these parts, but as if our powers of deglutition were not yet sufficiently strained, Rankine follows this up with another statement of the same law, in which we have to assert our intuitive belief that—

"If the absolute temperature of any uniformly hot substance be divided into any number of equal parts, the effects of those parts in causing work to be performed are equal."

The student who thinks that he can form any idea of the meaning of this sentence is quite capable of explaining on thermodynamical principles what Mr. Tennyson says of the great Duke:—

> "Whose eighty winters freeze with one rebuke
> All great self-seekers trampling on the right."

Prof. Clausius does not ask us to believe quite so much about the heat in hot bodies. In his first memoir, indeed, he boldly dismisses one supposed variety of heat from the science. Latent heat, he tells us, "is not only, as its name imports, hidden from our perceptions, but has actually no existence;" "it has been converted into work."

But though Clausius thus gets rid of all the heat which, after entering a body, is expended in doing work, either exterior or interior, he allows a certain quantity to remain in the body as heat, and this remnant of what should have been utterly destroyed lives on in a sort of smouldering existence, breaking out now and then with just enough vigour to mar the scientific coherence of what might have been a well compacted system of thermodynamics.

Prof. Tait tells us:—

"The source of all this sort of speculation, which is as old as the time of Crawford and Irvine—and which was countenanced to a certain extent even by Rankine—is the assumption that bodies must contain a certain quantity of actual, or thermometric, heat. We are quite ignorant of the condition of energy in bodies generally. We know how much goes in, and how much comes out, and we know whether at entrance or exit it is in the form of heat or of work. But that is all."

If we define thermodynamics, as I think we may now do, as the investigation of the dynamical and thermal properties of bodies, deduced entirely from what are called the First and Second laws of Thermodynamics, without any hypotheses as to the molecular constitution of bodies, all speculations as to how much of the energy in a body is in the form of heat are quite out of place.

Prof. Tait, however, does not seem to have noticed that Prof. Clausius, in a footnote to his sixth memoir,[1] tells us what he means by the heat in a body. In the middle of a sentence we read:—

". . . . the heat actually present in a unit weight of the substance in question—in other words, the *vis viva* of its molecular motions" . . . .

Thus the doctrine that heat consists of the *vis viva* o

[1] On the Second Law of Thermodynamics. *Phil. Mag.* Oct. 1865, § 12, p. 244; but in his paper on the Thermal Energy of Molecular Vortices, *Trans.* R.S. Edin., xxv. p. 557 [1869] he admits that the explanation of gaseous pressure by the impacts of molecules has been proved to be possible.

[1] Hirst's translation, p. 230, German edition, 1864, p. 258, "wirklich vorhandene Wärme, d.h. die lebendige Kraft seiner Molecularbewegungen."

molecular motions, and that it does not include the potential energy of molecular configuration—the most important doctrine, if true, in molecular science—is introduced in a footnote under cover of the unpretending German abbreviation "d.h."

J. CLERK MAXWELL

(*To be continued.*)

WOLF'S HISTORY OF ASTRONOMY

Geschichte der Astronomie. Von Rudolf Wolf. (München: R. Oldenbourg, 1877.)

THE "History of Astronomy," by Prof. Rudolf Wolf, of Zurich, a volume of 800 pages issued at a very moderate figure, is a contribution to the literature of the science of no ordinary value to the student. The production of such a work, involving an outline of the progress of astronomy from the earliest times to the present period, must have been a labour of great extent, requiring much research, notwithstanding the assistance that might be afforded by historical treatises previously in the hands of astronomers, and it is only due to Prof. Wolf to acknowledge the very able and complete manner in which he has accomplished the heavy task he had imposed upon himself some years since.

Those of our readers who may have been desirous of acquainting themselves with the general history of practical astronomy, and of familiarising themselves with the names and the nature of the services of the principal workers who have successively contributed to advance our knowledge of the science, more especially during the last three centuries, will, we think, have experienced difficulties which the volume before us is well calculated to obviate. The English reader has, it is true, Prof. Grant's classical work, the "History of Physical Astronomy," but there is much to be found in this volume, which it was hardly within the scope of Prof. Grant's work to incorporate. The writer of these lines very well remembers the fragmentary manner in which, some thirty-five years since, an English student of practical astronomy was under the necessity of obtaining information, more especially in private reading; and it is one of the most happy circumstances for the astronomical student of the present day that this want of suitable guides has been to a great extent removed, and his time therefore need not be wasted in a search for knowledge in second-rate or doubtful authorities, mistakes which he would be not infrequently led into thereby, being corrected only after vexatious delay and trouble.

Prof. Wolf divides his work into three books. The first deals with ancient astronomy and progress down to the fifteenth century, including theories, instruments, and writings. The second commences with "the reformation of astronomy" consequent on the publication of the great work of Copernicus, "De Revolutionibus Orbium Cœlestium," and treats of the advances made to the time of Newton; we find therefore in this division a summary of the labours of Galileo, Apian, Tycho Brahe, Kepler, Fabricius, Harriot, Hevelius, Huyghens, Gascoigne, and many others, including notices of the more important publications of the period, which are of interest and value. The third book treats of "the new astronomy," commencing with the discovery of universal gravitation and brings down the history of astronomical research and discovery to the present epoch. A very great amount of information is compressed into this last section of the work, and it is here that the care and research of the author are more particularly evidenced. There is much to be found in it, for which we should look in vain in a collective and compendious form elsewhere. It is well and accurately put together, the few errors we have remarked being of comparatively trifling nature; thus the Saturnian satellite *Tethys* appears as *Thetis*. The biographical notes, which are extended to contemporary astronomers, will be a welcome feature to many readers.

Students and others interested in the history of the most ancient of the sciences, who can command a sufficient knowledge of the German language, will find their advantage in the possession of Prof. Wolf's elaborate work, and we must not omit to say that that great desideratum in all works of the kind—a very sufficient index, at least as regards names mentioned in the history, will render it of easy reference.

J. R. HIND

(*To be continued.*)

OUR BOOK SHELF

Photographic Spectra. 136 Photographs of Metallic, Gaseous, and other Spectra printed by the Permanent Autotype Process. With Introduction, Description, &c., by J. R. Capron, F.R.A.S. (E. and F. N. Spon.)

WE gather from the author's introduction that he has chiefly aimed "to popularise a subject hitherto somewhat of a sealed book, confined to the laboratories of workers in special research." In this he should certainly succeed, though we think that his readers would not have been driven away if they had found a little more reference to the explanations of the various phenomena and the conclusions which have been drawn from them. As it is, the book is a good companion to Lecoq de Boisbaudran's "Spectres Lumineux." The spectra are sharp and clear, and the autotype process has lent itself well to this reproduction. The results are all the more commendable because Mr. Capron has not had the advantages of considerable dispersion.

The account of the method employed is full and clear, and will make the book a very useful one to beginners in spectrography.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Sun-spots and Terrestrial Magnetism

PROF. PIAZZI SMYTH will no doubt welcome from any quarter a satisfactory answer to his question about the discrepancy between Dr. Wolf's sun-spot period, 11·1 years, and the supposed 10·5 years' period for the magnetic needle. If Mr. Smyth will refer to Prof. Loomis's chart of magnetic oscillations given in Prof. Balfour Stewart's paper on the subject in NATURE (vol. xvi. p. 10), he will see that there are exactly seven minimum-periods from 1787 to 1871, the mean of which is twelve years; the mean of the seven corresponding maximum-periods is 11·8 years. The true magnetic declination-period is then the mean of these, viz., 11·9 years. In exactly the same manner I have found that the mean period of sun-spots is 11·9 years. The auroral displays also have the same period.

But what is this period of 11·9 years? It is Jupiter's anomal-

istic year, or the time which elapses between two perihelion passages.

Prof. Wolf and Messrs. De la Rue, Stewart, and Loewy have all distinctly stated their belief that Jupiter is the chief cause in the production of sun-spots. This 11·9 years' period will then, I believe, remove what little doubt remains in some minds on the subject. Mr. John Allan Broun, F.R.S., has already shown in NATURE (vol. xvi. p. 62) that Dr. Wolf, to be consistent with his own relative numbers, ought to take a period of 11·94 years rather than one of 11·1, and while he himself favours a 10·5 years' period, he admits that there is no combination of planetary positions which would produce such.

I may perhaps be allowed to state here that in a paper I have just forwarded to the Royal Astronomical Society I have given what I believe are satisfactory reasons for the variations of these curves, and such as will enable us for the future to calculate with considerable accuracy the lengths of the periods, and guided by these reasons I have ventured to state my belief that we are now passing through a long minimum-period—one very similar to that which occurred at the close of the last century, and that the next *maximum* of sun-spots will fall in the year 1887.

I make this statement from an examination of the causes which produce the sun-spots; and it is so far remarkably confirmed by the behaviour of the magnetic needle. Mr. Broun, in NATURE, vol. xvii. p. 183, speaking of the very gradual manner in which the curve has been going to a minimum during the last three and a half years, remarks that "no such constant state of the sun's magnetic action will have been observed since the last years of the eighteenth century." To this I would add that immediately prior to the commencement of that long sun-spot minimum period, the mean of the magnetic interval, which occurred then (reckoning the interval from minimum to maximum), fell in the year 1785, and corresponded with the time of Jupiter's perihelion passage. Suppose now we represent this synchronism by 0, it will be found that the mean point in the next period lagged behind the perihelion 1·6 year; next, 5·3 years; next, 5·3 years. Having reached its maximum of lagging, in the next period it lagged 3·9 years; next, 1·2 year; next, 0·6 year; and in the last period the mean point fell in the year 1868, coinciding for the first time since 1785 with Jupiter's perihelion, and will be represented by 0. So that the magnetic oscillation in 1868 was just where it was in 1785. Is it not a natural inference, then, that we have commenced another cycle of magnetic declination?

What produces this lagging? This is a very important question, and one which I have reason to believe can be satisfactorily answered. B. G. JENKINS

January 19

On a Means for Converting the Heat Motion Possessed by Matter at Normal Temperature into Work

MY attention has just been directed to Mr. S. Tolver Preston's two papers in NATURE, vol. xvii. p. 31 and p. 202, in which he points out what appears to be an exception to the second law of thermodynamics. Some years ago I illustrated the same subject in a somewhat different manner by an experiment which is in some respects better suited for lecture purposes, and while the subject is beng considered may be useful to your readers.

Into the cork of a large bottle were fitted two glass tubes. One tube went to the bottom of the bottle, its upper end being terminated in a fine jet. The other tube only passed a short distance into the bottle, and its upper end terminated about an inch above the cork. To its lower end was fixed some pieces of blotting-paper, to its upper end was attached a small test-tube, the two being connected by means of a piece of india-rubber tube. Some water was put in the bottle and the cork fitted close in its place. The test-tube was then filled with ether or some volatile fluid, and fitted to the end of the india-rubber tube.

After the apparatus had attained a uniform temperature, the test-tube was inverted, so as to cause the ether to flow down the tube, and enter the bottle, where it spread itself over the blotting paper and, rapidly evaporating, produced a pressure inside the bottle. The addition of the ether vapour to the air already at atmospheric pressure, produced a pressure sufficient to force the water up the tube and out of the jet, causing it to rise to a considerable height into the air. At the beginning of the experiment all the apparatus was at a uniform temperature, and, according to the generally received opinion, ought to have been incapable of developing energy, yet on account of the ether vapour not being diffused through the system, it was able to do work at the expense of part of the heat in the system. JOHN AITKEN

Darroch, Falkirk, January 18

No Butterflies in Iceland

ALLOW me to point out that the lepidopterous insects said by Olafsen (not Olaffson) and N. (not R.) Mohr, to be found in Iceland, are not butterflies at all, but moths, as shown by the generic term *Phalæna* applied by each of those authors to every one of them—a term whose meaning your correspondent and his informant have failed to see. Those venerable authors, though dead and buried long before I ever heard of them, are old friends of mine, and I feel it incumbent on me to ask your readers not to impute to them this and other mistakes in Dr. Rae's letter. Whether there have been or still be butterflies in Iceland I am not competent to declare. I did not see any when I was there, but they may have got out of my way. I have, however, yet to learn that they exist in that country, and therefore I am inclined to believe Mr. McLachlan is right when he said that there are none. We have the testimony of the late Sir William Hooker ("Tour," &c., ed. 2, vol. i. p. 333) that no butterfly had ever been met with in Iceland up to 1809, the year in which he visited that island. Gliemann ("Geogr. Beschreib. Isl.," p. 165) in 1824 was unable to add to Mohr's list of twelve species of moths, and included no butterflies. If any of the latter have since been found it would be well for Dr. Rae to give his authority for the fact, otherwise his ingenious supposition that Icelandic butterflies and their larvæ have been destroyed since 1786, is unnecessary, and his "only possible way" of reconciling "perfectly opposite authorities" falls to the ground through the absence of any opposition on the part of the authorities he has cited. ALFRED NEWTON

Magdalene College, Cambridge, January 25

[Dr. Rae writes "to explain and correct a mistake which, by a little care and attention on my part could and should have been so easily avoided."]

On some Peculiar Points in the Insect-Fauna of Chili

MY friend Mr. Birchall misconstrued the meaning of my notes (NATURE, vol. xvii. p. 162) in a manner incomprehensible to me, when penning his own (p. 221). I, and many others, will share his "surprise" when he can produce any species of the genera *Carabus*, *Argynnis*, and *Colias*, or any of the *Limnophilidæ* from Australia or New Zealand. If he will do me the favour to again read my notes he will find that I refer solely to Palæarctic and Nearctic forms occurring in the Chilian subregion and (unless by exception) nowhere else in the southern hemisphere.

Mr. Wallace's rebuke (p. 182) is to some extent merited. I *did not* give sufficient attention to the chapter in his work, to which he refers, in consequence of its *general* character. Mr. Wallace greatly extends the number of genera published by me as a sample. Some of these were perfectly familiar to me; others, I think, will fail to stand the test of minute application, partly because their distribution is more extended, partly because generic definitions are vague. I could add several interesting and marked genera. *Colias* may possibly be represented by more than one species on the Northern Andes; but it is the opinion of naturalists who, from practical acquaintance with the fauna of South America, and who, on a special point like this, are more competent than I to judge, that most of the very marked forms upon which I especially rely *do not* occur on the Northern Andes, which of late have been most assiduously worked by entomologists hunting insects for sale and perfectly alive to the value of such forms.

Mr. Darwin's theory alluded to by Mr. Birchall had not been overlooked. I was dealing with insects, and with a few marked genera, &c., of them, only. In plants there appears to be a tendency towards the appearance of analogous or identical forms all over the world when a sufficient altitude (varying according to the latitude) is reached. The laws that govern the distribution of the one ought equally to affect the other. Still the facts alluded to in my former letter remain unexplained. The southern portion of South America forms, as it were, an island, with a large admixture of Palæarctic and Nearctic faunistic elements existing in no other part of the southern hemisphere.

Lewisham, January 19 ROBERT McLACHLAN

The Radiometer and its Lessons

PROF. OSBORNE REYNOLDS arranges his last letter (NATURE, vol. xvii. p. 220) under four numbered heads, and in the reply which I appear called on to make I will follow this division.

1. In the first section he says, "There is nothing in my earlier papers that is 'admittedly erroneous.' If there is error in these papers I am not aware of it." This is strange. In his first paper (*Proceedings*, Royal Society, vol. xxii. p. 40) Prof. Reynolds declares its object to be "to point out and to describe experiments to prove that these effects (the motions observed by Mr. Crookes) are the results of evaporation and condensation." Now they are not the results of evaporation and condensation: and it might have been seen, *ab initio*, that evaporation and condensation could have had nothing to do with them; for evaporation and condensation can only produce a temporary force, ceasing so soon as the distillation is complete, and cannot therefore be any part of the cause of a persistent force such as that detected by Mr. Crookes, which lasts for any length of time during which the heat is applied. In the same paper Prof. Reynolds further says, "The reason why Mr. Crookes did not obtain the same results within a less perfect vacuum [than that of the Sprengel pump], was because he had then too large a proportion of air, or non-condensing gas, mixed with the vapour, which was also not in a state of saturation." All this is manifest error. But this is not all, for *the whole of the theory* of those papers is erroneous; neither condensable vapour nor residual gas acts in the way described by Prof. Osborne Reynolds. In investigating the force arising from evaporation and condensation, he has overlooked the circumstance that the evaporation from the disc will keep back part of the vapour which would otherwise have reached it, and in investigating the effect of condensation he tacitly assumes that it does keep it back. Now in both cases the reverse of the assumption is what takes place, and he actually arrives at the absurd result that "if the opposite sides of a pith ball in vapour were in such different conditions [*i.e.*, one surface evaporating, the other condensing] the ball would be forced towards the colder side" (p. 404). His conclusion amounts to this: that the recoil of a cannon would be doubled if it were struck from behind by a missile at the same moment that it discharges an equal mass with equal velocity forwards! If he had not made these mistakes he would have got out only the forces which result from "the perceptible motion of the vapour," which he states "would be insensible" (p. 403), along with alterations in the general tension of the vapour *which would act equally on both sides of the disc.* Those errors vitiate the whole of his mathematical reasoning, so that the value for f which he gets, is not, as he supposes, "the force arising from evaporation," and his law connecting it with the heat falls to the ground. I have all along supposed, from Prof. Reynolds's having long ceased to mention his theory of evaporation and condensation, that he was aware of some of its errors.

The same error vitiates his reasoning in reference to the action of residual gas. If the error is corrected, and if, as he assumes, the gas coming up to the disc had been unpolarised (*i.e.* had brought to the disc equal numbers of molecules, and with equal velocities from all directions in front), his investigation *would only have given him an increase in the general pressure of the gas*, acting, as I pointed out in paragraph 5 of my first paper (*Phil. Mag.*, March, 1876, p. 179), equally on the front and back of the disc, except during the almost inappreciable instant of adjustment. *Prof. Osborne Reynolds therefore wholly missed the source of the persistent force with which Mr. Crookes's experiments deal.*

2. Prof. Osborne Reynolds next says that his second paper "does not conclude with his own expression of opinion that residual gas is not the cause of the force observed by Mr. Crookes." In reply I have only to quote the concluding words of that paper (*Phil. Mag.*, November, 1874, p. 391). After passing under review the two agencies (condensable vapour and residual gas), which he supposes are to be considered, he decides in favour of the former in the following words: "hence in such cases [*i.e.*, under the conditions which he supposed to prevail in Mr. Crookes's experiments] it seems to me that the effects must be due to the forces of condensation."

3. In the next section of his letter Prof. Reynolds states that Clausius and Maxwell "established the law that the only condition of thermal equilibrium in a gas is that of uniform temperature." I am not aware that they have ever established this law. The converse of it is obviously true, and has often been used, and the law itself has sometimes been assumed, but has never, so far as I know, been proved. I am, however, disposed to concur with those who think that it is probably true, and the conclusion in my paper on penetration (which is the reverse of that attributed to me by Prof. Reynolds) is in conformity with it. My conclusion is expressed in the following words (*Phil. Mag.*, December, 1877, § 4):—"Hence there must, in the cases that really arise, be some escape of heat which may be small but cannot vanish." And, I may remark, there will, according to my view, be two other sources of escape of heat, viz., conduction by diffusion, which was excluded from my investigation; and conduction by radiation, which was excluded both from Clausius's investigation (*Phil. Mag.*, June, 1862, p. 422, footnote) and from mine.

Prof. Osborne Reynolds a second time objects to my having excluded conduction when investigating the penetration of heat. As he attaches weight to authority he will perhaps be reconciled to my doing so, by the example of Clausius, as cited above, and by his justification of it in the following words (*loc. cit.*):—"In any case, however, it is allowable to consider separately each of these two ways in which heat moves."

Before passing from this subject I wish to take the opportunity of stating that Dr. Schuster's letter (NATURE, vol. xvii. p. 143) has satisfied me that I have hitherto erred in my estimate of the relative efficiency of penetration and conduction as agents for conveying heat. I am now convinced that penetration is usually feeble compared with conduction, and, in the figures representing De la Prevostaye and Desains' experiments, is to be sought in those portions of the curves which slope steeply downwards. The second part of my paper on penetration, that in which I apply the theory to experiment, will accordingly require considerable modification; and some of the statements which I made in my papers on Crookes's force will need amendment. The corrections that are required do not, however, affect any of the material parts of my theories of Crookes's force and of penetration, which depend essentially on the fact that there is a layer in the gas extending to a limited distance from a heater or cooler, throughout which the effects of the discontinuity in the gaseous motions at the surface will be felt, and that within that layer the stresses and the communication of heat follow special laws.

4. I have to express my great satisfaction at the explicit admission made by Prof. Osborne Reynolds in the fourth section of his letter, in the following sentences:—"There is one statement in Mr. Stoney's letter which is not erroneous. He says, 'I cannot find anywhere in Prof. Osborne Reynolds's writings an explanation of the thing to be explained, viz., that the stress in a Crookes's layer is different in one direction from what it is at right angles to that direction.' I [Prof. Osborne Reynolds] do not at all admit that this is 'the thing to be explained,' and I am quite sure that Mr. Stoney would find no explanation of it in my writings." This admission disposes finally of all controversy as to priority between us.

I need hardly, after this admission, follow Prof. Osborne Reynolds through the rest of his letter. His supposed invariable law "that the [Crookes's] force always tends to drive the vanes or bodies in the direction of their colder faces," does not seem to be true. A familar exception occurs when a spheroidal drop is supported over a platinum dish. The Crookes's force acting upon the platinum dish is equal to the weight of the drop, and acts downwards, *i.e.* in the direction of the *hottest* surface of the dish.

In applying his hypothetical case of a heater and cooler, A and B, within an envelope of intermediate temperature, to prove that "the force that causes the motion in the bodies cannot be due" to the stresses of my theory, he has overlooked the very obvious circumstance that the envelope, as well as B, is a cooler in reference to A, and the envelope, as well as A, is a heater in reference to B.

Prof. Reynolds observes that I have not defined polarisation. I described the kind of polarisation that exists in radiometers in my first two papers, and I will give a formal definition of the term as applied generally to gases in an article which I am preparing, and which I hope will be admitted into the pages of NATURE, giving as clear an account of my theory as I can, compatibly with brevity and the omission of mathematics.

The way in which Prof. Reynolds has excluded polarisation from his explanation is by assuming that the state of the gas close to the heated disc may be adequately represented by unpolarised gas of one temperature coming up to the disk, and unpolarised gas of another temperature leaving it, *i.e.*, by mole-

cules coming up to the heater in equal numbers and with equal velocities from *all* directions in front, and by molecules receding from the heater equally in all directions, although with augmented velocities. Under these circumstances there would be no difference in the pressure on the front and back of the disc, except during the very brief period of adjustment.

By making this assumption Prof. Reynolds leaves the part of Hamlet out of the play; for Crookes's force arises out of the very circumstance which has been omitted, viz., that the molecules that come up to the heater or cooler, arrive in the form of a rain which predominates in a definite direction, a direction which is normal to the heater and cooler in the simple case of their being parallel.

G. JOHNSTONE STONEY

A Double Rainbow

ON the 28th inst., at about 6.30 P.M. while myself and some ten or twelve other gentlemen were playing cricket, we were surprised to see what we all considered a most novel phenomenon—*a double rainbow.* The sky was cloudy and the weather was thundery. At the time referred to a shower of rain fell; the sun was about 10° above the horizon, shining out very brilliantly and reflecting upon the waters of St. Vincent's Gulf. Great wonder was expressed at the strange appearance, and much curiosity as to the cause.

The appearance was as follows:—There were two distinct and well-defined bows; the feet were united, but the apices were a considerable distance apart.

I am of opinion that the lower bow was caused by the direct light of the sun, while the light reflected from the sea produced the upper one.

THOMAS NOYÉ

Willunga, South Australia, November 30

SCIENCE IN TRAINING COLLEGES

THE Science and Art Department has just issued a circular having an important bearing on the teaching of science is to take in our training colleges, and therefore also in elementary schools.

The Lords of the Committee of Council on Education believe that the time has arrived when a special examination should be instituted at a period of the year better adapted to the training colleges than May, and that the nature of the examination and the payments made on the results should be modified to suit the circumstances of those colleges. They have therefore determined that in future a special examination in science shall be held in training colleges in December, immediately before the ordinary Christmas examination.

The examination will not be open to acting teachers. It will be held in those subjects only for which a special course of instruction is provided in the time-table of the College, and will be conducted by one of her Majesty's inspectors or by an officer of the Science and Art Department. Special committees will no longer be required for the training colleges; such returns as are necessary will be made by the principal. No student in a training college will be allowed to attend the May examinations of the Science and Art Department, except in physical geography in May, 1878.

The examination will be confined to the following nine subjects:—1. Mathematics. 2. Theoretical Mechanics. 3. Applied Mechanics. 4. Acoustics, Light, and Heat. 5. Magnetism and Electricity. 6. Inorganic Chemistry, including Practical Chemistry. 7. Animal Physiology. 8. Elementary Botany. 9. Physiography.

No student will be permitted to take up more than two subjects in any one year, and women will not be permitted to take more than one subject in a year.

The examination, except for mathematics, will be based on the syllabus of the several subjects given in the Science Directory; but the two stages, elementary and advanced, will be treated as a whole—one paper only being set. These examination papers will be framed much as the present May papers are framed, that is to say, with a certain number of compulsory questions and a certain number of optional questions, some of the latter being more difficult and more highly marked than the rest. Questions will also be set on the method of teaching various branches of the subject.

The successful students will be placed in the first or second class, the standard for a second class being as high as that of a *good* second class in the present advanced stage, and for the first class of a *good* first class in the advanced stage. All students who pass will be registered as qualified to earn payments on results and will receive certificates, but no prizes will be given. A payment of 3*l.* will be made on account of each first class, and 1*l.* 10*s.* on account of each second class obtained by a student in a training college.

In addition to the payments for theoretical chemistry, payments will be made for practical chemistry, of the same amounts and on the same conditions as those detailed in the Science Directory, § XLV. The circular contains an appendix with a syllabus of the subjects for mathematics in training colleges. We should advise all interested in this matter to obtain a copy of the circular.

SUN-SPOTS AND TERRESTRIAL MAGNETISM

I HAVE seen only to-day the number of NATURE (vol. xvii. p. 220) containing a letter from Prof. Piazzi Smyth on the above subject. I have also just now seen for the first time a communication from M. Faye to the French Academy of Sciences on July 30 last, in which there is a reference to the same subject; this I regret much, as M. Faye, through an incomplete acquaintance with my investigations, has drawn conclusions from one of them which are not exact. I shall at present refer only to the subject of Prof. Smyth's letter.

M. Faye considers the difference of the periods found by Dr. Lamont and myself for the diurnal oscillations of the magnetic needle (10·45 years) and by Dr. Wolf from the sun-spots (11·11 years), a sufficient proof that these cycles are not synchronous, and therefore that there is no causal connection between the two phenomena. Prof. Smyth asks an explanation relatively to this difference, upon the supposition that the two periods found are the true mean durations of the cycles for the respective phenomena. This supposition, however, is erroneous, and consequently M. Faye's deductions from it fail.

I have shown in a paper cited by M. Faye[1] that if we determine the epoch of the maximum diurnal oscillation of the needle from Cassini's observations made at Paris, and from Gilpin's observations made at London, we find it to have occurred in 1787·25. This epoch agrees very nearly with that deduced by Dr. Wolf for the maximum of sun-spots. If we compare this epoch with that of the last maximum which occurred for both phenomena near the end of 1870, we shall obtain a mean duration of 10·45 years, upon the assumption that eight cycles happened between these two epochs. There is no difference between Dr. Wolf and the magneticians excepting upon the question whether there were eight or only seven cycles. Dr. Lamont considers that the data existing between 1787 and 1818 are worthless for a decision upon this point, and by induction from the known cycles has concluded that three cycles must have occurred in the thirty-one years 1787 to 1818. Dr. Wolf believes there were only two. I have given the evidence which makes the existence of three extremely probable. This question has no relation whatever to the synchronism of the two phenomena.

If we could accept Dr. Wolf's view we should find, as I have shown, that the mean duration of a cycle for *both* phenomena since 1787 would be 11·94 years, while the sun-spot results for eight cycles determined by Dr.

[1] "On the Decennial Period," *Edinb. Trans*, vol. xxvii.

Wolf during eighty years *before* 1787 give 10·23 years (or, if we take nine cycles, 10·43 years) for the mean duration. It is by mixing these two very different means that the Zurich astronomer finds 11·1 years, a mean that can evidently have no weight given to it. On the other hand, if Dr. Wolf is in error (as I believe he is) as to the existence of a maximum in 1797, the mean durations for the eighty years after, and for the eighty years before 1787 agree as nearly as the accuracy of the determinations for the beginning of the eighteenth century will admit.

I beg, then, to repeat that since the time when regular series of magnetic observations were commenced, till now, there is no difference whatever between Dr. Wolf and the magneticians as to the synchronism of the two phenomena.

Under these circumstances we come to the question—Are the sun-spot maxima and minima really synchronous with those of the magnetic diurnal oscillations? I have already said that this was so in 1787; and, considering only the cases for which we have complete materials for comparison, beginning with Schwabe's observations of sun-spots, it was so for the maxima of 1829, 1837, 1848, 1860, and 1870, and for the minima of 1824, 1833-4 (*q. p.*), 1844, 1856, 1866, and it is the case for the minimum at the present time. These coincidences are far more important, as showing a common cause, than may appear at first sight from this summary.

The successive oscillations of the sun-spot variations are not performed in equal times, neither are those of the magnetic variations. Was the duration of the oscillation for the sun-spots only eight years, as from the maximum in 1829 to that of 1837, so was that for the magnetic variations; did it amount to 12½ years nearly, for the sun-spots, as from the minimum of 1844 to that of 1856, this was also the case for the oscillations of the needle. Does the sun-spot variation proceed from a minimum to a maximum within about three and a half years as from 1833–4 to 1837, so does the magnetic oscillation. Does the sun-spot variation occupy nearly eight years between a maximum and the following minimum, as from 1848 to 1856, so does the diurnal oscillation of the needle.

It will be difficult to persuade physicists that, during nearly a century the sun-spot cycle has been shortened or lengthened, and the sun-spot variations have been accelerated or retarded, so nearly together with those of the diurnal oscillations of the magnet, by accidental coincidences. No doubt the admission of the existence of a causal connection between the two phenomena is opposed to the hypothesis, which many other facts render now wholly untenable, that the magnetic variations are due to the heating action of the sun.

I am obliged to Prof. Piazzi Smyth for giving me the occasion to explain a difficulty which has troubled others as well as himself. JOHN ALLAN BROUN

January 23

HENRI VICTOR REGNAULT

THE death of M. Becquerel, alluded to in our last issue, was followed on the 19th inst. by that of his friend and fellow-physicist, M. Regnault, whose name is associated so intimately with the elementary principles of our knowledge of heat. Henri Victor Regnault was born at Aix-la-Chapelle, July 21, 1810. His youth was spent in a hard battle against poverty in the effort to maintain not only himself, but his sister. While still a lad he wandered to Paris, and there obtained a position as assistant in the large drapery establishment known as Le Grand Coude, a name familiar at the present day to the lady visitors of Paris. Here ability and fidelity won for him friends, and at the age of twenty he was enabled to gratify his longings for a scientific education, and enter the École Polytechnique of Paris, the Alma Mater of so many famous French *savants*. After a course of two years here, in 1832 he entered upon active duties in the department of mines, and was absent from Paris for the next eight years. During the latter portion of this time he occupied a professor's chair at Lyons, and had a laboratory at his disposal. Here he embraced the opportunity to enter upon the field of research in organic chemistry, which had just sprung into existence as a branch of chemical science, under the hands of Liebig, Wöhler, Laurent, Dumas, and others. While many of the chemists of the day were engaged in theoretical disputes, and the battle between the electro-chemical theory and the newly-advocated type-theory was being hotly waged, Regnault devoted himself to the accumulation of the facts so sorely needed as foundation-stones by the disputants on both sides. Among his investigations at this time may be mentioned those on the composition of meconine, piperine, cantharidine, and other alkaloids, composition of pectic acid, identity of esquisetic acid with maleic acid, properties of naphthaline-sulpho-acid, &c. By the action of sulphuric anhydride on ethylene, he obtained the carbyl-sulphate, $C_2H_4S_2O_6$, which Magnus prepared later from alcohol. His most valuable researches, however, were on the halogen derivatives in the ethyl-group, especially interesting at the time of their appearance, when the theories of substitution were timidly being advocated. Among these compounds now familiar reagents to the organic chemists were mono-chloro-ethylene-chloride, $CH_2Cl.CHCl_2$, obtained by the action of chlorine on ethylene chloride, as well as the higher chlorinated derivatives, which offered one of the most striking instances of substitution. These were followed shortly after (1838) by the classical investigations on the actions of chlorine on ethyl-chloride C_2H_5Cl, in which one by one all of the hydrogen atoms were successively substituted by chlorine, until the limit, C_2Cl_6 was reached. Of importance also was the change of ether, $C_4H_{10}O$, into perchloroether, $C_4Cl_{10}O$. Another interesting series of preparations gave the substituted ethylenes by the action of alkalies on saturated halogen derivatives, ethylene-bromide for example, yielding vinyl-bromide, and hydrobromic acid:—

$$C_2H_4Br_2 + HKO = C_2H_3Br + KBr + H_2O.$$

By this method he discovered vinyl-bromide, vinyl-iodide, vinyl-chloride, dichlor-ethylene, $C_2H_2Cl_2$, and trichlor-ethylene, C_2HCl_3. Finally must be mentioned his discovery of carbon-tetrachloride, CCl_4, by leading chlorine into boiling chloroform. It is difficult for us at the present day to estimate the importance attached to these discoveries forty years ago, when every new fact was a glimmer of light to the organic chemist wandering in the dark, and few series of researches have stood the test of time so well as those carried out by Regnault in his Lyons laboratory. The faithful study of minute properties, and the careful attention to physical peculiarities, already gave evidence of the tendencies which were manifested more fully in another branch of science, and the appearance of his papers in the *Annales de Chimie et Physique* attracted the attention of the scientific world to the hitherto unknown provincial professor. In 1840 he was elected to replace Robiquet in the chemical section of the French Academy, and was appointed professor in the Ecole Polytechnique. In the following year he was elected to the chair of physics at the Collège de France. A few years later he became engineer-in-chief of mines, and in 1850 received the order of officer in the Legion of Honour.

With his removal to Paris the field of Regnault's investigations was changed. Like our own Faraday, after having obtained renown as a chemist, he suddenly turned physicist. He was scarcely established in Paris, when he began his famous series of experiments on specific heat. A few years previous, Dulong and Petit had determined the specific heat of a number of elements by

means of their calorimeter based on the method of cooling, and obtained data sufficiently accurate to warrant the establishment of their law that the product of the specific heat of an element and its atomic weight is a constant. Regnault, after having submitted their method to careful examination, found it useless for the exact determination of the specific heat of solids, and invented in its place the calorimeter bearing his name. It is based on the method of mixtures, viz., of heating a known weight of a substance to a known temperature, immersing it in a known weight of water at a known temperature, and determining the temperature of the mixture. With this apparatus, which is of a somewhat complicated character, in order to reduce to a minimum the possibilities of error, Regnault determined the specific heat of the liquid and solid elements, and of a great variety of compounds. From the comparison of these results he deduced the general law that for all compounds of the same formula and similar chemical constitution the product of the specific heat and the atomic weight is the same. He also confirmed, by his experiments, the hypothesis of Wöstyn, that the elements require the same amount of heat to be raised to a certain temperature, whether free or in combination, and showed, by his more exact results, the general truth of Dulong and Petit's law. In order to overcome the difficulties of determining the specific heat of gases, Regnault contrived an ingenious apparatus in which the gases passed through a spiral inclosed in a known weight of water. The volume of gas, its temperature on entering and leaving the apparatus, and the alteration in the temperature of the water supplied the necessary data. By this means he experimented with about thirty-five of the principal gases and vapours, and established the two important laws, 1, that the specific heat of any gas at constant pressure, whether simple or compound, is the same at all pressures and temperatures; and 2, that the specific heats of different simple gases are in the inverse ratio of their relative densities. Regnault prepared also an interesting table of the specific heats of various substances in the solid, liquid, and gaseous forms, from which it appears that the specific heat of the same body is commonly greater in the liquid than in the solid state, and always greater than in the gaseous state.

In his experiments upon heat Regnault was led to devise methods of measuring high temperatures accurately, and invented the well-known air thermometer, which can be used at all temperatures below that at which gas softens, and the mercury and hydrogen pyrometers, the latter of which permits the determination of the temperature in a furnace at any instant. In this connection he carried out also an elaborate series of experiments on the density and absolute expansion of mercury from 1° to 360°, the results of which, as tabulated, are of primary importance in the correction of thermometers and barometers, as well as in a multitude of physical experiments conducted with this liquid. Still more elaborate and exhaustive are the extensive series of determinations in connection with water, its specific heat at various temperatures, the tension of its vapour at various temperatures, and the latent heat of its vapour at various pressures, all of which were designed to serve as fundamental facts upon which to base the action of heat on water for industrial purposes. The specific heat of water was found to increase from 1 at 0° to 1·013 at 100° and 1·056 at 230°. For the determination of the tension of steam Regnault contrived a simple apparatus based on the fact that the maximum tension of steam at the boiling-point is equal to the external pressure, by the aid of which he was able to construct his table of tensions from 0·32 mill. at 32° to 20926 mill. at 230°.

The experiments with this apparatus were extended to a number of volatile liquids with the design of testing the truth of Dalton's supposition that the tension of the vapours of all liquids is the same at temperatures equally distant from their boiling points, and the results showed that although not a law, it was very nearly correct for small intervals of temperature in the neighbourhood of the boiling point. A variety of interesting results were also obtained from mixtures of gases and vapours, including the laws that a liquid does not give off a vapour of so high a tension in the presence of a permanent gas as in a vacuum, and that while the tension of the vapours of a mixture of liquids not dissolving each other is equal to the sum of the tensions of its liquids at the same temperature; on the contrary, the tension arising from a mixture of mutually solvent liquids is less than the sum of the individual tensions.

Perhaps the most important of Regnault's experimental investigations was that on the coefficient of expansion for air and other gases, as well as on the compressibility of gases. Dalton, Gay-Lussac, and Rudberg had obtained numbers for the coefficient of expansions differing widely from one another. It was reserved for Regnault to establish by the most delicate experiments the number ·03663 as the coefficient of expansion of air, and to show in addition that the law of Dalton and Gay-Lussac with regard to the regularity of expansion among gases was only approximately correct. A similar result was obtained in his investigations on the accuracy of Boyle and Mariotte's Law, on the compressibility of gases.

In addition to the chief lines of research alluded to, Regnault made a variety of interesting experiments on the phenomena produced by heat, and his hypsometer and hygrometer should be mentioned, on account of their simple and practical qualities. Some valuable investigations on the phenomena of respiration were made by him in connection with Reiset, and, together with Dumas, he carried out a lengthy research on illuminating gas.

His most valuable experimental results are collected together in vol. xxi. of the *Mémoires* of the French Academy, and a continuation is to be found in vol. xxvi. Regnault published, in 1847, a treatise on chemistry, which has survived numerous editions in France, and been translated into German, English, Dutch, and Italian.

In 1854 he was appointed director of the famous porcelain manufactory of Sèvres, and since that date much of his time has been devoted to improvements in ceramic processes. During the Franco-Prussian war he received a sad blow in the death, on the battle-field, of his second son, Henri Regnault, a promising artist, and universal favourite in Paris. He returned to his laboratory at Sèvres, after the declaration of peace, to find that the results of his last great research on the phenomena of heat accompanying the expansion of gases, derived from over 600 observations, had been destroyed. The announcement of this loss was his last communication to the scientific world. Since then, oppressed by grief and a victim to increasing infirmities, he has been forced to renounce his wonted pursuits. On the day when the gay artist world of Paris was celebrating the battle of Buzenval by laying wreaths on the grave of the young patriot-painter, the father was released from a long and painful illness by the hand of death.

As a scientific investigator, Regnault did not possess the brilliant originality of many of his fellow-physicists. It is as the patient, thorough, conscientious observer that he has won his way to the foremost rank. Possessing a wonderful ingenuity in the invention of mechanical appliances for the purposes of observation and a perfect familiarity with the mathematical department of physics, he has been enabled by means of his unflagging enthusiasm and unbending resolution to place the modern physicist and chemist in possession of an invaluable collection of constants, which at the present stage of science are in daily use not only in the laboratory of research, but for a large variety of industrial purposes.

T. H. N.

THE ORIGIN OF A LIMESTONE ROCK[1]

IN November, 1845, I laid before the Literary and Philosophical Society of Manchester my memoir "On some Microscopic Objects found in the Mud of the Levant and other Deposits; with Remarks on the Mode of Formation of Calcareous and Infusorial Siliceous Rocks," which memoir was published in vol. viii. of the second series of the Society's *Transactions*. In that memoir I sought to demonstrate two things—1st, that not only was Chalk made up of microscopic organisms, chiefly Foraminifera, as had recently been demonstrated by Ehrenberg, but that the fact was equally true and explanatory of the origin of all limestones except a few freshwater Travertins; 2nd, that some other extensive deposits, of submarine origin, in which no Foraminifera could now be detected, were not in the state in which they were originally accumulated. I concluded that Foraminifera had doubtless been present in them also, but that their calcareous shells had been dissolved out of them, and that this disappearance had been effected through the agency of water containing carbonic acid, at an early stage of the formation of these deposits. As is well known, this latter theory has been reproduced as a new one by some of the naturalists of the *Challenger* expedition, who have applied it to the explanation of phenomena of a substantially similar nature to those which I endeavoured to account for, in the same way, more than thirty years previously.

I am indebted for the slab of limestone forming the subject of this communication to my friends the Messrs. Patteson, the marble merchants of Oxford Street, Manchester. This slab appears to illustrate in an exquisite manner both the theories to which I have just referred. It is a specimen of the Bolland limestone, which, when sawn through, was found to contain a large concamerated Nautiloid shell more than twelve inches in diameter, which appears to me to have been a true Nautilus, though the section has not passed exactly through its centre so as to reveal any portion of its siphuncle. In the various parts of this slab we find the calcareous material exhibiting different conditions. Throughout the greater part of its substance we have evidence that it has originated in an accumulation of minute calcareous organisms—especially Foraminifera—but most of these are disintegrated and display vague outlines, a condition which I presume has resulted from the action of the carbonic acid already alluded to.

Scattered through the slab are numerous dark-coloured patches of a substance apparently identical with what the late Dr. Mantell designated Molluskite, and which he believed to be the remains of the soft animal substance of marine organisms. In many of these patches the Foraminiferous shells are better preserved than is the case with the rest of the matrix inclosing the large fossil shell. It appears as if this Molluskite had partially protected the calcareous Foraminifera from the solvent action which had disintegrated most of those forming the rest of the deposit.

But the most interesting features of the specimen are seen within the chambers of the Nautiloid shell. The Foraminiferous ooze has entered freely through the large, open mouth of the terminal chamber in which the animal resided and filled the entire cavity of that chamber. There is no doubt whatever as to the original identity in the character of the ooze thus inclosed within the shell and that which constitutes its investing matrix, though they now appear very different. The latter portion was freely permeated by water containing the solvent carbonic acid; hence the more or less complete disintegration of its Foraminiferous shells. But in the limestone inclosed within the large terminal chamber of the Nautiloid shell almost every Foraminifer is preserved in the most exquisite perfection. This is especially the case in the deeper part of the chamber, most remote from the mouth, as also in the instances of one or two of the more internal closed chambers, into which the mud has obtained entrance through small accidental fractures in the outer shellwall. It appears obvious to me that the thick calcareous shell of the Nautilus has protected the inclosed shells of the Foraminifera from the action of the solvent acid. I repeat that there is no room whatever for doubting that both portions of the Foraminiferous ooze, whether contained within or surrounding the Nautiloid shell, were originally in identical states. Microscopic observation makes this sufficiently plain. The differences now observable between them have arisen from changes which have taken place subsequent to their primary accumulation, and which changes have been due to differences of position; the one portion has been protected by the thick calcareous Nautiloid shell which would rob the water percolating through it of all its solvent carbonic acid, and thus preserve the contained Protozoa from destruction, and which protection would continue so long as any portion of the Nautiloid shellwall remained undissolved. The other, being unprotected, would be exposed to the full action of the solvent, which would percolate readily amongst the loosely aggregated microscopic organisms, and speedily act upon their fragile shells.

But there is a yet further feature in this interesting specimen requiring notice. The *closed* chambers of the Nautiloid shell are all filled with clear, crystalline, calcareous spar. The acidulated water, acting upon the calcareous Foraminifera of the ooze has become converted into a more or less saturated solution of carbonate of lime. This has passed, by percolation, through the shell of the Nautilus into its hollow chambers. Finding there suitable cavities it has gradually filled them up with a crystalline formation of calcareous spar, and which of course exhibits no traces of the minute organisms from which the calcareous matter was primarily derived. A similar crystallisation has filled up the smaller interspaces between the Foraminiferous atoms both inclosed within, and external to, the Nautilus, rendering the limestone capable of receiving a high polish.

If these explanations are as correct as I believe them to be, we have here the entire history of the origin of a limestone rock—from the first accumulation of the Foraminiferous ooze, as seen in the interior of the first large chamber of the Nautilus, to the deposition, in an inorganic mineral form, of the crystallised carbonate of lime within the closed chambers of the Nautilus, all being illustrated within the area of a slab of limestone little more than a foot in diameter.

[1] "On the Microscopic Conditions of a Slab from the Mountain Limestone of Bolland," by W. C. Williamson, F.R.S., Professor of Natural History in Owens College. Read before the Literary and Philosophical Society of Manchester, January 8.

THE LIQUEFACTION OF THE GASES

IN the recent article, in which the magnificent results recently obtained by MM. Cailletet and Pictet were detailed, we contented ourselves, in the account of the methods employed, by pointing out the extreme simplicity of that used by M. Cailletet. The simplicity, however, by no means takes away from the beauty of the method, and we now propose to return to it with a view of showing how closely it resembles in many of its details that employed by Dr. Andrews in his classical work on the continuity of the various states of matter.

Dr. Andrews, it will be remembered, in his experiments on the liquefaction of carbonic acid, used a glass tube capillary in the upper part, and in the remainder, of a bore just so wide that a column of mercury would remain in it when the tube was held in a vertical position. The gas to be operated on was confined to the narrow upper part of the tube by mercury, and the tube was tightly packed to an end piece of brass armed with a flange.

This permitted a water-tight junction with a corresponding end of a cold-drawn tube of copper of great strength. A similar end-piece was attached to the other extremity of this

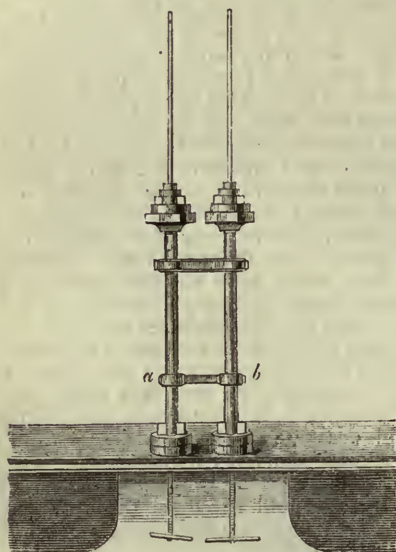

Fig. 1.—Two of Dr Andrews's tubes on a stand as in use

copper cylinder, and in the centre was a fine screw most carefully made and fitted, seven inches long, and packed so as to resist a pressure of 400 atmospheres or more.

Fig. 2.—Section of Tube. Fig. 3—Arrangements for Utilising Low Temperatures.

When low temperatures as well as high pressures were required, the tube was bent, as shown in Fig. 3, and inserted in a freezing mixture.

In all these tubes the pressure is produced by screwing up the mercury into the capillary tube.

We have next to consider the phenomena which Dr. Andrews observed, taking carbonic acid as an example.

On partially liquefying the gas by pressure and changing the temperature, the surface of demarcation between the liquid and the gas became less and less distinguishable, the tube seemed to be filled with a homogeneous fluid which, when the pressure was suddenly diminished, or the temperature slightly lowered, broke up into striæ, Fig. 4.

A cloud was also formed if the temperature were allowed to fall a little below the "critical point" 30°·92 C., showing the formation of liquid particles, Fig. 5.

We may now pass to M. Cailletet's method and the phenomena he observes, Fig. 6, for which we are indebted to the courtesy of the editor of *La Nature*, represents the great apparatus which M. Cailletet has constructed at his works of Châtillon-sur-Seine.

The apparatus is composed of a hollow steel cylinder A solidly fixed to a cast-iron frame by means of the hoops B B. A cylindrical shaft of soft steel acting the part of a plunger enters this cylinder, which is filled with water. The opposite extremity of the shaft is

Fig. 4.—Striæ.

Fig. 5.—Cloud.

terminated by a square-threaded screw, which traverses the bronze nut F, fixed in the centre of the fly-wheel M. According to the direction given to the fly-wheel by means of the handles with which it is provided, the plunger may be advanced into or withdrawn from the axis of the body of the pump. A leather packing prevents the compressed liquid from escaping from the cylinder.

In order to introduce the water or the liquid to be compressed, it is poured into the glass vessel G, which is in communication with the interior of the apparatus; a steel screw with conical point closes the narrow pipe through which the liquid passes. This screw is terminated by a small fly-wheel O, with handles. This arrangement permits of suddenly expanding the compressed gases, and seeing the cloud produced in the capillary tube where the gas under experiment is contained. (This tube is represented in the centre of the glass envelope, *m.*) The cloud is formed under the influence of the external cold produced by the sudden expansion, a certain sign of the liquefaction or even of the solidification of the gases regarded hitherto as permanent. *a* is a hollow steel reservoir

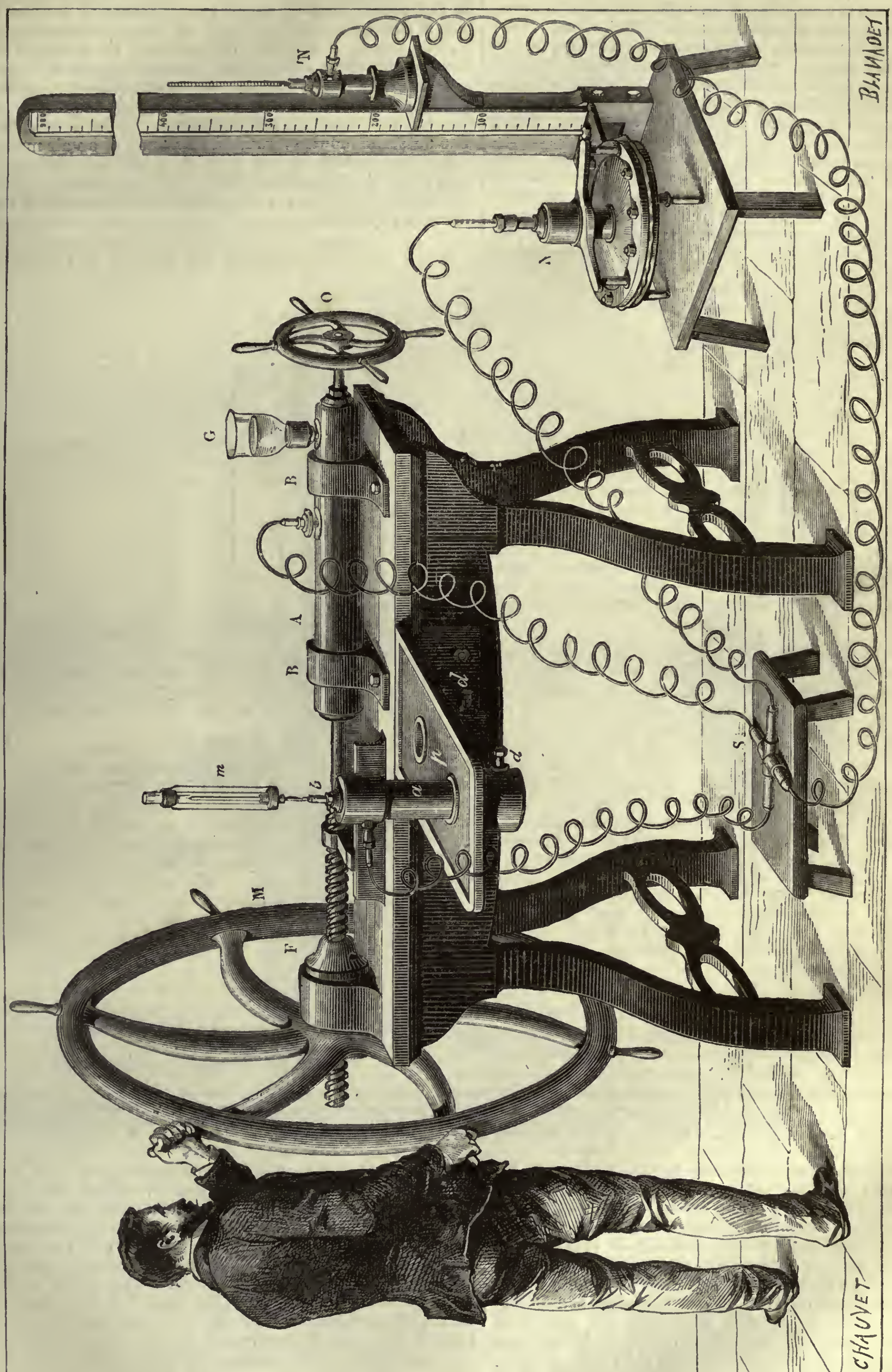

FIG. 6.—Great apparatus of M. Cailletet for the liquefaction of the gases A, screw-press for compression; *m*, glass cylinder, containing the glass tube in which the gas is liquefied.

capable of supporting a pressure of from 900 to 1,000 atmospheres ; it is connected with the compression apparatus by a capillary metallic tube. The water, under the action of the piston, arrives in this reservoir, *a*, and acts upon the mercury which compresses the gas. *b* represents the tube which connects this with the glass intended to contain the gas under experiment. A nut serves to fix this piece to the upper part of the reservoir. Fig. 7 shows this arrangement in half-size.

m is a glass cover containing a cylinder of the same material, in the middle of which is a small tube in which the liquefaction of the gas takes place. This capillary tube may be surrounded with refrigerating mixtures or with liquid protoxide of nitrogen. The exterior cover, *m*, concentric with the first, and containing substances strongly absorbent of moisture, prevents the deposit of ice or vapour on the cooled tube in which the experiments are made. *p* is a cast-iron tablet intended to support the reservoir, *a* ; the screws, *d d*, enable the reservoir to be raised or lowered for the spectroscopic examination or the projection of the experiments. An arrangement, S, unites the capillary metallic tubes which transmit the pressure to the various parts of the apparatus. N is a modified Thomasset manometer verified by means of an air manometer established on the side of a hill near the laboratory of Châtillon sur-Seine. N′ represents a glass manometer which serves to control the indications of the mercury apparatus.

It is a fortunate thing that the students of science in

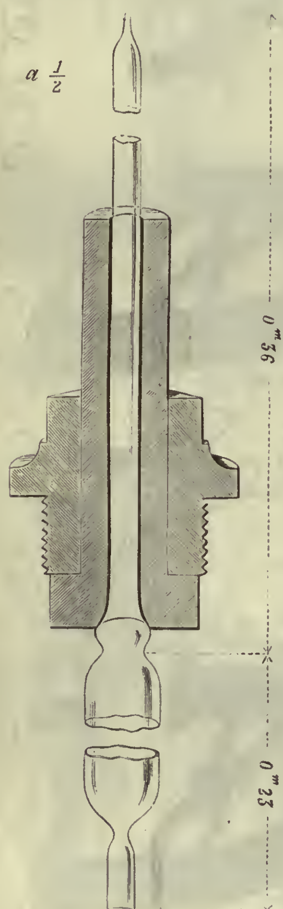

FIG. 7.

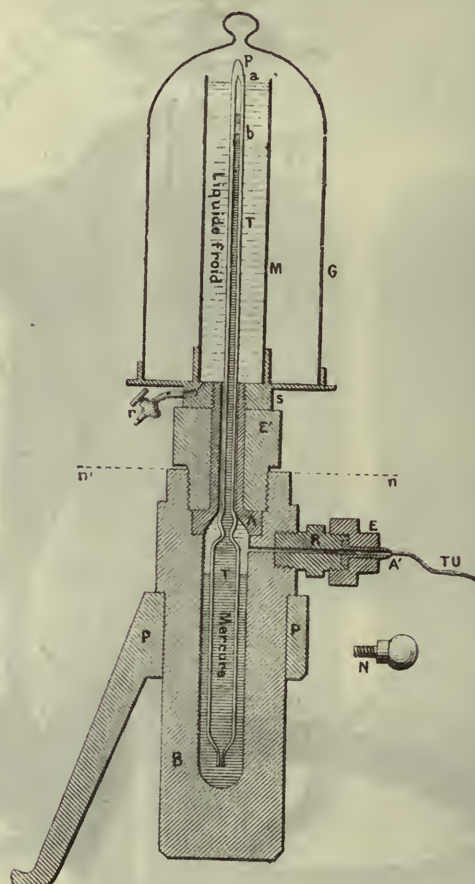

FIG. 8

FIG. 7.—Glass tube with thick sides in which the liquefaction of the gases is effected in M. Cailletet's apparatus. The gas is compressed in the upper part of the tube by the ascent of a column of mercury placed in connection with a screw-press acting on a mass of water. It condenses in a liquid drop or into mist under the action of expansion. This glass tube is enveloped in an envelope of the same substance containing the refrigerating mixture. See the centre of the tube *m* in Fig. 2. FIG. 8.—Small apparatus for the liquefaction of gases.

France have not been forgotten by M. Cailletet. He has not only devised the instrument above described for his own work, but he has occupied himself with a small lecture or laboratory apparatus which M. Ducretet has constructed according to his directions. It is an exact copy of the part, *a*, *b*, *m* of the apparatus of Châtillon-sur-Seine. The bell-glass alone is modified. The screw-press is, moreover, replaced by an easily-worked pump. In Fig. 8 T′ T is a glass tube filled with the gas to be compressed ; the tube has been traversed by the gas until air has been entirely excluded ; for this purpose it is placed in a horizontal position. When it is filled with the gas to be experimented on it is hermetically sealed at its extremity, *p*, closed with the finger at the other end, and introduced vertically into the iron apparatus as represented in the figure. It is inserted into a cylindrical cistern containing mercury. The upper part of the tube is enveloped in a glass envelope, M, filled with a refrigerating mixture. The whole is enveloped in a glass jar, G. The tube, T U is connected with a compressing pump, which is worked with the hand. The water compressed by the pump acts on the upper part of the mercury indicated in the figure by horizontal lines. This mercury is driven back into the

tube TT; it reduces the space *a b* occupied by the gas, and is soon surmounted by droplets of the compressed gas, which unite into a little mass of liquid, *b*.

The following are the parts of the apparatus:—B, a block of malleable iron with strongly-resisting walls; E′, E, screw nuts which may be unscrewed to arrange the apparatus before using it; P P, very solid tripod which receives the apparatus; S, support of the bell G and the envelope M; N supplementary screw intended to close the hole in the joint R when the mercury is poured into the apparatus.

OUR ASTRONOMICAL COLUMN

THE ROYAL OBSERVATORY, CAPE OF GOOD HOPE.—Since the appointment of Mr. Stone to the directorship of this establishment, in 1870, not only have all arrears of observations with the transit-circle, first brought into use in 1855, been reduced and published, but Mr. Stone has lately issued the results of observations taken in 1875, and has thereby overtaken the position of publications of the Royal Observatory, Greenwich, and the Radcliffe Observatory, Oxford, which have been conspicuous amongst astronomical establishments for the expedition with which the great mass of work involved in the reduction of the observations has been performed, and the results given to the scientific public.

The chief work of the year was the continuation of the general re-observation of the stars in the *Cœlum Australe Stelliferum* of Lacaille, attention in 1875 having been directed to those stars lying between 145° and 155° of north polar distance at the present epoch, all of which appear to have been observed, usually three times in both elements, together with a number of other stars in the same zone, which, though not generally much below the seventh magnitude, were not observed by Lacaille. Mr. Stone mentions that stars within limits of N.P.D 135°–145° were observed in 1876, and stars between 125°–135° in 1877.

Should it be deemed advisable shortly to form another general catalogue of stars, similar to the British Association Catalogue, say to stars of the seventh magnitude inclusive, Mr. Stone's recent volumes will be of the utmost value in extending the precision now attainable for such stars in the northern hemisphere to the southern heavens, not only as regards positions for the present epoch, but in the determination of proper motions of a considerable number of stars by comparison with Taylor's catalogues, which have not yet been systematically examined for that purpose. And we will take this opportunity of expressing the hope that if another catalogue like the B.A.C. should be undertaken, the time, labour, and expense involved in the preparation of so-called star-constants may be avoided, and attention paid instead to a more general and systematic investigation of proper motions, which, it can hardly be doubted, must lead to results of great interest and importance.

THE TOTAL SOLAR ECLIPSE OF JULY 29.—It was mentioned in NATURE last week that facilities would be afforded to intending observers of this phenomenon near Denver, Colorado, one of the chief places included in the belt of totality in the United States, and situated on the Pacific line of railway. By the elements of the *Nautical Almanac* the track of central eclipse appears to pass about twenty-five miles south of Denver, assuming its longitude from Greenwich to be 7h. 0m. 20s. W., and latitude 39° 48′, and at Denver the total phase commences at 3h. 28m. 14s. local mean time, and continues 2m. 45s., with the sun at an altitude of 42°; the circumstances by the elements of the American ephemeris are almost identical, as indeed was to be expected seeing that the moon's place in the latter work differs from her place in the *Nautical Almanac* by only + 3″·4 in R.A. and + 1″·0 in decl. and the sun's place by − 1″·1 in R.A. and + 0″·3 in decl., while the semi-diameters employed are each less by about 2″. In the American ephemeris the lunar tables of Peirce and the solar tables of Hansen are employed.

The northern and southern limits of totality in the eclipse of July 29, with the duration of total phase upon the central line, for nearly the whole track across the North American continent will be found at p. 400 of the *Nautical Almanac* for 1878.

CHEMICAL NOTES

TEMPERATURE OF FLAMES.—In the *Gazetta chimica Italiana* an account is given by F. Rosetti of some experiments on the above subject. To examine the temperatures he employs a thermo-electric element consisting of an iron and a platinum wire wound closely together and connected with a galvanometer. This latter was graduated to various temperatures by observing the deviation consequent on bringing the element in contact with a copper cylinder heated to known temperatures; these being determined by introducing the cylinder into a calorimeter. With such an arrangement he has investigated the flame of a Bunsen's burner, finding that in the same horizontal strata there were but slight alterations in the temperature, with the exception of the dark interior portion. Thus, where the external envelope showed 1,350°, the violet portion of the flame was 1,250°, the blue 1,200°, but the internal portion much lower, its temperature gradually decreasing from the base of the flame upwards. A flame produced by the combustion of a mixture of two volumes of illuminating gas and three volumes of carbonic oxide, showed a temperature of 1,000°.

STARCH IN PLANTS.—Botanists have hitherto held that all the starch in the chlorophyll cells of the leaves of plants is a product of the direct assimilation of carbon dioxide and water, basing this belief on the fact that the starch in these cells disappears when the plants are deprived of the power of assimilating carbon dioxide, but reappears on their exposure to light in an atmosphere containing that substance. Prof. Bohn, of Vienna, in a recent number of the *Deut. chem. Ber.*; throws some doubt on this conclusion by experiments he has made on the leaves of the scarlet runner. His results show that if the primordial leaves of this plant are shaded from light, the starch at first entirely disappears; after a few weeks, however, the chlorophyll cells of these shaded leaves show almost as high a percentage of starch as the parts of the plant which have been exposed to light. These observations demonstrate, therefore, that starch can be formed in the leaves from matter which has already been assimilated, and has entered into the leaf after its removal from the sunlight.

SIPYLITE, A NEW MINERAL CONTAINING NIOBIUM.—Mr. Mallett has found this mineral among some quantities of allanite from Amhurst county, Virginia. A few crystals have been obtained, but as they are of rather imperfect nature the measurement of the angles has only been attempted in a rough manner. The mineral in the mass was of a brownish black nature, but in thin plates it exhibited a reddish-brown colour, and possesses a pseudo-metallic lustre. The hardness is estimated at about 6, and the specific gravity as equal to 4·89. From the results of analyses Mr. Mallet considers that placing together the acid oxides of niobium, tantalum, tungsten, tin, and zirconium, reducing the basic oxides to equivalent amounts of dyad oxides, and eliminating the water, the following ratio may be obtained:—$R''O : M^{v}{}_2O_5 = 221 : 100$, leading to the formula $R''_3M^{v}{}_2O_8 \cdot 4R''_2M^{v}{}_2O_7$, that is a single group of orthoniobate associated with four of pyroniobate. If the water be taken into account in the calculation and considered basic, then placing it on the same footing as the dyad oxides, we should have the

relation $R''O : M^v{}_2O_5 = 311 : 100$, or nearly $3:1$, thus giving the simple formula $R''_3M^v{}_2O_8$; this latter the author considers the more probable. Whatever formula, however, may be taken for the mineral it differs from niobates hitherto described, the one view making it an approach to a simple pyroniobate, the other making it an orthosalt like Fergusonite, but partially acid in character, or containing basic hydrogen.

MOLYBDENUM.—The atomic weight of this metal has hitherto been quite uncertain, some chemists regarding it as 96, others as 92. Fresenius, the leading authority in analytical chemistry, has always adopted the latter number. Prof. Rammelsberg, of Berlin, has lately settled the question by careful experiments on the reduction of molybdic acid in an atmosphere of hydrogen, and has found 96 to be the correct atomic weight, – 96·18 being the exact number obtained. Taking this number as a basis, he has sought to solve the problem of the composition of the yellow phospho-molybdate of ammonium, which is used generally for the determination of phosphoric acid, and the exact formula of which has never been satisfactorily determined. A large number of analyses of the ammonium salt and the corresponding potassium salt show that the composition is undoubtedly

$$3(NH_4)_2O + P_2O_5 + 22MoO_3 + 12H_2O.$$

RELATIONS BETWEEN THE VOLUMES OF SILVER SALTS.—H. Schröder communicates an interesting series of observations on this subject in the *Berichte der deutschen chemischen Gesellschaft*, for November, from which it appears that the atomic volumes (*i.e.*, the quotient resulting from the division of the molecular weight by the specific gravity) of these salts are all simple multiples of the atomic volume of silver, or rather of its half atomic volume, 5·14. In the fatty series an accession of CH_2 to a compound increases the atomic volume by $3 \times 5{\cdot}14$. For example :—

$$C_2H_3AgO_2 = 10 \times 5{\cdot}14 = 51{\cdot}4$$
$$C_3H_5AgO_2 = 13 \times 5{\cdot}14 = 66{\cdot}8$$
$$C_4H_7AgO_2 = 16 \times 5{\cdot}14 = 82{\cdot}2$$
$$C_5H_9AgO_2 = 19 \times 5{\cdot}14 = 97{\cdot}7, \text{ \&c.}$$
$$C_6H_5CAgO_2 = 20 \times 5{\cdot}14 = 102{\cdot}8$$
$$C_4H_4Ag_2O_4 = 17 \times 5{\cdot}14 = 87{\cdot}4.$$

ORNITHURIC ACID.—Prof. Jaffe, of Königsberg, in the course of experiments on the transformation of organic bodies on passing through the digestive organs of fowls, has obtained a new acid in a way decidedly different from the usual methods of chemical synthesis. Benzoic acid, C_6H_5COOH, which has been given to birds, is found to be entirely changed by passing through their organisms into a new and well-defined acid, which crystallises in colourless needles, forms a series of salts, and receives the name ornithuric acid. It appears to arise from the combination of benzoic acid with a base $C_5H_{12}N_2O_2$, present in the system, and which can be separated from ornithuric acid by treatment with hydrochloric acid. The formation is as follows :—

$$2C_6H_5COOH + C_5H_{12}N_2O_2 = C_{19}H_{20}N_2O_4 + 2H_2O.$$

DISTILLATION OF ORGANIC LIQUIDS BY MEANS OF STEAM.—Prof. Naumann, of Giessen, describes, in a recent series of papers in the *Berichte der deut. chem. Gesellschaft*, the results of his observations on the phenomena attendant on the passage of steam through organic liquids. As is well known to the experimental chemist, aqueous vapours, on passing through a liquid, carry with them frequently large portions of the latter, even when it boils at a temperature far above that of water—aniline, for example, at 180°. The process also is one of every-day occurrence in the organic laboratory, being used for the purpose of separating such liquids from their impurities. Prof. Naumann has studied in this connection liquids both specifically lighter and heavier than water, as well as liquids boiling below and boiling above 100° C., recording the physical phenomena produced by the passage through each of a regular current of steam. In all cases he finds them obeying a few invariable laws, viz., 1°. For every mixture of a liquid with water there is a constant boiling-point, which is below that of the lower boiling liquid ·2°. A constant ratio exists between the respective quantities of the two liquids found in the distillate ·3°. The temperature of the distilling vapours is always slightly higher than that of the mass of liquid. From among the numerous results the following will convey a general idea of the experiments. The first column contains the boiling points of the respective liquids, the second the temperature of the liquid while steam is being passed through it, and the last the number of cubic centimetres of the liquid found in the distillate for every 100 c.c. of water :—

Benzene	79°·5 ...	68°·5 ...	8·5
Toluene	108·5 ...	82·4 ...	21·2
Xylene	135·5 ...	89 ...	44
Nitrobenzene ...	205 ...	98·5 ...	14

An attempt was made to discover a connection between the molecular weights of the three first hydrocarbons of the aromatic series and the respective quantities of these liquids in the distillates, but without success. While studying the relations of the numbers yielded by the experiments, Prof. Naumann finally discovered that all the liquids obeyed a general fixed law, viz., *when a liquid is distilled by means of steam, the ratio between the volumes of the liquids and the water in the distillate, expressed in multiples of their molecular weights, is equal to the ratio between their vapour-tensions at the temperature at which the distillation occurs.* It is at once evident that by the discovery of this law the chemist is placed in command of a most valuable auxiliary for determining the constitution of a variety of compounds at present to a certain extent doubtful. The law holds equally good for any liquid the vapour of which is used instead of that of water.

GEOGRAPHICAL NOTES

EARLY AFRICAN EXPLORER.—Don Marcos Ximenez de la Espada of Madrid is now having printed a document of extraordinary interest for geographical science, viz., an account of the travels of an unknown missionary, of the fourteenth century, which Don Marcos has recently discovered. The enterprising author, in the years from 1320 to 1330, undertook extensive travels in Africa, not only along the west coast to Sierra Leone and thence to Dahomey, but also, it is stated, from the mouth of the Senegal river straight across the interior of the great continent. He visited the Soudan States, got as far as Dongola, and thence proceeded down the River Nile, finally reaching Damietta.

AFRICAN EXPLORATION.—In reply to a question from Mr. H. Samuelson last Friday in the House of Commons, the Chancellor of the Exchequer stated that it was not the intention of Government at present to devote any public money to African exploration. We can hardly expect that they would in the present state of public affairs; and even if they could it would be difficult to see in what direction they could take action. There are many expeditions of various kinds in the African field at present, working away with little or no connection with each other; even the International African Association has not been able to organise them, but is simply sending out more expeditions. There seems to us to be considerable waste of power and resources here.

MR. STANLEY.—The Geographical Society's dinner to Mr. Stanley is to take place on February 9. Arrangements are being made to accommodate the Fellows and

their friends in St. James's Hall, but as the hall holds only 2,000, and as there are between 3,000 and 4,000 Fellows, we suspect, making all allowances, that many hundreds will be disappointed. Why does the Society not boldly take the Albert Hall and admit the outside public at a moderate charge? We are sure there would be a balance over after clearing expenses.

BERLIN GEOGRAPHICAL SOCIETY.—The Berlin Gesellschaft für Erdkunde celebrates on April 27 and 28 the completion of its fiftieth year. The festival committee, consisting of Baron v. Richthofen, Dr. Nachtigal, Dr. Jagor, and other well-known explorers, have issued invitations to all the geographical societies of Germany and Austria to send delegates. It is expected that over a thousand will be present at the closing dinner. This society, founded by Alexander von Humboldt and Karl Ritter, has manifested from its commencement a vitality and energy second to none of the European geographical societies, and forms in Berlin a favourite gathering-place for the leading minds in all departments of science. Its membership numbers at present 700.

AUSTRALIA.—An exploring party sent into the interior from Port Darwin, North Australia, under the leadership of Mr. Sergison, has returned to the latter place, and reports that in the vicinity of Victoria River, which runs into the Queen's Channel on the west coast of the Northern Territory, as well as near Fitzmaurice River, which flows more to the north, and near Daly River, which runs into Anson Bay, it discovered land with excellent soil, with a comparatively cool climate, and with numerous creeks in every direction.

ARCTIC EXPLORATION.—The preparations for the Dutch North Polar Expedition are being actively continued, as the expedition is to sail in May next. The first and principal halt will be made at Spitzbergen. The erection and fitting up of a station for meteorological observations is reserved for a future expedition; the present one, however, is to select the place best adapted for a station of this nature. 10,000 florins are still wanting to cover the expenses of the expedition.

CANADA.—On December 22 last a Canadian Geographical Society was founded at Quebec. The principal aim of the society will be to obtain a thorough knowledge of the geography of Canada.

NOTES

THE distribution of the prizes for 1877 by the French Academy of Sciences took place on January 28 under the presidency of M. Peligot. For the two great prizes in mathematics and in physical science no memoir worthy of mention has been sent to the academy. The subjects were very limited in their scope and it is said that the academy proposes to alter its system and confine itself to giving its highest prizes to independent workers irrespective of the subject-matter of their work. Among other prizes awarded we at present mention the following:—The Plumet prize was taken by M. de Freminville, for his improvements in marine steam engines; the Fourneyran prize was awarded to M. Malet, for tramway steam-engines, as used from Bayonne to Biarritz; the Lalande prize in astronomy has been rightly awarded to Prof. Asaph Hall, the discoverer of the satellites of Mars; the Valz prize to the Brothers Henry, for their celestial maps; the Montyon prize in physiology was awarded conjointly to Prof. Ferrier and MM. Carville and Duret; the Lacaize prize for the best work in physics has been given to M. Cornu, the well-known professor of the Polytechnic School for his determination of the velocity of light by direct measurement; the Breant prize (4,000*l.*) to the discoverer of a cure for cholera has not, of course, been awarded, but the interest of that sum has been given to M. Rendu, for several memoirs of etiology. A copy of Laplace's works, magnificently bound, has been delivered, as usual, in the name of Laplace's deceased wife, to the pupil of the Polytechnic School who has passed the most successful examination. The young laureate for 1877 is M. Dougadot, a native of Carcassonne (Aude), where he was born in 1855.

ON January 11 the centennial of Linné's death was observed in nearly all the cities of Sweden. In Stockholm the Academy of Sciences held a special session, attended by King Oscar, at which Prof. Malmsten delivered an interesting oration on the scientific achievements of the great botanist. At Upsala the occasion summoned together a number of notabilities who listened to an address from the Swedish botanist, Prof. Th. Fries. The university of Lund celebrated the day in a similar manner, the rector issuing, in connection with it, a short sketch of Linné's residence there, and Prof. Ogardh delivering the oration. At Frankfort-on-the-Main the memorable day was celebrated by a solemn meeting of the "Freie deutsche Hochstift," in the Goethe House. The president, Prof. Volger, in a brilliant speech, gave an outline of the life, the mental development, the activity, and importance of Linné, and closed by praising the mental ties which unite all races and nations. The meeting unanimously resolved to send a congratulatory telegram to King Oscar II., of Sweden, which was then sent off, written in the Latin language. An hour later his Majesty telegraphed his thanks in the same language. At Amsterdam, where the great Swedish botanist passed the early part of his life, there was also a Linné celebration on Jan. 10. At the same time an exhibition of objects relating to him, such as manuscripts, medals, portraits, &c., was arranged. Prof. Oudemans delivered the memorial speech.

THE French Scientific Association has issued the programme of its weekly lectures for the next three months, and provides a most promising list of famous names and attractive subjects. Among them we notice Prof. Dumas, "Eulogy on Leverrier;" M. Wolf, "Variability of the Nebulæ," which were given on January 26; M. Cornu, "The Phylloxera," February 2; M. Jamin, "Electric Illumination," February 9; Prof. St. Claire Deville, "Liquefaction of Gases," February 23; Prof. Bert, "Influence of Light on Life," March 9; Prof. Mascart, "Atmospheric Electricity," March 23; M. Tissandier, "The Upper Regions of the Air," March 30; M. Blanchard, "Geographical Distribution of Animals," April 13. The lectures take place at the Sorbonne, and as admission is easily obtained by strangers, they offer visitors to Paris an admirable opportunity of hearing the leading French *savants*. The first meeting, on January 26, was attended by more than 1,000 people, under the presidency of M. Dumas. The proceedings were opened by a report read by M. Milne-Edwards, the president of the Association, reviewing the work done by the Association, which was created by M. Leverrier more than fifteen years ago. It is owing to the assistance lent by the Association that weather-warnings have been so largely popularised in rural France and the agricultural service established by the physicist of the observatory.

JAPAN has an active archæological society, bearing the title of Kobutzu-Kai (Society of Old Things). Its members, numbering 200, are scattered throughout the land, but meet once a month in Yeddo. They consist chiefly of wealthy Japanese gentlemen, learned men, and priests; the latter especially have been the means of bringing before public attention a vast number of ancient objects which have been hidden in the treasures of the temples, or preserved in private families. H. von Siebold, Attaché of the Austrian Embassy, at Yeddo, and a member of the society, has lately published a *brochure*, which will serve as a guide for the systematic archæological study of the land; von Siebold has lately made a most interesting dis-

covery of a prehistoric mound at Omuri, near Yeddo, containing over 5,000 different articles in stone, bronze, &c. In a recent communication to the Berlin Anthropologische Gesellschaft, he describes the origin of the terra-cotta images found in old Japanese burial grounds. It appears that up to the year 2 B.C. it was the custom to surround the grave of a dead emperor or empress with a number of their attendants, buried alive up to the neck, their heads forming a ghastly ring about the burial spot. At the date referred to the custom was abolished, and the living offerings were replaced by the clay figures, which have hitherto attracted so much attention.

THE new ethnographical museum in the Palais de l'Industrie, at Paris, was opened on Wednesday last week, the Minister of Public Instruction pronouncing the opening discourse. Deputations were present from all the learned bodies and public institutions of the city, and general satisfaction was expresssed at the admirable manner in which Baron de Watteville, the director, had accomplished his task of organisation and arrangement.

THE Bolton Corporation have just adopted plans for the Chadwick Museum to be erected in the Bolton public park at a cost of 5,000*l*. The amount was left by the will of Dr. Chadwick for this purpose upon condition that the Corporation provided a site. The architect is Mr. R. K. Freeman.

PROF. W. M. GABB writes as follows from Puerto Plata, Sto. Domingo, December 29:—In the issue of November 1 you quote a Paris correspondent of the *Times*, who says that the Madrid people deny the authenticity of the recent finding of the remains of Columbus safe in the Cathedral of Santo Domingo. Of course the Spaniards are not willing to acknowledge that they were hoaxed, but the fact is nevertheless beyond dispute. The remains of Christopher Columbus are to-day in Santo Domingo. Unfortunately I am not able now to send you the full data. Suffice it to say that the chain of evidence is complete and has been verified with all possible precaution. The cheat was perpetrated by a then member of the "Cabildo," who had the knowledge, the tact, and the unscrupulousness to perpetrate it successfully. The whole consular corps, all the Government officials, and all the better class alike of natives and foreigners at the time in Santo Domingo city are witnesses of the authenticity of the "find."

ON Monday afternoon a powerful shock of earthquake was felt in the island of Jersey. It was so strong as to cause houses to totter and bells to ring. Its course was from east to west. There was at the time a heavy gale from the south-west in the English Channel. At 11·55 A.M. the same day a shock, lasting about four seconds, was felt at Eastern Alderney. No doubt it was the same earthquake which was felt at Brighton, Blackheath, Fareham, and St. Leonards, as reported in yesterday's *Times*, and at Paris, Havre, and Rouen, as stated by the *Times* Paris correspondent. Mr. Dobson, writing to us from the Royal Victoria Hospital, Netley, Southampton, states that the first shock occurred there at seven minutes to twelve o'clock exactly, and lasted about five or six seconds. It was sufficiently strong to cause the door to shake with some violence, and many objects in the room continued to vibrate for a considerable time. The second shock occurred a few seconds afterwards, but lasted for a much shorter period. A shock was felt at Lisbon on Saturday, being the third shock during the present winter.

A CIRCULAR signed by Mr. Justin Winsor, librarian of Harvard College, Cambridge, Mass., informs us that it is proposed to issue by subscription a catalogue of scientific serial publications in all languages, which has been prepared by Mr. Samuel H. Scudder, librarian of the American Academy of Arts and Sciences, and formerly librarian of the Boston Society of Natural History, and well known for his various scientific publications. This work, which has double the extent of any existing list of the like kind, aims to include all society transactions and independent journals in every branch of natural, mathematical, and physical science, excepting only the applied sciences—medicine, agriculture, technology, &c. The different institutions or periodicals are arranged under the towns in which they are established or published, and the towns follow an alphabetical order under their respective countries. Cross references are given wherever desirable. The work will be printed in large octavo, will extend to almost 300 pages, and will be delivered, bound in cloth, to subscribers at four dollars the copy. Other copies will be printed on one side of the leaf—to be cut up for catalogue use—and will be delivered in folded sheets at five dollars the copy. Further details may be obtained from Mr. Winsor.

A SECOND edition of Dr. M. Foster's "Text-Book of Physiology," has been published by Messrs. Macmillan and Co. The work has been revised and enlarged, and a number of figures of instruments has been introduced.

IN a recent paper to the Göttingen Society of Sciences, M. Grinitz has compared what data he could obtain regarding the effects of the earthquake at Iquique on May 9 last year. Among other points, it appears that the wave travelled from Iquique to Hilo, in Hawaii, a distance of 5,526 nautical miles, in fourteen hours; which is at the rate of 670 feet per second. From this velocity the average depth of that portion of the ocean traversed can be calculated by Airy's or Russell's formulæ; it is found to be 2,324 fathoms. The wave had an unbroken course to Hilo, but not so to Honolulu, as it encountered the islands of Hawaii, Maui, &c. The average velocity to Honolulu was 654·5 feet per second; and the average sea-depth inferred is 2,219 fathoms. The corresponding numerical data for Apia, Lyttelton, Uskaroa, in New Zealand, Komaishi, in Japan, and other places, are given. (For the last-named a velocity of 679 feet per second was obtained.) On comparison with Hochstetter's results for the earthquake of 1868, and with direct sea-measurements there is seen to be a very fair agreement. Hochstetter's assertion is, on the whole, confirmed, that the velocity of the earthquake wave and the lunar tide wave are identical.

WE have received from Messrs. Hardwicke and Bogue the first volume of their illustrated publication, *Industrial Art*, a monthly review of technical and scientific education at home and abroad. We have carefully examined the work and can say that the text and illustrations run each other very hard for carrying off the palm of excellence. We are glad to gather from the evident success of the venture that the time has arrived when scientific matter is regarded as the natural and necessary accompaniment of a complete reference to art matters. The articles on technical education in France, Austria, and Germany are thoroughly well done.

WE are glad to be able to point to another instance of a collection of the papers of a scientific man during his lifetime. Following hard upon the appearance of Dr. Frankland's collected papers Dr. Lloyd, of Dublin, has published a volume of 500 pages (Longmans) containing his memoirs, reports, and addresses given from time to time, from his classical paper on Conical Refraction to his address delivered before the British Association in 1857. The volume is a very valuable one for a scientific library, for at different times Dr. Lloyd has directed his attention to optics, terrestrial magnetism, and meteorology, and not only have we here the original papers but a series of reports on the progress and present state of physical optics extending over nearly 150 pages somewhat after the style of Verdet's introductions to the various parts of his work.

THE first general meeting of the Institute of Chemistry of Great Britain and Ireland will be held at the rooms of the Chemical Society, Burlington House, Piccadilly, to-morrow, at 4 P.M., to receive the report of the Council. A balance-sheet will also be presented by the treasurer.

WE have received one more evidence of the revival of activity in Italy, in the shape of the first number of a new weekly journal, *La Rassegna Settimanale di Politica, Scienze, Lettere ed Arte*, in which a fair amount of space is devoted to science. It is published at Florence.

THE following is a simple method recommended by Dr. Günther of Berlin, of observing the reversal of the coloured lines of flame-spectra. A thin platinum wire about five ctm. long, is fixed with one end in a glass tube (as holder), and one or two ctm. from the glass it is bent round to a right angle, and inserted in the envelope of a Bunsen flame, so that the free end, held vertical, is heated to a white glow. Into the diametrically opposite part of the flame-sheath is brought a sodium salt. This colours the flame. You then look through a weakly-dispersing prism (the combinations used for direct vision spectroscopes serve best), and through the sodium flame, towards the glowing wire. Two things are observed, (1) the spectrum of the monochromatic sodium flame, which appears in the form of the flame; (2) the spectrum of the glowing wire, which appears as a coloured band, but is broken by the dark D-line. Other metallic spectra may also be shown in this way; only care must be taken that the coloration of the flame be very intense.

AT Hanover the skeleton of a mammoth has just been found, through some excavations which are being made for waterworks near the Ricklinger Beeke. At present only the skull and a tusk have been brought to light, the latter having the circumference of a human leg. The fossils are lying at a depth of six metres.

THE publishing firm of Edouard Rouveyre, in Paris, announces the publication of a voluminous catalogue containing the titles, &c., of all those works, books, pamphlets, &c., which, in the period from October 21, 1814, down to July 31, 1877, have been prosecuted, suppressed, or confiscated, in France. It will appear in five parts, at two francs each.

AT the beginning of the year the new Royal Library of Stockholm, which has now been transferred to the new building at the Humlegaarden, was opened to the public. The new edifice was erected after the design of the architect, Herr G. Dahl, at a cost of 900,000 Swedish crowns. The library, which at the beginning of the present century only numbered 30,000 volumes, now contains 200,000.

A NEW monthly periodical, exclusively devoted to the art of photography and its various branches, is being published since January 1, by Messrs. Ad. Braun and Co., of Dornach. Each number contains an artistic photograph. The title of the new serial is *Die Lichtbildkunst*.

UNUSUALLY severe avalanches are reported this winter from Styria. In the neighbourhood of Hieflau one descended upon a railway train, crushing the carriages, and wounding a number, while at Neuberg another fell upon a châlet containing twelve persons, none of whom escaped.

IN the closing session of the German Chemical Society for 1877, Prof. Kekulé, of Bonn, was elected president, Professors Hofmann and Liebermann of Berlin, Prof. Fehling of Stuttgart, and Prof. Erlenmeyer of Munich vice-presidents. The Society elected also as honorary members the two physicists, Prof. Buff of Giessen, and Prof. Kirchhoff of Berlin, and Dr. Stenhouse of London. At the end of its first decade the German Chemical Society looks back upon a period of rapid growth in numbers and efficiency certainly unparalleled in the history of any society devoted to a special science. These results are due to several marked causes, which could well be imitated by other associations possessing analogous aims, viz., ease of admission, absence of entrance fee and smallness of the annual subscription, simplicity of the statutes, and rapidity and frequency in the publication of the proceedings. The number of members at present is 1,827, showing an increase of 229 during the year. Of these 206 reside in Berlin and 542 outside of Germany and Austria. The membership compares favourably with that of the older sister societies in London (952), and Paris (371). Although the annual payment is so small (15 marks) the society possesses a capital at present of 22,700 marks. During the past ten years the *Berichte* of the society have contained 3,726 communications, covering nearly 14,000 pages. A very complete index to this enormous amount of material will appear during the course of the present year, the compiler of which was selected by competition from among the twenty-nine applicants attracted by the unusually liberal appropriation of 5,000 marks for the work. In addition to the extensive chemical correspondence from America, England, France, Italy, Russia, Sweden, Switzerland, &c., the value of the *Berichte* in the future is to be increased by a complete series of abstracts on all papers appearing in German chemical periodicals. In the last number we notice a very full and interesting sketch of the late Prof. Oppenheim from the pen of Prof. Hofmann, as well as a detailed account of the Chemical Section at the German Association meeting at Munich by Prof. Liebermann.

THE French Academy of Sciences numbers at present 63, three places being vacant by the deaths of Regnault, Becquerel, and Leverrier, the members being divided into eleven sections of six each. There are in addition ten French free academicians and eight foreign associates. The corresponding members, of whom there can be 100, are divided according to their nationality as follows:—France, 32; Germany, 19; Great Britain, 16; Russia, 6; Italy, 2; Austria, 1; Denmark and Sweden, 4; Switzerland, 4; Belgium, 2; United States, 3; Brazil, 1; and there are 11 vacancies.

THE additions to the Zoological Society's Gardens during the past week include a Common Fox (*Canis vulpes*), European, presented by Mr. George Fredericks; two Black Swans (*Cygnus atratus*) from Australia, presented by Capt. W. H. Eccles; a Wood Owl (*Syrnium aluco*), European, presented by Mr. J. E. Liardet; a Common Magpie (*Pica caudata*), a Jackdaw (*Corvus monedula*), European, presented by Mr. G. E. Ladbury; a Hoary Snake (*Coronella cana*) from South Africa, presented by the Rev. G. H. R. Fiske, C.M.Z.S; a Jackass Penguin (*Spheniscus magellanicus*), an Upland Goose (*Bernicla magellanica*) from Chili, two West Indian Rails (*Aramides cayennensis*) from South America, purchased; a Derbian Opossum (*Didelphys derbianus*) from South America, deposited; a Hog Deer (*Cervus porcinus*) born in the Gardens.

RAINFALL IN INDIA

WE have received so many long letters from India on the various aspects of the rainfall question that we must either, from want of space, leave them unpublished, or briefly give the gist of them. We adopt the latter course.

Mr. Archibald sends us a long letter on the seasonal rainfalls of Northern India in connection with the sun-spot period, in which he communicates a few of the principal results obtained from a more detailed and extensive comparison, which the paucity of data at his command hitherto had rendered it impossible to undertake. In the present investigation the registers of eight stations, four in Bengal, and four in the N.W.P. have been employed, and the two seasonal falls of each year, compared (1) for each station separately, and (2) for groups of four and all together, with its position in the sun-spot cycle.

The stations and the periods over which their registers of summer and winter rainfall respectively extend are as follows:—

		Summer rainfall. Years.	Winter rainfall. Years.
Bengal	Calcutta	43	44
	Dacca	24	25
	Hazaribagh	15	15
	Patna	18	19
N.W.P.	Dehra Dun	16	18
	Roorkee	16	18
	Meerut	15	17
	Benares	15	17

"When the deviations from the local average seasonal falls in each year are calculated for each of the above stations separately, and the average taken for each year of the sun-spot cycle, it is found, notwithstanding individual irregularities which occur chiefly in the summer falls, (1) that the winter rainfalls uniformly exhibit a marked tendency to vary *inversely* with the sun-spots at all the stations, (2) that the summer rainfalls show a corresponding tendency to vary *directly* with the sun-spots, which, though strongly marked at the stations in the N.W.P., is scarcely perceptible at the Bengal stations. The result is best seen by combining several of the stations together, and since, owing to the large differences between the actual amount of rainfall at different stations, it is impossible to combine the deviations from the local averages, registered in inches, I have arranged the latter in the form of percentages of their respective averages, then multiplied each percentage deviation in each year of the sun-spot cycle by the number of years corresponding to it at each station, added the several products for the same year, and divided by the sum of the multipliers. By this means each station contributes to the final result in proportion to the extent of its register." Mr. Archibald then gives the tabulated results of combining according to this method (1) the four Bengal stations, (2) the four N.W.P. stations, and (3) all together.

From these tables it is seen that with very few exceptions the inverse relation between the two seasonal falls is strongly manifested throughout, the winter rainfall generally tending to rise above the average in proportion as the summer rainfall tends to fall below the same, and *vice versâ*. The winter rainfall moreover in every case tends to rise to a single maximum exactly coinciding with the period of minimum sun-spot, descending thence to a single minimum which occurs a year or two after the period of sun-spot maximum. The summer rainfall on the other hand exhibits two maxima and minima, and though varying more or less directly with the spots, this variation is principally confined to the N.W.P. stations.

The preceding peculiarities may be rendered still more apparent if we take as a new mean for each year of the cycle the mean of the mean percentage of the year itself together with half that of the preceding and succeeding years.

On the whole it is evident (1) that the winter rainfall throughout Northern India as well as at Calcutta is subject to a periodic variation amounting to nearly 50 per cent. of the average winter fall and corresponding approximately with the inverse phases of sun-spot frequency; (2) that the variation in the summer rainfall, though relatively much smaller, is of an almost exactly opposite character, and that while well-marked in the N.W.P., it is scarcely appreciable in Bengal; (3) that from the last table the cycle may be divided into two distinct portions, viz., the five years preceding, and the six years succeeding, the year of sun-spot maximum. In the former the winter rainfall is excessive and the summer rainfall defective, while in the latter the inverse relations hold, a fact somewhat analogous to the periodic change in the direction of the wind at Oxford and Prague as recently determined by Messrs. Main and Hornstein.

It is scarcely possible at present to indicate the practical deductions that may arise from a consideration of the preceding data. One inference, however, would appear to be immediately deducible, viz., that in any future comparison of the rainfalls of Northern India and other countries similarly subject to distinct seasonal rainfalls, due to distinct aërial currents—such as the monsoon and the anti-trade winds—with the sun-spot period, the summer and winter falls should be compared separately, otherwise it may be found that the combined effect of two opposite seasonal variations renders the resultant variation in the total annual fall very insignificant, or perhaps obliterates it altogether.

With reference to Mr. Hill's letter in NATURE, vol. xvi. p. 505, Mr. Blanford writes that he learns that Mr. Hill was not aware of the existence of his (Mr. Blanford's) paper in the forty-fifth volume of the *Journal* of the Asiatic Society of Bengal (1875), "hence, perhaps, what I cannot but regard as his under-estimate of the extent and validity of the evidence opposed to his view. He discusses the registers of three stations, one in the North-west Himalaya, and two on the dry plains of the Upper Gangetic Valley. My conclusions were based on the data of eleven stations altogether, viz., one in Roorkee, which is also selected by Mr. Hill, and one in Behar; one on the Eastern Himalaya, one on the plateau of Western Bengal, one in Orissa, one in the Andaman Islands, and the remainder in Lower Bengal and Cachar. Moreover, I was careful to eliminate all errors arising from the use of different uncompared instruments; and how necessary such a proceeding is, I illustrated by the remark that I have known sun-thermometers bearing the names of the best London makers differing 10° and 15° in their indications when exposed side by side under similar conditions to the sun. This precaution Mr. Hill has not taken, and I think his results are probably in a great measure due to that fact." Mr. Blanford thinks the sudden changes in the Roorkee register may be accounted for by the fact that the thermometer was twice changed, and the apparent increase in the wind's velocity by the shifting of the anemometers at Benares and Hazaribagh.

With respect to what Mr. Hill has said of the elements of error probably inherent to the method of discussion which Mr. Blanford adopted, while he admits the great difficulty there is in eliminating the effect of disturbing causes, he cannot admit that any *systematic* error was introduced, in the way suggested by Mr. Hill.

Mr. Blanford concludes:—"While on this subject I would direct attention to the importance of regular actinometric observations, of an absolute not merely relative character (such as are shown by the ordinary sun thermometers). The importance of making the solar changes a part of meteorological study is now fully recognised, and it is understood that a trained photographer is about to be sent to India to take photographs of the sun, but if this is so, regular actinometric observation should certainly form a part of the work. The best place perhaps would be Leh, where the atmosphere is remarkably free from haze and dust, which is not the case on the plains of Upper India; nor indeed, in dry weather, on the north-west Himalaya. At Leh, 11,500 feet above the sea, the radiation is most intense. Regular observations with the actinometer carried on for a few years at this place should satisfactorily decide the question of the variation of the sun's heat."

Mr. Blanford also sends a reply to the letter of "Old Madrassee" in NATURE, vol. xvi. p. 519. Mr. Blanford believes that to anyone who has seen or can readily refer to the report on the question of the periodical variation of the rainfall of Madras, it will be abundantly obvious either that "Old Madrassee" can never have seen that report or that he must have misinterpreted its whole purport and argument, and in his references to it, must have trusted to a somewhat unusually treacherous memory.

On the subject of solar radiation and sun-spots Mr. Hill writes that since his article (vol. xvi. p. 505) was written he has gone over the registers of four other stations at which solar thermometers have been in use for five or six years. The former method of treatment is not applicable to these on account of breaks in the registers and changes in the instruments; but adopting suitable methods to compensate for this, the results are as given in the following table, which shows the variation of each year from the preceding one:—

Stations.	1869.	1870.	1871.	1872.	1873.	1874.	1875.	1876.
Benares . .	° —	° − 1·0	—	° + 0·4	° + 1·9	° − 5·8	° + 3·3	° + 2·0
Gorakhpur.	− 0·6	—	—	+ 1·0	+ 0 9	− 0 4	− 2·7	− 1·5
Ranikhet .	—	—	—	+ 0·4	− 3 2	− 6·1	− 3·3	+ 2·9
Ajmere . .	—	− 2·0	+ 2·9	+ 1·7	+ 12·1	+ 3·2	− 4·7	− 0·3
Mean . .	− 0·6	− 1·5	+ 2·9	+ 0·9	+ 2·9	− 2·3	− 1·8	+ 0·8

Owing to inequalities in the number of months combining to

give the averages in the table, and to variations in the number of clear days in each month, the changes from year to year are very irregular, but on the whole there is a decided increase from 1870 to 1876. The sudden fall from 1873 to 1874 must be attributed, Mr. Hill thinks, to the greater diathermancy of the clear air at three of the stations in the former than in the latter year. It is worthy of note that 1873 was a very dry year at all the stations, but that 1874 was much wetter than usual except at Ajmere, where it was drier than 1873. At this station the solar radiation temperature shows a rise instead of a fall between 1873 and 1874.

With regard to the change of anemometer referred to by Mr. Blanford, Mr. Hill says that fortunately, in the present case, any other pair of stations, such as Madras and Vizagapatam, will do as well. With reference to the possible variation of the winter rain of Europe according to the supposed variation in the force of the anti-trade, Mr. Hill notices that the rainfall of London shows such a variation, though not very clearly. He adduces some figures in support of this.

In Mr. Hill's paper, vol. xvi., the word *minimum*, p. 505, second column, eighteenth line from bottom, *exact*, same column, third line from bottom, and *commutative*, p. 506, first column, fourth line from bottom, should be *maximum*, *excess*, and *cumulative* respectively.

Mr. Hill also writes that the large double oscillation in the decennial period of rainfall in Southern India, pointed out by Mr. J. A. Broun, in NATURE, vol. xvi. p. 333, will probably be found to exist in other parts of the country, including the north. One of the longest continuous registers of rainfall in existence for any station in Upper India is that kept by the G.T. Survey Office at Mussoorie, in the Himalayas, lat. N. 30° 28′, long. E. 78° 7′, altitude 6,500 feet. The rain has been recorded since 1854, but only during the rainy season, May to October, inclusive; and the register down to 1873 has been already published by Mr. J. B. N. Hennessey, in the *Proc. R.S.*, vol. xxii. No. 152. Mr. Hennessey's table, extended down to the present year by means of a register kept by the Civil Surgeon, gives a general mean for the twenty-four years of 83·2 inches, and an absolute range of no less than 104 inches.

When the yearly rainfalls are arranged in series of two, three, &c., up to twelve years each, beyond which number it is impossible to extend the series without taking as representative the uncorrected falls of single years, it becomes evident that the great periodic oscillation that underlies the irregular variations must complete its cycle in from nine to twelve years, for the 9-, 10-, 11-, and 12-year series, all show a large amplitude of oscillation, and in the 11-year series the maximum and minimum occur at nearly opposite phases of the cycle. It is also evident that in the 6-year series the conditions are the same, the only difference being that the amplitude does not appear quite so great.

Calculating the coefficients of the equation of sines for the ten-and-a-half-year period, as Mr. Broun has done, we get for the variable part of the Mussoorie rainfall—

$$y = 11{\cdot}4 \sin \theta + 14{\cdot}0 \sin (2\theta + 337°).$$

This may be compared with the equations given in Mr. Broun's article for Madras and Trevandrum,[1] for in all these equations $\theta = 0$ for the years 1838·5, 1849, 1859·5, 1870, &c. The difference of the two angles, 259° and 337°, causes a difference of more than a year in the epochs of the maxima and minima of the secondary oscillations, otherwise there is a wonderful similarity between the formulæ for two such widely-separated stations as Mussoorie and Trevandrum.[2]

Mr. Hill thinks it most likely that the oscillation of the five-yearly period is either purely accidental or the effect of some cause not yet understood.

Mr. Archibald, writing on the subject of Cyclone Generation, directs attention to an exceedingly interesting article in the *Pioneer* of October 30, entitled "A Cyclone Study," in which the author brings forward some very strong additional proof in confirmation of the "condensation theory" held by Messrs. Eliot and Blanford as opposed to the "parallel wind theory" advocated by Drs. Hann and Thau, and Messrs. Meldrum and Willson. After giving a clear account of the main points of difference between the above theories, the writer then proceeds to trace the history of the last cyclone in the Bay of Bengal, the Madras cyclone of May last, from its origin to its final disappearance, pointing out certain circumstances as giving strong support in favour of the condensation theory, and as completely disposing of the parallel wind theory—at all events as far as regards this particular cyclone.

[1] Viz.: $y = 5{\cdot}4 \sin (\theta + 50°) + 4{\cdot}6 \sin (2\theta + 252°)$,
and $y = 5{\cdot}6 \sin (\theta - 17°) + 8{\cdot}4 \sin (2\theta + 259°)$.

[2] The above equation for Mussoorie gives the maxima in 1860·3, 1870·8, &c., and the minima in 1857·2, 1867·7, &c. The first term alone would give the maxima in 1860·1, 1870·6, &c., and the minima in 1855·9, 1866·4, 1876·9, &c.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—The Mathematical Tripos' list this year contains ninety-four names. There are thirty-one classed as Wranglers, thirty as Senior Optimes, twenty-nine as Junior Optimes, and four Ægrotant. The Senior Wrangler is Mr. Ernest William Hobson, Scholar of Christ's College, eldest son of Mr. W. Hobson, proprietor and editor of the *Derbyshire Advertiser*. He was educated at Derby School, and in 1874 obtained an open scholarship at Christ's College. During his undergraduate career he obtained the first place among the students in the college examinations in mathematics. His college tutor was Mr. Peile, and his private tutor Mr. E. J. Routh, of St. Peter's College. Next to him are Mr. John Edward Aloysius Steggall, scholar of Trinity College, and Mr. Christopher Graham, scholar of Caius.

During the present term three courses of lectures on chemistry will be delivered. A general course by Mr. Main at St. John's College; a course by Mr. Lewis at Downing College; and a course on the non-metallic elements by Mr. Pattison Muir at Caius College.

MUNICH.—The university is becoming in point of numbers one of the foremost in Germany. The calendar for the present year shows an attendance of 1,360, of whom 1,014 are from Bavaria and 346 from other countries. In the theological faculty there are 82, in the legal 387, in the medical 341, in the philosophical (history, philology, &c.) 246, (science) 151, together with 136 pharmaceutical chemists, and 17 in forestry and agriculture. The corps of instructors numbers 114. The university, although but fifty-two years old, has been well supported by the State, and possesses a large variety of laboratories, cabinets, &c., and a library of 20,000 volumes.

SOCIETIES AND ACADEMIES

LONDON

Royal Astronomical Society, January 11.—Dr. Huggins, F.R.S., in the chair.—A paper by Mr. W. F. Denning on suspected repetitions or second outbursts from radiant points, and on the long duration of meteor showers, was read, showing that a radiant in some cases continues active during three or four months, and sometimes a second outburst occurs after an interval of six months, so that meteors may be seen coming from the same radiant at opposite sides of the earth's orbit. Capt. Tupman commented on this paper at some length, and pointed out some of the difficulties these conclusions presented.—Dr. Wentworth Erck read a paper on a combined position and setting circle, rendering the declination circle unnecessary on large Newtonian equatorials. He also showed a small and singularly portable equatorial mounting, and read a note on a spectroscope made by Mr. Grubb for Prof. Young, showing certain improvements. Mr. John Browning admired the ingenuity of these, and explained which of them were new and which were not.—Mr. A. A. Common read a note on the satellites of Mars and Saturn.—A note was read describing the failure of the Melbourne telescope to deal with the satellites of Mars.—Mr. S. Waters read a paper on the distribution of the fixed stars in space.—Mr. Christie read a paper on specular reflection from Venus, the purport of which was that his recent observations of the planet with the polarising eye-piece emphatically corroborated those made in 1876. By means of this eye-piece the light of the disc is gradually reduced; and he found in every examination that the last part of the disc to disappear was situated at a point which was found by calculation to coincide with the point indicated by the theory of specular reflection, thus confirming Mr. Brett's original description of the phenomenon. Mr. Christie had the assistance of Capt. Tupman in his recent observations. Mr. Neison suggested certain other explanations of the appearances described, and after further discussion the meeting adjourned.

Zoological Society, January 15.—R. Hudson, F.R.S., vice-president, in the chair.—A communication was read from Mr.

Andrew Anderson, F.Z.S., containing some corrections and additions to a former paper of his on the raptorial birds of the north-west provinces, read before the Society on March 21, 1876.—A communication was read from Mr. F. Moore, F.Z S., containing a revision of the genera and species of European and Asiatic lepidoptera belonging to the family Lithosiidæ. The author characterised thirty-eight genera in this memoir, and gave the descriptions of eighty new species.—Mr. A. Boucard, C.M.Z.S., read a paper in which he gave a list of the birds he had collected during a recent expedition to Costa Rica. The number of birds collected during his five months' stay was about one thousand in number, representing 250 species, amongst which were two new to science (*Zonotrichia boucardi* and *Sapphironia boucardi* of Mulsant) and many others of great rarity.—Two papers were read by Mr. G. French Angas. The first contained descriptions of seven new species of land shells recently collected in Costa Rica by M. A. Boucard. The second contained the description of a new species of *Latiaxis* from an unknown locality, proposed to be called *L. elegans*.—A communication was read from Dr. H. Burmeister, containing notes on *Conurus hilaris* and other parrots of the Argentine Republic.—A communication was read from the Count Salvadori, C.M.Z.S., in which an account was given of the birds collected during the voyage of H.M.S. *Challenger*, at Ternate, Amboyna, Banda, the Ké Islands, and the Aru Islands.—Prof. Garrod, F.R.S., read a paper on certain points in the anatomy of the Momotidæ, in which he adduced facts substantiating their affinities with the Todidæ, Alcedinidæ, and other Piciformes. The second paper described the extraordinary structure of the gizzard of the Fijian Fruit Pigeon (*Carpophaga latraus*), in connection with the fruit on which it feeds, that of *Oncocarpus vitiensis*.—A communication was read from Mr. Edgar A. Smith, F.Z.S., containing the description of a new species of Helix from Japan, which he proposed to call *Helix* (*Camena*) *congener*.—A communication was read from the Marquis of Tweeddale, F.R.S., containing an account of a collection of birds made by Mr. A. H. Everett in the Philippine Islands of Dinagat, Bazol, Nipak, and Sakeryok. Six new species were found in this collection and were named *Ceyx argentata*, *Hypothymys cœlestis*, *Mixornis capitalis*, *Dicæum schistaceum*, *D. everetti*, and *Prionochilus olivaceus*.—A second paper by the Marquis of Tweeddale gave the description of a new genus and species of bird from the Philippine Island of Negros, for which the name *Dasycrotapha speciosa* was proposed.

Photographic Society, January 8.—James Glaisher, F.R.S., president, in the chair.—Papers were read by Capt. Abney, F.R.S., on the theory of the destruction of the undeveloped photographic image; by Edward Viles, on the production of enlarged photographs of microscopic objects; and by Edwin Cocking, "stray thoughts on the exhibition."—Capt. Abney in his paper stated the result of experiments undertaken to ascertain the cause of the fading away of the undeveloped image on dry plates by long keeping after exposure. Films of pure silver iodide, and of pure silver bromide, after exposure, were washed with potassium permanganate, potassium bichromate, and chromic acid; with the silver iodide salt, *all* destroyed the image, with the silver bromide salt the last two oxidising agents alone were effective. If this destruction of the image was caused by oxidation of the silver atom, it should also be oxidised by ozone—which experiments showed was the case. Capt. Abney then assumes that the effect of time on the image on a dry plate is to oxidise an atom of each of the molecules forming the image.

Institution of Civil Engineers, January 15.—The newly-elected president, Mr. John Frederic Bateman, F.R.SS.L. and E., delivered an inaugural address. After a passing allusion to the growth of the Institution, which at the end of 1844 numbered only 552 of all classes, now increased to 3,189, reference was made to some of the addresses of the eighteen gentlemen who had previously occupied the presidential chair, mainly for the purposes of comparison. Proceeding to matters more personal to every member of the Institution, the President urged that engineering was but, in fact, the embodiment of practical wisdom; or, in the words of Bacon, "the conjunction of contemplation and action."

EDINBURGH

Royal Society, January 7.—Bishop Cotterill, vice-president, in the chair.—Prof. Blackie read a paper on Mr. Gladstone's theory of colour-sense in Homer, which he completely refuted. A discussion followed, in which Principal Sir Alexander Grant, Bart., the Rev. Dr. Cazenove, Prof. Fleeming Jenkin, Dr. Donaldson, and others took part.—Prof. Tait postponed his paper on the intensity of currents required to work the telephone but mentioned that Mr. James Blyth had obtained good results with telephones in which he had employed discs of copper-wood vulcanised india-rubber paper, instead of the usual iron ones.—Prof. Tait also laid on the table a double mouthed-piece horn for producing chords by two performers on the same instrument.

VIENNA

Imperial Academy of Sciences, November 16, 1877.—On ice in the Danube in Lower Austria, in the winter 1876–77, by the Minister of the Interior.—Researches on the consciousness of place and its relation to the conception of space, by M. Stricker.—On the temperature of Vienna according to 100 years' observations, by M. Hann.—On the phanerogam flora of the Hawaii Islands, by M. Reichardt.

November 22, 1877.—On a partial differential equation of the first order, by M. Hocevar. The laws of the individuality of the planets of our solar system; attempt to establish a general theory, by M. Lehmann.

December 6, 1877.—The velocity of propagation of spark waves, by MM. Mach, Tumlirz, and Kögler.—On the application of Doppler's principle to the progressive motion of luminous gas molecules, by M. Pfaundler.—On some problems of the theory of elastic reaction, and on a new method of observing vibrations by reading of mirrors, without loading the vibrating body with a mirror of considerable size, by M. Boltzmann.—Determination of surfaces any of whose parts, from two fixed points, are projected through cones the apertures of which are in a given proportion, by M. Weyr.—On mononitrobrenzcatechin, by M. Benedikt.—Size and position of the optical axes of elasticity in gypsum, by M. von Lang.—On the orbit of the planet Laurentia (162), by M. Zelbr.

PARIS

Academy of Sciences, January 21.—M. Daubrée in the chair.—On account of the death of MM. Becquerel and Regnault, the *séance* was adjourned. The funeral of M. Becquerel took place the same day, that of M. Regnault next day. Discourses on the former were pronounced by MM. Fizeau and Daubrée; on the latter by MM. Debray, Jamin, Daubrée, and Laboulaye. [These are reported in the *Comptes Rendus* for the week.]

CONTENTS

THURSDAY, FEBRUARY 7, 1878

THE SOCIETY OF TELEGRAPH ENGINEERS

WHEN a society which numbered 110 members at the date of its first public meeting can, after an existence of only six years, count 1,000 names upon its books, it has at least justified its existence, and those who have taken the chief part in calling it into being and guiding its course, may fairly consider that the numbers who have sought association with them prove that their proceedings have been, at any rate, not injudicious. It was, therefore, with good reason, that Dr. C. W. Siemens began his address to the Society of Telegraph Engineers on the occasion of his re-election to the office of President, by congratulating the society on the progress made by it since he addressed it in the capacity of its first President on February 28, 1872.

In these congratulations we heartily join, and we think that no one will question the wisdom of the society in calling back to the Presidentship a man who did so much in the early days of its career to prepare the way for the success since realised.

The claim of the Society of Telegraph Engineers to rank as a scientific institution cannot, however, be founded upon the mere number of its members, nor even on the scientific eminence of some of the names to be found in the list. Its scientific position must of course be judged of by considering, not how many or who its members are, but what they do in their associated capacity for the advancement of science. Ample materials for forming such an estimate as this are afforded by the six substantial volumes already published of the *Journal* of the Society of Telegraph Engineers. These volumes contain the papers communicated to the Society and reports of discussions at the meetings, and in addition a considerable number of reprints or abstracts of papers published elsewhere, bearing on the objects pursued by the Society. As might be expected in the case of a society founded primarily to promote the advancement, not of abstract science, but of a branch of industrial enterprise, papers of a so-called "practical" kind are the most numerous, and, if we may judge from the reported discussions, papers of this class are those which call forth the most general interest at the Society's meetings. But even among such papers, embodying as they usually do the results of careful observation and long experience on the matters of which they treat, there are few from which the student of physics may not gather some hint of value. There are, however, a considerable number of papers of which the scientific bearing is more direct. These are papers which, dealing with questions arising primarily out of the practice of the telegraph engineer, treat the problems discussed from the point of view afforded by the general scientific principles applicable to them, or which contain results of no less scientific than practical value. Among papers of this class, one by Mr. Hockin (vol. v. pp. 432-459) on "The Magnitude of Signals received through a Submarine Cable with various Connections at each end, and the best Resistance for the Recording Instrument," is specially deserving of mention. It contains a very masterly treatment of what is in reality a purely scientific problem, though one which has very direct practical importance. And here we may digress for a little in order to point out that this paper of Mr. Hockin's affords an instructive illustration of the mutual beneficial interaction between "theory" and "practice" of which the whole history of the electric telegraph is full. The telegraph is in a fuller degree than most practical inventions the direct outcome of scientific investigation, but when, in the progress of telegraphic enterprise, the project arose of laying long submarine lines, it was found that, though the general nature of the electrical difficulties to be encountered was known, yet the scientific knowledge of the time was not sufficient to indicate clearly the way in which they were to be overcome, and from the nature of the case but little help was forthcoming from empirical experience. The matter was in this state when Sir William Thomson took up the question of the transmission of signals through submarine telegraph cables, and showed how the practical message-carrying power of an insulated conductor laid under water is connected with the dimensions and certain definite electrical qualities of the conductor and its insulating coating. The conclusions which he arrived at mathematically as long ago as 1855 have since remained the foundation of all successful practice in the manufacture of telegraph cables. Sir W. Thomson, however, took account only of the properties of the cable itself, whereas in the actual working of submarine telegraphy very much depends upon the proper selection and arrangement and adaptation to each particular cable of the sending and receiving apparatus employed at the two ends; and what Mr. Hockin has now done is to give a general theory which takes account of the electrical properties of the instruments as well as of the cable. Returning to the *Journal* of the Society of Telegraph Engineers, we may mention a short paper by Mr. Sabine on the Capacity of Accumulators Variously Combined, one by Sir William Thomson on the Comparison of Electrostatic Capacities, and a note by Prof. Maxwell on the Theory of Lightning Conductors, among the original articles, as well as Messrs. Longridge and Brooks's paper on the Submergence of Telegraph Cables and Mr. Schwendler's on the Theory of Duplex Telegraphy, among the reprints, as examples which afford further proof that the Society, is established for practical objects, is not blind to the aid to be derived in the pursuit of those objects from the study of scientific principles. And although we do not suppose that all the 1,000 members study such papers as we have referred to with great eagerness, yet the mere fact of their circulation must do something to convince the most arrogantly "practical" man among them that ignorance is not in all respects a ground for thankfulness.

So far this flourishing society has professedly occupied itself only with telegraphy, but there are not wanting signs either in the Journal or in Dr. Siemens's address, of the difficulty of separating telegraphy from other departments of what may be called applied electricity. Thus, more than one paper has been read to the society upon the application of electricity to firing mines and torpedoes, an operation which, when successfully performed, generally results in causing the persons affected to dispense permanently with telegraphic communication, and Dr. Siemens devotes nearly a quarter of his address

to discussing the application of electricity for illuminating purposes, to the transmission of motive power, and in metallurgic processes. Recent improvements in the means of obtaining powerful electric currents seem to open up a prospect of such applications as those just mentioned, assuming in the near future greater practical importance than they have hitherto possessed, and it does not seem unlikely that, whether or not they think fit to assume the wider designation, the Society of Telegraph Engineers will have become a Society of Electrical Engineers.

G. C. F.

TAIT'S "THERMODYNAMICS"[1]

II.

Sketch of Thermodynamics. By P. G. Tait, M.A., formerly Fellow of St. Peter's College, Cambridge, Professor of Natural Philosophy in the University of Edinburgh. Second Edition, revised and extended. (Edinburgh: David Douglas, 1877.)

PROF. CLAUSIUS is himself the principal founder of the kinetic theory of gases. The theory of the exchanges of the energy of collections of molecules was afterwards developed by Boltzmann to a much greater extent than had been done by Clausius, and it appears from his investigations that whether we suppose the molecules to be acted on by forces towards fixed centres or not, the condition of equilibrium of exchange of energy, or in other words the condition of equality of temperature of two bodies, is that the average kinetic energy of translation of a single molecule is the same in both bodies.

We may therefore define the temperature of a body as the average kinetic energy of translation of one of its molecules multiplied into a constant which is the same for all bodies. If we also define the total heat of the body as the sum of the whole kinetic energy of its molecules, then the total heat must be equal to the temperature multiplied into the number of molecules, and by the ratio of the whole kinetic energy to the energy of translation, and divided by the above constant.

The kinetic theory of gases has therefore a great deal to say about what Rankine and Clausius call the actual heat of a body, and if we suppose that molecules never coalesce or split up, but remain constant in number, then we may also assert, all experiments notwithstanding, that the real capacity for heat (as defined by Clausius) is constant for the same substance in all conditions.

Rankine, indeed, probably biased by the results of experiments, allowed that the real specific heat of a substance might be different in different states of aggregation, but Clausius has clearly shown that this admission is illogical, and that if we admit any such changes, we had better give up real specific heat altogether.

Statements of this kind have their legitimate place in molecular science, where it is essential to specify the dynamical condition of the system, and to distinguish the kinetic energy of the molecules from the potential energy of their configuration; but they have no place in thermodynamics proper, in which we deal only with sensible masses and their sensible motions.

Both Rankine and Clausius have pointed out the importance of a certain function, the increase or diminution of which indicates whether heat is entering or leaving the body. Rankine calls it the thermodynamic function, and Clausius the entropy. Clausius, however, besides inventing the most convenient name for this function, has made the most valuable developments of the idea of entropy, and in particular has established the most important theorem in the whole science,—that when heat passes from one body to another at a lower temperature, there is always an increase of the sum of the entropy of the two bodies, from which it follows that the entropy of the universe must always be increasing.

He has also shown that if the energy of a body is expressed as a function of the volume and the entropy, then its pressure (with sign reversed) and its temperature are the differential coefficients of the energy with respect to the volume and the entropy respectively, thus indicating the symmetrical relations of the five principal quantities in thermodynamics.

But Clausius, having begun by breaking up the energy of the body into its thermal and ergonal content, has gone on to break up its entropy into the transformational value of its thermal content and the disgregation.

Thus both the energy and the entropy, two quantities capable of direct measurement, are broken up into four quantities, all of them quite beyond the reach of experiment, and all this is owing to the actual heat which Clausius, after getting rid of the latent heat, suffered to remain in the body.

Sir William Thomson, the last but not the least of the three great founders, does not even consecrate a symbol to denote the entropy, but he was the first to clearly define the intrinsic energy of a body, and to him alone are due the ideas and the definitions of the available energy and the dissipation of energy. He has always been most careful to point out the exact extent of the assumptions and experimental observations on which each of his statements is based, and he avoids the introduction of quantities which are not capable of experimental measurement. It is therefore greatly to be regretted that his memoirs on the dynamical theory of heat have not been collected and reprinted in an accessible form, and completed by a formal treatise, in which his method of building up the science should be exhibited in the light of his present knowledge.

The touchstone of a treatise on thermodynamics is what is called the second law.

Rankine, as we have seen, founds it on statements which may or may not be true, but which cannot be considered as established in the present state of science.

The second law is introduced by Clausius and Thomson as an axiom on which to found Carnot's theorem that the efficiency of a reversible engine is at least as great as that of any other engine working between the same limits of temperature.

If an engine of greater efficiency exists, then, by coupling this engine with Carnot's engine reversed, it is possible to restore to the hot body as much heat as is taken from it, and at the same time to do a certain amount of work.

If with Carnot we suppose heat to be a substance, then this work would be performed in direct violation of the first law—the principle of the conservation of energy. But if we regard heat as a form of energy, we cannot apply

[1] Continued from p. 259.

this method of *reductio ad absurdum*, for the work may be derived from the heat taken from the colder body.

Clausius supposes all the work gained by the first engine to be expended in driving the second. There is then no loss or gain of heat on the whole, but heat is taken from the cold body, and an equal quantity communicated to the hot body, and this process might be carried on to an indefinite extent.

In order to assert the impossibility of such a process in a form of words having sufficient verisimilitude to be received as an axiom, Clausius, in his first memoir, simply says that this process "contradicts the general deportment of heat, which everywhere exhibits the tendency to equalise differences of temperature, and therefore to pass from the warmer to the colder body." [1]

In its obvious and strict sense no axiom can be more irrefragable. Even in the hypothetical process, the impossibility of which it was intended to assert, every communication of heat is from a warmer to a colder body. When the heat is taken from the cold body it flows into the working substance which is at that time still colder. The working substance afterwards becomes hot, not by communication of heat to it, but by change of volume, and when it communicates heat to the hot body it is itself still hotter.

It is therefore hardly correct to assert that heat has been transmitted or transferred from the colder to the hotter body. There is undoubtedly a transfer of energy, but in what form this energy existed during its middle passage is a question for molecular science, not for pure thermodynamics.

In a note added in 1864 Clausius states the principle in a modified form, "that heat cannot of itself pass from a colder to a warmer body" [1] and finally, in the new edition of his "Theory of Heat" (1876) he substitutes for the words "of itself" the expression "without compensation." [2]

With respect to the first of these emendations we must remember that the words "of itself" are not intended to exclude the intervention of any kind of self-acting machinery, and it is easy, by means of an engine which takes in heat from a body at 200° C., and gives it out at 100° to drive a freezing machine so as to take heat from water at 0°, and so freeze it, and also a friction break so as to generate heat in a body at 500°. It would therefore be necessary to exclude all bodies except the hot body, the cold body, and the working substance, in order to exclude exceptions to the principle.

By the introduction of the second expression, "without compensation," combined with a full interpretation of this phrase, the statement of the principle becomes complete and exact; but in order to understand it we must have a previous knowledge of the theory of transformation-equivalents, or in other words of entropy, and it is to be feared that we shall have to be taught thermodynamics for several generations before we can expect beginners to receive as axiomatic the theory of entropy.

Thomson, in his "Third Paper on the Dynamical Theory of Heat" (*Trans.* R.S. Edin., xx., p. 265 (read March 17, 1851) has stated the axiom as follows:—

"It is impossible, by means of inanimate material agency, to derive mechanical effect from any portion of matter by cooling it below the temperature of the coldest of surrounding objects."

Without some further restriction this axiom cannot be considered as true, for by allowing air to expand we may derive mechanical effect from it by cooling it below the temperature of the coldest of surrounding objects.

If we make it a condition that the material agency is to be left in the same state at the end of the process as it was at first, and also that the mechanical effect is not to be derived from the pressure of the hot or of the cold body, the axiom will be rendered strictly true, but this brings us back to a simple re-assertion of Carnot's principle, except that it is extended from heat engines to all other kinds of inanimate material agency.

It is probably impossible to reduce the second law of thermodynamics to a form as axiomatic as that of the first law, for we have reason to believe that though true, its truth is not of the same order as that of the first law.

The first law is an extension to the theory of heat of the principle of conservation of energy, which can be proved mathematically true if real bodies consist of matter "as per definition," acted on by forces having potentials.

The second law relates to that kind of communication of energy which we call the transfer of heat as distinguished from another kind of communication of energy which we call work. According to the molecular theory the only difference between these two kinds of communication of energy is that the motions and displacements which are concerned in the communication of heat are those of molecules, and are so numerous, so small individually, and so irregular in their distribution, that they quite escape all our methods of observation; whereas when the motions and displacements are those of visible bodies consisting of great numbers of molecules moving all together, the communication of energy is called work.

Hence we have only to suppose our senses sharpened to such a degree that we could trace the motions of molecules as easily as we now trace those of large bodies, and the distinction between work and heat would vanish, for the communication of heat would be seen to be a communication of energy of the same kind as that which we call work.

The second law must either be founded on our actual experience in dealing with real bodies of sensible magnitude, or else deduced from the molecular theory of these bodies, on the hypothesis that the behaviour of bodies consisting of millions of molecules may be deduced from the theory of the encounters of pairs of molecules, by supposing the relative frequency of different kinds of encounters to be distributed according to the laws of probability.

The truth of the second law is therefore a statistical, not a mathematical, truth, for it depends on the fact that the bodies we deal with consist of millions of molecules, and that we never can get hold of single molecules.

Sir William Thomson [1] has shown how to calculate the

[1] Und das widerspricht dem sonstigen Verhalten der Wärme, indem sie überall das Bestreben zeigt, vorkommende Temperaturdifferenzen auszugleichen und also aus den wärmeren Körpern in die kälteren überzugehen.

[2] Dass die Wärme nicht von selbst aus einem kälteren in einem wärmeren Körper übergehen kann.

[3] Ein Wärmeübergang aus einem kälteren in einem wärmeren Körper kann nicht ohne Compensation stattfinden.

[1] "On the Kinetic Theory of the Dissipation of Energy," *Proc.* R.S. Edin., February 16, 1874, vol. viii. p. 323, also in NATURE, vol. ix. p. 441.

probability of the occurrence within a given time of a given amount of deviation from the most probable distribution of a finite number of molecules of two different kinds in a vessel, and has given a numerical example of a particular case of the diffusion of gases.

The same method might be extended to the diffusion of heat by conduction, and the diffusion of motion by internal friction, which are also processes by which energy is dissipated in consequence of the motions and encounters of the molecules of the system.

The tendency of these motions and encounters is in general towards a definite state, in which there is an equilibrium of exchanges of the molecules and their momenta and energies between the different parts of the system.

If we restrict our attention to any one molecule of the system, we shall find its motion changing at every encounter in a most irregular manner.

If we go on to consider a finite number of molecules, even if the system to which they belong contains an infinite number, the average properties of this group, though subject to smaller variations than those of a single molecule, are still every now and then deviating very considerably from the theoretical mean of the whole system, because the molecules which form the group do not submit their procedure as individuals to the laws which prescribe the behaviour of the average or mean molecule.

Hence the second law of thermodynamics is continually being violated, and that to a considerable extent, in any sufficiently small group of molecules belonging to a real body. As the number of molecules in the group is increased, the deviations from the mean of the whole become smaller and less frequent; and when the number is increased till the group includes a sensible portion of the body, the probability of a measurable variation from the mean occurring in a finite number of years becomes so small that it may be regarded as practically an impossibility.

This calculation belongs of course to molecular theory and not to pure thermodynamics, but it shows that we have reason for believing the truth of the second law to be of the nature of a strong probability, which, though it falls short of certainty by less than any assignable quantity, is not an absolute certainty.

Several attempts have been made to deduce the second law from purely dynamical principles, such as Hamilton's principle, and without the introduction of any element of probability. If we are right in what has been said above, no deduction of this kind, however apparently satisfactory, can be a sufficient explanation of the second law. Indeed some of them have already indicated their unsoundness by leading to determinations of physical quantities which have no existence, such as the periodic time of the alternations of the volume of particular gases.[1]

J. CLERK MAXWELL

[1] Szily, *Phil. Mag.*, October, 1876; Clausius, *Pogg. Ann.*, cxlii., p. 433; *Pogg. Ann.*, cxlvi., p. 585, May, 1872; J. J. Müller, *Pogg. Ann.*, clii., p. 105.

OUR BOOK SHELF

Heroes of North African Discovery. By N. D'Anvers. (London: Marcus Ward and Co., 1877.)

MR. D'ANVERS has here made an interesting *résumé* of the work of the principal travellers who have made Africa known to the world. He briefly dismisses the earlier explorers, the bulk of the volume being devoted to those of the eighteenth and nineteenth centuries. Mr. D'Anvers has evidently read his authorities carefully, and gives a clear account of his heroes' adventures, and of the main results achieved. The book is evidently meant for young readers, and to them both the text and the numerous illustrations will prove attractive. But all who wish to have a fair knowledge of what has been hitherto achieved in the field of African discovery should read this interesting and instructive volume. The author prefixes a list of the authorities he has consulted, and promises another volume on South Africa, in which the results obtained by Mr. Stanley will be embodied.

Manual of Agriculture; including the Application thereto of Chemistry, Geology, Botany, Animal Physiology, and Meteorology. By Richard Henderson.

THIS is a reprint of one of the Highland Agricultural Society's prize essays. It forms a very marked exception from the thoroughly practical essays which are usually published by that society, so much so indeed, that it is a source of regret that a society which has done so much to improve agricultural education, should have in any way stamped the present work with its approval and authority.

The work is divided into seven chapters, of which five are devoted to some notices of chemistry, geology, botany, animal physiology, and meteorology, and the seventh alone treats upon the application of these sciences to agricultural practice, which is the professed subject of the work.

A few extracts from the first six chapters will given an idea of the character of this part of the work. The second chapter deals with chemistry, and is largely made up of comments upon eighteen elements, the descriptions being remarkably similar to those given by Roscoe in his "Lessons." It is fair to say that the author occasionally introduces original remarks, as, for instance, in saying that "carbon forms about fifty per cent. of the residue of plant-life when the latter is charred, and access of atmospheric air or oxygen prevented, for oxidised carbon escapes as a gas." Prof. Roscoe fares rather badly at the hands of our author, since he in another place says, "Roscoe gives the following graphic formula as the average composition of blood," and he appends the average percentage composition.

We are told again that at the sea-level the pressure of the air "can support a column of mercury thirty inches high in a tube *in vacuo*." Concerning fogs and mists, "they result from the radiation of heat from land and water, taking with it aqueous vapour, which becomes visible upon encountering cooler air. Similarly rain is produced when heated volumes of air are deprived of their heat, through the fall of condensed vapour, which assumes, according to the temperature it encounters, the form of hail, rain, or snow."

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Sun-Spots and Terrestrial Magnetism

MR. B. G. JENKINS, in his letter to NATURE, vol. xvii. p. 260, says, "I have ventured to state my belief that we are now passing through a long minimum period, one very similar to that which occurred at the close of last century." It was the chief object of

my communication to NATURE, vol. xvii. p. 183, to show the latter fact as far as the observations go up to the present time. I did not, however, venture in that article to make a guess as to the future, which really could have little weight till we have another year's observations. Yet I thought it desirable, nearly two months ago, to place in the hands of Prof. Balfour Stewart the evidence that the possibility of such an event, as an obvious conclusion from my results, had not escaped me. As Mr. Jenkins has published his guess, I may do the same with mine. The latter differs, however, from his in a very important way. He supposes the next maximum will be in 1887, whereas I suppose that the weak maximum of 1797 may be repeated near 1880. In this latter case the interval between the two will be nearly double that found by me (forty-two years), during which the sun-spot period appears to have all its different lengths.

February 1 JOHN ALLAN BROUN

Terrestrial Magnetism

I HEREWITH submit a notice of an experiment for illustrating to a class the action of terrestrial magnetism. In a simple way it clearly exhibits to a large audience the action of the currents of electricity that pass around the earth. The experiment was suggested on reading a paper by Prof. J. W. Mallet, F.R.S., of the University of Virginia, on "The Apparent Alteration in Weight of a Wire placed East and West, and Traversed by an Electric Current" (*Phil. Mag.*, November, 1877).

Instead of disconnecting the wires placed east and west from the portion of the rectangle, as was done by Prof. Mallet in the experiment alluded to, whereby the attractive or repulsive action of the earth currents on one side only of the rectangle was obtained, it occurred to me to suspend the whole rectangle to a balance. Properly arranged in this way the attraction for parallel currents in same direction, and repulsion for currents in opposite direction would generate a couple, tending to produce rotation around an east and west horizontal axis, and hence augment the deflection of the balance.

A rectangular frame was made of light poplar wood, of section three by two centimetres, whose sides were one metre in length by three-fourths of a metre in breadth. About the perimeter of this rectangle there were wrapped twenty coils of insulated copper wire. Each extremity of the wire was made to terminate near the centre of one of the shorter sides, and passing through the wooden frame, was fastened and cut off about 3 c.m. from the frame.

This rectangular frame was then so suspended, in a horizontal position, by wires attached to the pans of an ordinary Delenil's hydrostatic balance, that the longer sides were at right-angles to the beam. By adjusting weights in the scale pans the index of the balance was brought to the zero. Two small orifices bored in a block of wood a centimetre apart, served as mercury cups, in which the extremities of the short terminal wires were immersed; near the bottom and through the walls of these wooden mercury-cups were screened small brass hooks, which served as connectors, to which the wires of the battery were attached. The balance was now so placed that the longer sides of the suspended rectangle were *at right-angles with the magnetic meridian*, or in the *magnetic* east and west line.

When the current from the battery was made to pass around the rectangle from east to west, on the northern side, and from west to east, on the southern side, by the theory of terrestrial magnetism, the north side of the rectangle would be attracted to the earth, and the south side repelled, and that this was so the corresponding deflection of the balance rendered plainly visible. When the current was reversed the deflection was in the opposite direction. By breaking and closing the circuit at proper intervals to augment the oscillations, the large frame was readily made to oscillate through an arc of 5°. When the sides of the rectangle were placed north-east and south-west, the current produced no sensible effect. A bichromate of potash battery of sixteen cells was used with plates of zinc and carbon 25 cm. by 6 cm.

With a rectangle containing a larger number of coils of wire attached to a delicate balance by the use of a *constant* battery, the variations in the earth's magnetism might be thus observed.

WM. LEROY BROUN

Vanderbilt University, Nashville, Tenn., January 11

Seiches and Earthquakes

IN the last number of NATURE (p. 234) you make an allusion to the fact that the earthquake of October 8, 1877, has not been traced by the self-registering "limnimeters" (tide-gauge) of M. Ph. Plantamour at Geneva, and myself at Morges. Let me take the opportunity of the great publicity of your excellent paper to ask the naturalists who live in other countries more frequently visited by earthquakes, for an explanation.

I believe I have demonstrated in many different papers that the phenomenon called *seiches*, which consists in rhythmical movements of the level of the lake, is a balancing-wave, a wave of stationary uninodal oscillation. The water moves in balancing oscillation in the two principal diameters of the lake, in the direction of the greatest length and of the greatest breadth. For setting the water in such an oscillatory movement there are two possible causes:—

1. A shock given to the water itself is the most frequent case, and I can prove that generally the *seiches* are caused by some rupture of the equilibrium of the atmospheric pressure; many storms, and especially those that fall somewhat abruptly on the lake, are accompanied by very high *seiches*, and I have many examples of the beginning of the oscillatory movement of the water exactly at the same time the storm commences.

2. A movement of the soil on which the water lies, an earthquake. It is evident that a shock given to a basin can put the water in oscillatory movement. In fact, it happens frequently. I will only recall the colossal transmission-waves in the Pacific Ocean on August 13, 1868, after the earthquake of Arica; that of May 9, 1877, at Iquique; and in earlier times, the earthquakes at Messina, 1783; at Port-Royal (Jamaica), 1692; at Callao, 1586, &c. If such enormous waves had taken place in a closed basin, as our lakes, it would probably have brought about an oscillatory movement of *seiches*. I could bring many facts to prove it; only one example, the earthquake of Lisbon (1755), was noticed in Switzerland and Germany, chiefly by the movements of the water of the lakes; the description of these movements recalls perfectly the *seiches*.

It is also theoretically probable that the shock given to the ground extends to the waters, and that an earthquake will produce *seiches* in a lake. Unfortunately the facts observed up to this time do not confirm this theoretical view. Since I established at Morges a self-registering limnimeter of the greatest sensitiveness, in March, 1876, six different earthquakes have been noticed in our country, and specially three earthquakes were felt at Morges itself—May 7 and November 29, 1876, and October 8, 1877. Not one of those six earthquakes has been traced by the self-registering limnimeter; not one has interrupted the rhythmic oscillation of the *seiches* which were taking place; not the smallest alteration of the curve has shown that the water had been acted upon in a peculiar manner; neither was the limnimeter of M. Ph. Plantamour, which was at work during the earthquake of October 8, 1877, influenced by that very severe shock. And yet our apparatus are extremely sensitive; when the lake is sufficiently calm my limnimeter can show the waves originated by a steamboat which passes 10–15 kilometres off the apparatus, or it registers the waves caused by a steamer which has passed by my observatory two or three hours before.

How can these contradictory facts be explained? On one hand, the earthquakes cause in many places enormous waves; on the other, three earthquakes strong enough to have awakened men out of their sleep, have not put in movement the most sensitive, always working, self-registering apparatus.

I suppose that the shocks of the earth do not transfer always the movement to the water; that only in a special direction of the shock a special intensity, a special duration, the water itself is put in movement and takes the rhythmic oscillation of the *seiches*. If I shake a basin the water is not always and necessarily put in oscillatory movement. I think it is the same for the *seiches*, and I believe that only certain movements of the earth cause the water of the lakes to move.

It is the point on which I seek an answer from naturalists who have more opportunity to observe the effects of earthquakes. I ask if each earthquake is accompanied by waves of the sea; if each shock of the same intensity is accompanied by waves of the same amplitude; if there are not differences between the different earthquakes; if some have the enormous waves of Iquique or Arica; if others are without those waves?

I should be very thankful to receive an answer to the above questions. F. A. FOREL

Morges, Switzerland, January 24

Electrical Experiment

A FRIEND of mine has called my attention to a letter of F. T. Pirani, of Melbourne, accompanied by some remarks of Prof. J. C. Maxwell, in NATURE, vol. xvii. p. 180.

Mr. Pirani concludes his letter with the words, "If the phenomenon (described in the letter) has not been noticed before, I shall be obliged to you if you will kindly communicate it to NATURE."

I take the liberty to request you to call, by means of your esteemed journal, the attention of the author to an article of mine, published in the late Prof. Poggendorff's *Annalen der Physik* (vol. clvii., 1876), an abstract of which appeared in the *Philosophical Magazine* (5 ser. vol. i.). The phenomenon alluded to in Mr. Pirani's letter, *i.e.* the existence of an electromotive force due to gravity, in a vertical column of an electrolyte, is, I believe, fully proved by the experiments described in the article. The same difficulties met with by Mr. Pirani and Prof. Maxwell, who repeated the experiment, that is, the presence of irregular, casual currents, due to bubbles of air, &c., have also been encountered by me; I intimate the means of getting rid, to a certain extent, of this influence.

The transport of metal in one direction being accompanied by a transport of the other ion in the opposite direction, the phenomenon is more complete than it might appear at first sight, and the electromotive force changes its sign according to the electrolyte employed. R. COLLEY

Kasan, Russia, January 23

Oriental Affinities in the Ethiopian Insect-Fauna

MANY naturalists have already drawn attention to the Indian affinities in the African fauna; in other words, the zoological relationship between the Oriental and Ethiopian regions. The late Dr. Stoliczka has pointed this out in the Malayan ornithology; Mr. Wallace has described the same thing in the mammalia and birds of West Africa, these possessing "a special Oriental or even Malayan element." He has also drawn attention to the Oriental element in the Ethiopian reptiles and amphibia, and to the many cases of the same in the South African fauna. Mr. Blandford has treated of the "African element in the fauna of India," more particularly as regards the mammalia; and the late Mr. Blyth has shown the ancient date of this relationship from the evidence afforded by the Siwâlik deposits. Mr. Murray has even inclined to the opinion that the Indo-Malayan region should be included with that of Africa, south of the Sahara.

The "Insecta" of the Ethiopian region also shows the same Oriental relationship, which seems to have hitherto received less attention. Dr. Stoliczka has described this in the "Indian Arachnoidea," and Mr. A. Murray in the coleoptera of which he has given the names of eleven genera common to the two regions.

The same thing may be seen in the Lepidoptera and Hemiptera, of which I can only treat briefly, hoping to deal with the subject in a more exhaustive and analytical form when possessed of adequate data, which at present do not exist.

Of the Lepidoptera a few specific examples will perhaps serve the purpose better than the names of the many genera that could be adduced. In the Rhopalocera:—*Danais chrysippus*, *Melanitis leda*, *Atella phalanta*, *Hypanis ilithyia*, *Lycæna telicanus*, *Idmais phisadia*, and *Callosune danæ*, all belong to the two regions, and with the exception of *C. danæ* and *I. phisadia*, have been all recorded from Madagascar. However, *D. chrysippus* (Greece and Turkey), *H. ilithyia* (Nubia, Abyssinia, and Arabia), and *L. telicanus* (Egypt and Arabia), would seem to show from those habitats their route of migration from one region to the other. In the Heterocera two examples must suffice, and may be accepted as typical of what probably occurs to a far greater extent among the large number of African moths still unknown to science. *Plusia verticillata* and *Patula macrops* have a wide range over the two areas.

In the African Hemiptera-Heteroptera the Oriental relationship is very pronounced. The following are some of the genera common to the two regions:—*Solenosthedium*, *Hotea*, *Coptosoma*, *Brachyplatys*, *Plataspis*, *Canthecona*, *Agonoscelis*, *Antestia*, *Bathycœlia*, *Catacanthus*, *Tesseratoma*, *Aspongopus*, *Phyllocephala*, *Macrina*, *Mictis*, *Leptoglossus*, *Odontopus*, *Physopelta*, *Lestomerus*, *Catamiarus*, *Pachynomus*, *Acanthaspis*, *Oncocephalus*, and *Thodelmus*. Genera, of course, are subject to constant revision and redivision, making, as a rule, generic calculations of geographical distribution very uncertain and unstable. A genus of to-day may embrace species belonging to two regions; to-morrow an author may split this genus into two, for which he may find local characters. In other words, genera common to two regions at the present time may be shown as the contrary by a later worker. In a general way the value of the term genus is often equal to the value of the term species. The twenty-four genera of Hemiptera, however, which I have enumerated above, may be accepted as more certain examples. Dr. Stål has paid particular attention to this order, and has made many genera from a minute examination of structure, and I think his divisions must at least be considered as sufficiently exhaustive. I have carefully compared my list with his latest classification, and find that eighteen out of the twenty-four genera still remain intact on his catalogue, one other is common to the two regions from an East African species I recently described, and so only five remain, which Dr. Stål has further subdivided. Of these twenty-four genera, *twenty-two extend to the West African sub-region*, twelve have at the present time also been recorded from China, and twelve from the Australian region. When we further analyse the list as to the probable route of migration, it is found that eight genera appear in Madagascar and two in Réunion; whilst a northern junction is also indicated by one genus being found in Tangier and Syria, two in Egypt, and one in Abyssinia. A few species are common to the two regions, as *Leptoglossus membrananceus*, *Oncocephalus annulipes*, &c.

It is probable that the African Neuroptera and Orthoptera may show the same affinities. W. L. DISTANT

Derwent Grove, East Dulwich

Sense in Insects—Drowned by a Devil-Fish

In the file of NATURE from October 18 to the end of November which I have just received, I find a discussion regarding the senses possessed by insects, especially the lepidoptera. For years I have been in the habit of collecting these insects for my friends, and of course have become more or less acquainted with their habits. I recall one or two instances in point. In Costa Rica the Heliconias frequent certain flowers, and pass over others of the same colour and same approximate size without noticing them. But the most marked case was of the large brilliant Morphos. My Indian servants always carried with them a fermented paste of maize flour, which they mixed with water to the consistency of gruel as a beverage. On our arriving at the side of a stream in a narrow gorge, invariably, within a few minutes after they opened a package of this paste, although there might not have been a butterfly in sight before, those most brilliant of their kind would come sailing up, always from leeward, and I have made some of my best catches in this manner. I have also caught them by baiting with a piece of over-ripe or even rotting banana. At other times they were almost unapproachable. They seem to live on fruits just merging into the state of rottenness.

I have never been able to detect any sensitiveness to sound in insects, and suspect that the case cited by one of your correspondents might be equally explained by sight, or by the vibration of the air caused by striking the glass. That certain coleoptera and diptera are attracted by smell alone is too obvious to require proof.

The same may be said of ants in following an established trail. I have experimented with this frequently, obliterating the scent for a space of but a few inches; and watching the puzzled wanderers each going an inch or less beyond his predecessor, hunting the lost clue until the blank was finally bridged over. After that if the new route as re-opened differed from the old, it was nevertheless rigidly followed even if longer and less direct.

Another matter. You mention a case of "drowning by a devil-fish" (NATURE, vol. xvii. p. 27). The story is to me very probable. I once measured a specimen of my *Octopus punctatus* caught in San Francisco harbour, which gave clear 15 feet from point to point of the arms. The animal, as I bought it from a fisherman, filled a champagne basket. W. M. GABB

Puerto Plata, Sto. Domingo, December 29, 1877

Drowned by a Devil Fish

THOUGH in British Columbia at the time of the occurrence of the incident referred to by Mr. Moseley in NATURE (vol. xvii. p. 27) I was in the interior, and consequently heard nothing of the matter. On reading Mr. Moseley's letter, however, I wrote

to my friend Dr. W. F. Tolmie, of Victoria, and have just received from him an account verifying in all essential particulars the extract quoted by Mr. Moseley from the *Weekly Oregonian*.

A party of Makaw or Makah Indians of Cape Flattery were returning from a visit to the Songish Indians of the vicinity of Victoria, and camped the first afternoon at Metchosin, on the south shore of Vancouver Island. A young woman having separated herself from the others to bathe, did not return in the evening, and after having searched for her in vain the next morning, the rest of the party were about to continue on their journey, when, on rounding the first point, they saw the body of the woman as if seated on the sandy sea-bottom, with a large octopus attached to it, which, according to the description of Dr. Tolmie's informant, resembled a "fifty-pound flour sack, full." The body was rescued in the manner described in the *Oregonian*, and when brought ashore, still had portions of the arms of the octopus adhering to it.

Dr. Tolmie also mentions the case of an Indian woman at Fort Simpson, who had, many years ago, a narrow escape from a similar death; also that among the Chimsyan Indians traditions of escapes and occasional cases of drowning exist, and further, that among these people a story is current that "A two-masted vessel manned in part or whole by men with obliquely placed eyes and wearing queues (at Milbank Sound, lat. 52°, about seventy years ago) was seized by an enormous squid, whose tentacles had to be chopped with axes ere the craft was clear of it. The ship is said to have been wrecked further south on the coast, in consequence of the evil influence of the monster."

GEORGE M. DAWSON

Geological Survey of Canada, Montreal, January 11

Eucalyptus

IN NATURE, vol. xvii. p. 10, Mr. A. Nicols says he has seen attacks of fever come on in a forest of Eucalyptus; malaria prevails there, he maintains. Does that malaria, the degree of gravity of which he does not describe, seriously compromise health? That is the question. It is probable, notwithstanding the presence of Eucalyptus, that there are yet numerous cases of fever near Lake Fetzara (Algeria), but really of such small importance as to permit, without serious danger to health, the working of the ground or the mines of these districts.

As to mosquitoes, allow me to recall that there exist very many species of these animals which, apart from their common quality of feeding on and tormenting mammals, and especially man, have origins, habitats, evolutions, and habits completely different; some live only in the larval state, others frequent moist ground, and others live, always in the larval state, in fungi. In a country which is far from being tropical and marshy, Newfoundland, the pine woods are infested during the short summer by myriads of mosquitoes, which become a real danger for the rash traveller. It will be understood that all these species do not exist at the same time in the same place, and that at Lake Fetzara the marshes are being profoundly modified, or are disappearing, and the mosquitoes, properly called, are also disappearing. Moreover, if there does not exist in the country, as is probable, any species of mosquito living in the shade of the forest, the country will be rid of these animals, a thing which cannot take place in Australia, where there are species living in the forests. In other words, it is not the Eucalyptus which at Fetzara has caused the mosquitoes to disappear, but rather the absence of the conditions necessary to the life and reproduction of mosquitoes, which have become deficient in consequence of the modification of the soil, brought about by the numerous plantations of Eucalyptus.

DR. CALMY

Saigon, December 19, 1877

Explosive Dust

IN NATURE, vol. xvii. p. 123, I noticed a letter by A. Mackennah on an explosion of malt dust in a grinding machine. This I believe to be not an uncommon occurrence, as I hear there have been three explosions in our mill within a period of four years, and these not due to any such culpable carelessness as allowing a naked flame to approach the heated impalpable dust, but ignited either by a spark from a piece of flint passing through the steel rollers (barley from some localities is invariably accompanied by quantities of small fragments of flint), or from excessive friction on some part of the wood fittings.

The following facts I obtained from the man in charge of our mill at the time of the worst of these explosions, about three years ago:—

They were grinding at the ordinary pace about mid-day with the window open and no gas turned on.

The explosion was quite sudden and the flame sufficient to singe the man's whiskers; the force was so great that the door of the engine-room was blown open, though the only opening between the two rooms was a small hole through which the shafting worked.

Having had several holes bored through the wood lining to allow a free current of air, there has been no explosion since.

The danger of fine impalpable coal dust in collieries is too manifest to need argument based on the action of analogous bodies, but still the above facts may interest some of your readers.

F. E. L.

Burton-on-Trent, January 22

Dendritic Gold

WILL one of my fellow-readers of NATURE be good enough to inform me, through its columns, with the name and publisher of such a work on mineralogy (short, if possible) as will give me the best information on the subject of the dendritic gold existing in sandstones in New Zealand, as reported in the *Proceedings* of the Wellington Society (NATURE, vol. xvi. p. 567).

It is my wish specially to know the colour of such dendrites, the geologic age of the rock containing them, and, if possible, to obtain a satisfactory account of their origin, as hitherto I have believed that metals take this form solely by deposition from solution.

I ask this in the interest of friends in South Africa (in addition to the personal desire for knowledge), where, in many parts of the Transvaal, gold "prospects" can be obtained, though usually in quantities unprofitably small, in nearly every case there being no quartz from which it could have been derived; at least so said my informants, old Australians.

Black dendrites I have noticed between the (once) horizontal strata of sandstone boulders in the Kimberley diamond mine, but was unable, at the time, to decide their nature.

R.

DEMONSTRATION OF CURRENTS ORIGINATED BY THE VOICE IN BELL'S TELEPHONE

IF two wires, A and B, be respectively connected with the two binding screws, R and S, of a telephone, and the other ends of the wires be connected with a Thompson's reflecting galvanometer, the following experiments can be made:—

1. On pressing in the iron disc a deflection is produced on the scale, say, from right to left.

2. On reversing the wires so that A is connected with S and B with R, and repeating Experiment 1, a deflection is produced in the opposite direction, *i.e.* from left to right.

3. Shouting or singing produces no deflection.

If a Lippmann's capillary electrometer be substituted for the galvanometer, the following results are obtained:—

4. If Experiments 1 and 2 be repeated, similar movements are observed, *i.e.* in one case the mercury column moves to the point of the capillary tube, in the other away from it.

5. If the gamut be loudly sung up, note by note, to the sound *ah*, one note is found to give a movement of the mercury column, about ten times as great as that observed in Experiment 4, towards the point of the tube. The octaves, especially the higher ones, and some harmonics of this note yield similar results. (It is this note which tetanises a nerve muscle preparation as observed by Fick, &c.)

6. If the wires be reversed and the same note sung, a movement of the mercury column is seen as large as that in Experiment 5, but in the *same* direction. *So that reversing the wires does not alter the direction as indicated by the electrometer.*

7. If the primary wire of a Du Bois Reymond's coil be placed in the circuit of a telephone, and the wires from the secondary circuit coupled with the electrometer, the note mentioned above produces the same movement as in Experiments 5 and 6, when the secondary coil is about

8 cm. from the primary. *Reverse the wires in the secondary circuit, reverse the wires in the primary circuit, how you please, the mercury always moves towards the point of the capillary.*

8. Shouting or singing (excepting the above-mentioned note) produces no visible effect under the conditions mentioned in Experiments 5, 6, and 7.

9. If the secondary coil be now moved close up, so as to cover as completely as possible the primary, talking to the telephone with the ordinary voice, *i.e.* with moderate strength and at any pitch, produces a definite movement of the mercury column for each word, some sounds of course giving more movement than others, *but the movement is always towards the end of the capillary.* Singing the note mentioned in Experiments 5, 6, and 7 loudly, produces a movement too large to be measured with the electrometer.

Reversing the poles of the magnet in the telephone does not alter the results of Experiments 5, 6, 7, and 9.

On mentioning the above results to Dr. Burdon Sanderson, he suggested that the apparently anomalous behaviour of the electrometer might be accounted for, by supposing that the mercury moved *quicker* when a current passed towards the point of the capillary than when it flowed in the opposite direction; so that if a succession of rapidly alternating currents be passed through the instrument, the mercury will always move towards the point of the capillary, the movement away from the point being masked by the sluggishness of the instrument in that direction. That this explanation is the correct one is proved by the following experiment:—The current from two Grove's cells is sent through a metal reed vibrating 100 times a second, the contact being made and broken at each vibration, the primary wire of a Du Bois Reymond's induction-coil is also included in the circuit; on connecting the electrometer with the secondary coil placed at an appropriate distance the mercury always moves to the point of the tube whatever be the direction of the current.

F. J. M. PAGE

Physiological Laboratory, University College, London, February 2

NOTE.—On February 4 Prof. Graham Bell kindly placed at my disposal a telephone much more powerful than any of those I had previously used. On speaking to this instrument, the electrometer being in the circuit, movements of the mercury column as considerable as those in Experiment 9 were observed.—F. J. M. P.

CHEMISTRY AND ALGEBRA

IT may not be wholly without interest to some of the readers of NATURE to be made acquainted with an analogy that has recently forcibly impressed me between branches of human knowledge apparently so dissimilar as modern chemistry and modern algebra. I have found it of great utility in explaining to non-mathematicians the nature of the investigations which algebraists are at present busily at work upon to make out the so-called *Grundformen* or irreducible forms appurtenant to binary quantics taken singly or in systems, and I have also found that it may be used as an instrument of investigation in purely algebraical inquiries. So much is this the case that I hardly ever take up Dr. Frankland's exceedingly valuable "Notes for Chemical Students," which are drawn up exclusively on the basis of Kekulé's exquisite conception of *valence*, without deriving suggestions for new researches in the theory of algebraical forms. I will confine myself to a statement of the grounds of the analogy, referring those who may feel an interest in the subject and are desirous for further information about it to a memoir which I have written upon it for the new *American Journal of Pure and Applied Mathematics*, the first number of which will appear early in February.

The analogy is between atoms and *binary* quantics exclusively.

I compare every binary quantic with a chemical atom. The number of factors (or rays, as they may be regarded by an obvious geometrical interpretation) in a binary quantic is the analogue of the number of *bonds*, or the *valence*, as it is termed, of a chemical atom.

Thus a linear form may be regarded as a monad atom, a quadratic form as a duad, a cubic form as a triad, and so on.

An invariant of a system of binary quantics of various degrees is the analogue of a chemical substance composed of atoms of corresponding *valences*. The order of such invariant in each set of coefficients is the same as the number of atoms of the corresponding *valence* in the chemical compound.

A co-variant is the analogue of an (organic or inorganic) compound radical. The orders in the several sets of coefficients corresponding, as for invariants, to the respective valences of the atoms, the free valence of the compound radical then becomes identical with the degree of the co-variant in the variables.

The weight of an invariant is identical with the number of the bonds in the chemicograph of the analogous chemical substance, and the weight of the leading term (or basic differentiant) of a co-variant is the same as the number of bonds in the chemicograph of the analogous compound radical. Every invariant and covariant thus becomes expressible by a *graph* precisely identical with a Kekuléan diagram or chemicograph. But not every chemicograph is an algebraical one. I show that by an application of the algebraical law of reciprocity every algebraical graph of a given invariant will represent the constitution in terms of the roots of a quantic of a type reciprocal to that of the given invariant of an invariant belonging to that reciprocal type. I give a rule for the geometrical multiplication of graphs, *i.e.* for constructing a *graph* to the product of in- or co-variants whose separate graphs are given. I have also ventured upon a hypothesis which, whilst in nowise interfering with existing chemicographical constructions, accounts for the seeming anomaly of the isolated existence as "monad molecules" of mercury, zinc, and arsenic—and gives a rational explanation of the "mutual saturation of bonds."

I have thus been led to see more clearly than ever I did before the existence of a common ground to the new mechanism, the new chemistry, and the new algebra. Underlying all these is the theory of pure colligation, which applies undistinguishably to the three great theories, all initiated within the last third of a century or thereabouts by Eisenstein, Kekulé, and Peaucellier.

Baltimore, January 1

J. J. SYLVESTER

PALMEN ON THE MORPHOLOGY OF THE TRACHEAL SYSTEM

DR. PALMEN, of Helsingfors, has recently published an interesting memoir on the tracheal system of insects. He observes that although the gills of certain aquatic larvæ are attached to the skin very near to the points at which the spiracles open in the mature insects, and though spiracles and gills do not co-exist in the same segment, yet the point of attachment of the gills never exactly coincides with the position of the future spiracle. Moreover, he shows that even during the larval condition, although the spiracles are not open, the structure of the stigmatic duct is present, and indeed that it opens temporarily at each moult, to permit the inner tracheal membrane to be cast, after which it closes again. In fact, then, he urges, the gills and spiracles do not correspond exactly, either in number or in position, and there can therefore be between them no genetic connection. He concludes that the insects with open tracheæ are not derived from ancestors provided with gills,

but, on the contrary, that the possession of a closed tracheal system is a secondary condition, derived from ancestors provided with spiracles.

He adopts the view that the existing insects are derived from an ancestor, in which the larvæ resembled the existing genus Campodea, with a hemimetabalous metamorphosis, and an open tracheal system; and he dwells on the important fact that in Campodea each spiracle has an independent set of tracheæ. So also in the course of embryonal development, the tracheal systems rise separately, and then the anterior and posterior branches unite to form the lateral ducts.

In a still earlier stage he thinks it probable that the tracheæ resembled those of the curious genus Peripatus. He observes that the skin-glands of certain worms secrete not only fluid, but also gas (carbonic acid), and from this to an absorbing function would be a comparatively small step. He supposes, then, that the tracheæ are derived from the skin-glands of worms, passing firstly through the stage now represented by Peripatus, in which there are a number of tracheal tubes with numerous scattered openings; secondly, though one represented now by Campodea and certain myriapods, in which the spiracles are situated in pairs, and are connected with separate tracheal systems. I. L.

ON THE EVOLUTION OF HEAT DURING MUSCULAR ACTION[1]

PROF. A. FICK, of Würzburg, in continuing his researches on the source of muscular power, has obtained some new and exceedingly important results, of which the following is a condensed account:—

It is obviously an interesting question in the physiology of muscle what fraction of the work yielded by chemical action in muscular tissue can be employed in overcoming mechanical resistance? the remainder of the chemical work appearing, in all probability, as heat.

Many years ago Helmholtz calculated, from certain considerations, into which, however, there entered several hypothetical factors, that possibly one-fifth of the total work yielded by chemical force in the human body might be employed in muscular action, the remaining four-fifths appearing as sensible heat. From this it necessarily follows that a much larger proportion than one-fifth of the work yielded by chemical force *in the muscle itself* can be employed in overcoming mechanical resistance, inasmuch as it is assumed that a great part of the oxidation takes place in other tissues, where mechanical work is quite out of the question, and where heat alone can be the result.

If, however, thermodynamical experiments show that of the chemical work going on in the muscle only a small fraction, not much exceeding one-fifth, produces mechanical effect; then, supposing the coefficient of Helmholtz to be true, it would be proved that only minute quantities of combustible material are oxidised elsewhere than in the muscles. The author's experiments have been made with a view to answer the first of the above questions—what fraction of the chemical force eliminated in the muscle is used in mechanical work? Such experiments can, of course, with the present means of research, only be carried out upon the muscles of the frog. How far the results obtained are applicable to other classes of animals, is a distinct question.

Thus two magnitudes have to be determined in absolute measure, viz., the mechanical work performed by the muscle, and secondly, the amount of chemical work that the muscle has yielded during the action.

The amount of heat produced in the muscle was of course measured by multiplying the rise in temperature of the muscle by its capacity for heat. In the calculations the specific heat of muscle was taken as equal to that of water. It cannot be greater, and is probably not much less, inasmuch as three-fourths of living muscle are water. The rise in temperature was measured by thermo-electrical means. The galvanometer used had no fixed magnet, and its constancy was proved to extend over many weeks, and even months. The thermopile had to be so arranged that it was as much as possible surrounded by the mass of muscle; its construction will be better understood after the preparation has been described. The gastrocnemius muscle, which is the favourite preparation in such experiments, was replaced by the masses of muscle which pass from the pelvis to the tibia on the inner side of each thigh, whilst the other muscles, with the sartorius and biceps, as well as both the thigh-bones, were removed. Then, on suspending the pelvis, the two prepared masses of muscle hung vertically downwards in intimate contact with each other, all the nerves belonging thereto being easily preserved. One end of the thermopile, with very flat and thin elements, was then placed in the fissure between the two masses of muscle, this arrangement being found by experience to be a perfectly trustworthy one.

[1] "Ueber die Wärmeentwickelung bei der Muskelzuckung," in the *Archiv. f. Physiologie*, Band xvi.

A remark is necessary concerning the method of irritating the preparation. Some years ago the author had the opportunity of observing, in some unpublished experiments, that an electric current of sufficient strength to produce the most powerful contraction in a muscle, does not appreciably raise the temperature of the latter. Even with Heidenhain's exceedingly delicate thermopile there was scarcely any evidence of heat being produced in a dead muscle through which a current of twenty-four Daniell's elements was passing for several seconds; and even induction currents of immense strength produced no visible thermal effect. This fact is of great interest in myothermic experiments, as it is thus no longer necessary to impart the stimulus through the nerve, but simply to subject the muscle to direct electrical irritation.

In his experiments, the author has adopted preferentially the method of direct irritation, one of the two copper wires connected with the induction-coil being attached to the pelvis, and the other to the knee of the frog.

The mechanical work was measured by connecting the preparation with one arm of a lever to which a weight was attached, and, in some of the experiments, there were also two balanced weights placed upon the lever to increase its inertia, by which it was found that the work performed was very considerably increased.

The following is a summary of the chief results arrived at by these experiments:—

1. By the interposition of a thin thermopile between suitable masses of muscle, it is possible to determine with great accuracy the absolute amount of heat produced by their contraction.

2. The determination of the muscle-temperature is not interfered with by electrical currents, which, for the purpose of irritation, are passed through the muscle. Therefore direct electrical irritation of the muscle is permissible, and indeed far preferable, in myothermic researches.

3. To the fundamental law of Heidenhain, that a muscle contracting to its greatest extent evolves more heat the greater its initial tension, we may now add that, with equal initial tension, a muscle will evolve more heat if, by means of weights in equilibrium, greater tension be produced during the contraction.

4. A muscle overcoming a greater resistance, works not only with more activity but also with more economy than when occupied in a smaller effort.

5. In an energetic muscular contraction against as great a resistance as possible the eliminated chemical force is about four times as great as the mechanical work it performs. With a less resistance the chemical is a greater multiple of the mechanical force, and with no resistance at all it is obviously indefinitely greater.

6. The amount of heat produced by the eliminated chemical force in an energetic contraction of 1 grm. of

untired frog's muscle is sufficient to raise 3 mgrm. of water from 0° to 1° C.

7. By adopting some very probable assumptions it can be inferred that the combustion of assimilated food, as far as the oxygen inspired is employed in producing chemical force, takes place almost exclusively in the muscular tissues.

P. FRANKLAND

ERNST HEINRICH WEBER

WE are called upon to chronicle the death, at Leipzig, on January 26, of Prof. Ernst Heinrich Weber, whose name is so closely united with the fundamental principles of modern optics and acoustics. He was born at Wittenberg, June 24, 1795, and after having studied at the university of that city received, in 1815, the degree of M.D. Two years later he published a short work on the anatomy of the sympathetic nerves, which brought his name at once into prominence. The following year he was appointed extraordinary professor of anatomy at the University of Leipzig, and in 1821 he became ordinary professor of human anatomy. He was early well known by his edition of Hildebrandt's "Anatomie," of which he wrote anew a considerable part in 1830. The chair of physiology was offered to him in 1840, and he actively fulfilled the duties of this position until a short time before his death. During this period he issued several manuals of physiology, and published a number of investigations, the most valuable of which are gathered together in his book "Annotationes anatomicæ et physiologicæ" (1851). Science is, however, chiefly indebted to Prof. Weber for the classical researches carried out by him and his brother Wilhelm Eduard while still young men, on which is grounded the celebrated wave-theory. The work in which their investigations are recorded—"Die Wellenlehre auf Experimente gegründet" (1825), is a remarkable relation of the most delicate and ingenious observations ever undertaken to establish a series of physical laws. Among the most notable of these might be mentioned the experiments on waves of water in mirrored troughs, by means of which they found that the particles near the surface move in circular paths, while those deeper in the liquid describe ellipses, the horizontal axes of which are longer than the vertical. By another series of comparative observations on water and mercury the law was established that waves moved with equal rapidity on the surfaces of different mediums, while the rapidity increases in both cases with the depth of the liquid. These and a multitude of other facts, studied and elaborated in the most scrupulous and conscientious manner, form the basis for the whole theoretical structure accepted at present as explanatory of the phenomena of light and sound. So thoroughly and scientifically were these researches carried out that subsequent physicists have never been called upon to correct them. In 1850 Prof. Weber completed an extensive series of experiments designed to study the wave-movement in the arterial system and explain the fact that the pulse-beat was felt at the chin a fraction of a second sooner than in the foot. The results showed that the pulse-beat travels with a rapidity of about thirty-five feet per second, and that in general the rapidity of a wave in small elastic tubes is not affected by the increase of pressure on the walls. At a later date Prof. Weber published some interesting results of experiments on the mechanism of the ear, as well as on the microscopic phenomena visible on bringing together alcohol and resin suspended in water in capillary spaces.

DR. P. BLEEKER

ON January 24 death quite suddenly overtook one of the most indefatigable workers in the field of zoological science, the well-known ichthyologist, Dr. P. Bleeker, who died at his residence in the Hague, at the age of fifty-nine. Born at Zaandam in 1819, he had an early taste for natural history, and studied medicine with a view to an appointment in the army. In 1838 he received an appointment in the medical staff of the East Indian army, and left for Batavia. Here an immense field was soon opened to his activity. He set himself to form an immense collection of fishes from different parts of the colonies, assisted in many ways by a number of his medical colleagues at different stations. He himself always remained at Batavia, gradually rising in his profession till he obtained the inspectorate of the Colonial Medical Service. At the same time he was the centre of a keen scientific movement in the capital of the Dutch Indies, starting several societies and taking the chair in the principal of them for many consecutive years. His contributions to the Indian ichthyological fauna were regularly published in Batavian scientific journals. In 1860 he returned to his native country, and first took up his residence at Leyden, with a view to comparing the treasures contained in the zoological collections there with his own. Extensive consignments of fishes had been made by him to this institution at the time of his residence in Batavia, part of the arrangement and determination of which he now took upon himself. Not long afterwards he went to live at the Hague, where the dignity of Councillor of State Extraordinary was conferred upon him. He set to work at the gigantic task he had undertaken—the publication of his "Atlas Ichthyologique des Indes Orientales Néerlandaises," seven volumes of which, illustrated by several hundreds of coloured plates have appeared. He was herein largely assisted by grants from the Colonial Government. Many important groups, the Gobioidæ, the Scombridæ, the Scorpænidæ, &c., as well as the whole of the Elasmobranchs are left unfinished. He himself estimated that little less than half of the work remained to be published, and latterly had misgivings whether he would really be able to finish it.

The number of separate publications on East Indian fishes which have appeared from his hand in different journals exceed three hundred; they form the basis on which he gradually raised the structure of his Atlas.

He had brought home his large collection of spirit specimens which has always remained in his private possession. Of late years, as he advanced with the publication of his Atlas, he disposed of the specimens of those groups which he had finished; in this way no less than 150 of his unique type-specimens were acquired by purchase by the British Museum. Another disadvantage under which a private collection of these dimensions often labours—and Bleeker's was no exception—is the loss of the exact localities from which the different specimens of one species were procured, a detail which is afterwards of such high importance in determining the geographical range of varieties. Here, however all the specimens are mixed together in one bottle without being separately labelled.

An extensive collection of reptiles and amphibians from the Archipelago, on which he had published several papers during his stay in India, have passed to the British and Hamburg Museums.

ABOUT FISHES' TAILS

MOST people know the difference in shape that there is between the tail (caudal fin) of a salmon and that of a shark; how in the former the lobes of the fin seem to be equal or symmetrical (homocercal), and in the latter only the lower lobe of the fin is, as it were, developed, and the back bone (vertebræ) of the fish seems to be prolonged into the feebly-developed upper lobe (heterocercal). This remarkable distinction was first of all recognised by Agassiz, and long ago Owen wrote, "the preponderance of heterocercal fishes in the seas of the geological epochs of our planet is very remarkable; the prolongation of the superior lobe characterises every fossil fish of the strata anterior to and including the magnesian limestone; the

homocercal fishes first appear above that formation and gradually predominate until, as in the present period, the heterocercal bony fishes are almost limited to a single ganoid genus (Lepidosteus)." "Indeed," writes Prof. Owen in another place, "it [the heterocercal] was the

Fig. 1.

fashion of tail which prevailed in fishes throughout the palæozoic and triassic periods." It never seems to have been settled whether the fish with the homocercal tail was or was not better off than the fish with the heterocercal tail. If the more recent fishes have improved in this matter of tails upon the more ancient fishes, as was to have been

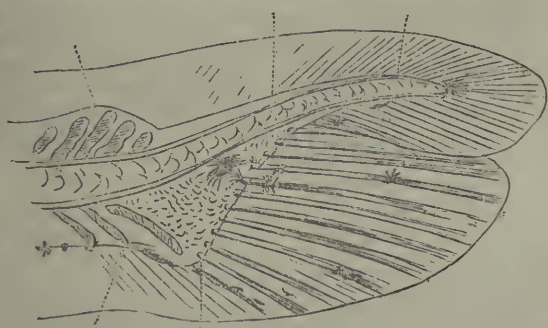

Fig. 2.

expected, certain it is that the shark of to-day can wheel quickly enough about in pursuit of his prey, and that the sword-fish can come thundering against a ship's timber with a vigour not easily matched by any fish with a symmetrical tail. Be this, however, as it may, the structure of fishes' tails has engaged the attention of most of our comparative anatomists, and the student will find large stores of facts collected and arranged for him by Agassiz, Vogt, Owen, Kölliker, Hæckel, Huxley, and Lotz. The latter four anatomists have plainly shown that while the external appearance of the tail of modern bony fishes is, as we have seen, homocercal, their real structure is only a modified heterocercal one, so that, as far as we now know, the tail of all fishes is built upon modifications of the same type, and in a paper just published by Alexander Agassiz, "On the Young Stages of some Osseous Fishes," he proves still further that this tail fin does not differ in its mode of development from the primitive embryonic fin, or from that of the back (dorsal) fin. He describes the gradual change of the

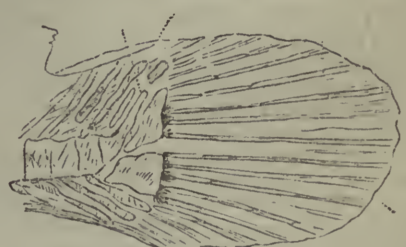

Fig. 3.

embryonic tail in several species of bony fishes, and he calls attention to the remarkable presence of an embryonic caudal lobe, which has, to this, apparently escaped the attention of naturalists, and which shows remarkably well the identity of growth between the tails of ganoid and of bony fish.

Alexander Agassiz traces the changes gradually taking place in the tail of the common flounder, from the time the little fish leaves the egg until it has nearly assumed the final shape of the adult. At first (Fig. 1) the caudal end of the chorda is straight. The caudal fin is rounded. In the next the caudal extremity of the chorda has become slightly bent upwards, and there will be found the first

Fig 4.

trace of the division line between the embryonic and the permanent caudal fins. In further stages this indentation between these two becomes more marked the chord becomes more arched, and the permanent caudal at length projects well beyond the outline of the embryonic fin fold, so that antecedent to the ossification of any of the vertebral column, the tail has assumed a heterocercal form.

In the stage (Fig. 2) in which the embryonic caudal assumes the shape of a large independent lobe, while the

permanent fin appears like a second anal fin, the resemblance to the tail of a young Lepidosteus is most striking. The extremity of the notochord at last disappears preparatory to the formation of the urostyle, while the permanent caudal gradually develops, and soon it (Fig. 3) presents the general outline of the adult form.

A. Agassiz has traced the presence of this remarkable embryonic caudal-lobe in a large number of genera of bony fish. In the young of Syngnathus it is well marked. In the young of the fishing-frog (Fig. 4) (*Lophius*) the termination of the notochord remains unchanged quite late in life, but in all the genera examined the permanent tail passes quite gradually from a strictly ventral appendage placed below the dorsal column to that of a terminal tail placed in the continuation of the vertebral column.

A. Agassiz thinks that though Agassiz and Vogt were mistaken as to their details, their great generalisation will still remain true, and that there is a complete accordance between the embryonic growth of fishes' tails and the development of fishes in time, only we must now remember that the heterocercal tail is *not* the earliest stage—that the earliest stage is a nearly symmetrical one; this which he calls the leptocardial stage is that assumed by the tails of bony as well as of all other fishes, and *precedes* the heterocercal stage. As to the palæontological record, if one examines the tails of the Devonian fish as we know them from the restorations of Agassiz, Hugh Miller, Hæckel, Huxley, and others, one is quite struck by the perfect parallelism of these ancient fishes, as far as the structure of their tail is concerned, with the structure of the stages of the flounder's tail already referred to, thus carrying out the parallelism of Agassiz and Vogt far beyond anything they even conjectured. This important paper of A. Agassiz was presented to the American Academy of Arts and Sciences in October last, and for an early copy of it we are indebted to the author.

E. PERCEVAL WRIGHT

OUR ASTRONOMICAL COLUMN

LITERATURE OF THE NEBULÆ AND CLUSTERS.—No. 311 of the *Smithsonian Miscellaneous Collections* is just received. It contains an "Index Catalogue of Books and Memoirs relating to the Nebulæ and Clusters, &c.," by Prof. Holden, of Washington, commenced in 1874 for his own use, and now published in the hope, as he states, that it may be found as useful to others as it has already been to himself. It is believed to be nearly complete so far as the uses of the astronomer can require, but it has not been Prof. Holden's object to make an index for the bibliographer. The present catalogue affords facilities in the several cases that are most likely to arise, as first, in the event of all that is published on nebulæ and clusters in a particular series—the *Philosophical Transactions*, for instance—being required; again, where all papers upon the subject by any one author are sought for, and further, when all papers written upon any special subject, no matter by what author, are in question. A very useful indication of the contents of a large number of the memoirs and notices forms a feature in the work, Sir W. Herschel's papers being noticed in abstract with particular fulness. The great nebula in Orion and the variable nebulæ claim separate sections. There are also lists of figured nebulæ and an index to Sir W. Herschel's Catalogues adopting the identifications of his son's General Catalogue.

Prof. Holden has rendered an essential service to all who may be occupied with this interesting branch of astronomy, who will find his index of the greatest assistance in enabling them to learn, at the expense comparatively of little time and trouble, all that has been written upon many special subjects and upon the nebulæ and clusters generally.

NEW SOUTHERN VARIABLE STAR.—Mr. Tebbutt—who, it will be remembered, was the discoverer of the great comet of 1861 while yet telescopic—writing from Windsor, New South Wales, on November 23, notifies his having detected what would appear to be a remarkable variable star in the constellation Ara. He had seen it as a star of the fifth magnitude while observing Comet III., 1862, between October 3 and 9; it was then brighter than σ Aræ, and plainly visible to the naked eye. Its place was fixed by sextant-distances from four stars. At the time of writing, Mr. Tebbutt mentions that the only star in the observed position was one of the eleventh magnitude, barely distinguishable in moonlight in his 4½-inch equatorial. When this star was placed in the centre of a field of about 45′, no stars above the tenth magnitude were visible. But, in this case, what has become of No. 6142 of the Paramatta Catalogue, rated 7·8 m.? Mr. Tebbutt found the place of his star for 1878·0, R.A. 17h. 30m. 13s·2, N.P.D. 135° 24′ 17″, in which case Brisbane's star would be distant 16′·8 on an angle of 193°, and should therefore have been in the field.

While writing on the subject of variable stars, we may mention that the *Annuaire du Bureau des Longitudes* for 1878 contains very full lists and ephemerides of these objects, which have been ably prepared from Prof. Schönfeld's catalogue and other sources by M. Lœwy, who now has charge of the popular French work. In other respects the *Annuaire* for the present year is to be recommended as a valuable repertory of scientific facts and data.

THE ROYAL OBSERVATORY, BRUSSELS.—M. Houzeau, the successor of the late M. Quetelet in the direction of this establishment, has issued his report on the work of the year 1877. The Observatory is at present in a transition state, the instruments which have long been in use being about to be replaced by others of greater capacity. A meridian circle, almost entirely similar to that constructed for the new Observatory at Strasburg, has been ordered from Repsold; and Dent, of London, supplies the standard sidereal clock, to be accompanied by a chronograph: various modifications have been introduced into these instruments after careful consideration. A refractor of 38 centimetres aperture is in course of construction by Merz, the object-glass having already arrived at Brussels. The ancient meridian instruments have been employed on the observation of stars exhibiting decided proper motion, a work long pursued. On the mounting of the large refractor, M. Houzeau proposes to fix his attention upon three principal objects:—1st. Micrometrical measures of a certain number of double stars—binaries, and those which are affected with rapid proper motion. 2nd. To observe, with particular care, the passages of the satellites of Jupiter across his disc, and their occultations and the transits of their shadows. 3rd. Spectroscopic observations, for which a smaller refractor will also be available. Meteorological observations which have occupied much of the time of the observers during M. Quetelet's superintendence, will be continued, but in a department distinct from that devoted to astronomy, a very necessary arrangement if observations of a routine nature are not to be allowed to interfere with those of a higher class.

A FORECAST OF THE SATELLITES OF MARS.—In the last number of the *Astronomische Nachrichten*, Prof. von Oppolzer, of Vienna, draws attention to the curious passage in the "Travels into Several Remote Nations of the World by Lemuel Gulliver"—of Swift, which he transcribes from the edition of 1755. A correspondent of the *Times* referred to the same passage soon after the discovery of the satellites of Mars by Prof. Asaph Hall became known in this country. We read "they have likewise discovered two lesser stars or satellites which revolve about Mars, whereof the innermost is distant from the centre of the primary planet exactly three of his diameters, and the outermost five; the former revolves in the space of ten

hours, and the latter in twenty-one and a half; so that the squares of their periodical times are very near in the same proportion with the cubes of their distances from the centre of Mars, which evidently shows them to be governed by the same law of gravitation that influences the other heavenly bodies." This idea of Swift's, which appears to have only recently come to the knowledge of Prof. v. Oppolzer, is so singular a one taken in connection with the facts of the discovery of the satellites of Mars, that it is not surprising the editor of the *Astronomische Nachrichten* should have transferred it to his columns. Possibly the opinion which has prevailed largely amongst astronomers that, if satellites of Mars existed, they must be very small and close to his disc, may have had originally some connection with Swift's fancy.

BIOLOGICAL NOTES

PAPUAN PLANTS.—In the Appendix to Baron von Mueller's "Descriptive Notes on Papuan Plants," which we have just received we find some interesting additions to orders already considered, and which we have had occasion to refer to before. In Leguminosæ, *Acacia holosericea* is recorded from Geelvink Bay, found by Beccari; from the Fly River, by D'Albertis; and Baxter's River, by Reedy. In Myrtaceæ are four additions—*Tristania macrosperma*, *Myrtella beccarii*, *M. hirsutula*, and *Bæckea frutescens*. A remarkable myrtaceous plant, with the habit of a *Psidium*, is stated to be contained in Dr. Beccari's collection, which Baron Mueller thinks is probably referable to the genus *Eugenia*. The only flower available for examination had eight petals, being double the number of the calyx lobes. Unless this augmentation arose from monstrous growth we are reminded that we have here a species abnormal not only in the genus *Eugenia* (and to which the name of *E. pleiopetala* might be given), but also in the whole order of Myrtaceæ, except *Gustavia*. From Mount Arfak, at an elevation of about 6,000 feet Dr. Beccari obtained the first epacrideous plant recorded from New Guinea, though in all likelihood others will yet be detected in the higher mountain regions.

HORSE-SHOE CRABS.—With reference to the fact that large numbers of trilobites are found on their back, and the inference that when living they probably swam in this position, Mr. Alex. Agassiz states (*Sillimann's Journal*) that he has for several summers kept young Limuli (horse-shoe crabs) in his jars, and has noticed that besides often swimming on their backs, they will remain in a similar position for hours, perfectly quiet, at the bottom. When they cast their skin it invariably keeps the same attitude on the bottom of the jar. It is not uncommon to find on the shores, where Limuli abounds, hundreds of skins thrown up and left dry by the tide, most of which are turned on their backs. Again, young Limuli generally turn on their back while feeding. Moving at an angle with the bottom, the hind extremity raised, they throw out their feet beyond the anterior edge of the carapace, browsing, as it were, on what they find in their road, and whisking away what they do not need by means of a powerful current produced by their abdominal appendages.

GREEN ALGÆ.—Our knowledge of the life-history of those green-coloured algæ which seem to possess a true reproductive system, is progressing with rapid strides, and in the *Botanische Zeitung* for October and November last two most remarkable papers on two well known (so far as external form goes) species have very considerably advanced our knowledge of the group. The earlier in date (October) of these two memoirs is by the well-known botanists Rostafinski and Woronin on *Botrydium granulatum*. This alga was described by Ray nearly 200 years ago, and is probably known to many as growing up in damp clayey spots, and presenting the appearance of bright green blobs about the size of large mustard seeds. Common as this plant is, it is only now that after several years' consecutive watching the authors have been able to clear up the mystery of its life, and to determine that the formation of ordinary zoospores can eventuate in the four following ways (*a*) from the vegetative plant, (*b*) from an ordinary zoosporangium, (*c*) from the root-cells, and (*d*) from a Hypnosporangium: and as still further means of increase we have (*e*) cell division, (*f*) formation of spores, and (*g*) formation of isospores. Botrydium would also seem to enjoy a five-fold resting state: 1. The asexual aquatic zoospores with a quiescence of one month. 2. The root-cells, quiescence the year through in which they are formed. 3. The hypnosporangia, quiescence the same. 4. The spores, quiescence a year. 5. The isospores, quiescence at least over the year in which they were formed. The next memoir is a joint one by A. de Bary, the able editor of the journal, and E. Strasburger, and is about that very beautiful green seaweed not uncommon in the Mediterranean, called *Acetabularia mediterranea*. This genus was so called by Lamouroux on account of the saucer (acetabulum) like form assumed by the little rows of filaments that crown the cylindrical stalks. There are three species known, perhaps they may be all varieties of the one now referred to. Prof. de Bary was only enabled to watch the progress of the spore development to a certain stage, but by Strasburger's researches, carried on at Spezia, we are enabled to read the whole history and to know that the motile bodies of protoplasm set free from a mother-cell, can and do conjugate, forming a resting body which can and does vegetate. At the close of this memoir Strasburger proposes that we should call the body formed by the conjugation of the contents of two cells (Gametæ) by the name of Zygote, and that those plants whose Gametæ are active might be called Planogameta, and those where (as in Desmids) the Gameta are at least quasipassive, might be called *Aplanogameta*.

DEEP SEA ASCIDIANS.—Mr. H. N. Moseley has published (*Transactions*, Linnean Soc. S.S. Zool., vol. 1) a description, accompanied by excellent figures, of two very remarkable forms of ascidians. The first described was obtained from the great depth of 2,900 fathoms in the North Pacific Ocean, and is called *Hypobythius calycodes* in allusion to its occurrence at so vast a depth and to its cup-like form. Its outer skin is hyaline and extremely transparent, but in certain places it is strengthened by the presence of tough cartilaginous plates, and these are arranged in a nearly symmetrical manner. It is attached by means of a stalk. It is probably allied to the genus Boltenia, but is abundantly distinct from all known forms. The second is a beautiful stellate form taken 1070 fathoms, not far from one of the Schouten Islands. From its having eight long radiating processes it was at first taken for a medusoid form. Its test is hyaline and gelatinous and it is also an attached form, but the stalk is short. The respiratory sac is flattened out so as to become nearly horizontal, and there is no gill net-work present. It has been called *Octacnemus bythius*.

THE BYSSUS IN THE MUSSEL.—Tycho Tulberg has published in the *Transactions* of the Royal Society of Natural History of Upsala (July, 1877) an account of the structure of the byssus-forming gland in *Mytilus edulis*. The strong silky threads formed by this gland, which moor the mussel shell so firmly to its resting-place, must be familiar to most. In an allied genus (Pinna) these threads have been even spun and formed into gloves. The manner in which the tongue-like foot can affix these threads is easily to be seen by watching a small specimen of the common mussel when in a healthy condition and confined in a glass jar. The minute structure of the gland that secretes the threads is well described by Mr. Tulberg, who promises further to publish an account of

the structure of this organ in some other species of byssus-forming bivalves.

AQUATIC RESPIRATION.—Some experiments on the breathing of aquatic animals (both fresh and salt water) have been recently described by MM. Jolyet and Regnard in the *Archives de Physiologie.* The results are briefly as follows :—These animals, living in a medium very poor in oxygen, and having a blood-liquid with small respiratory capacity, have the least vigorous respiration. In the free natural act of respiration the oxygen which disappears is not exactly represented by the oxygen in the carbonic acid produced; the ratio $\frac{CO_2}{O}$ is always less than 1; *i.e.*, aquatic animals, in the normal state, never give off more carbonic acid than the oxygen they absorb. (The opposite result got by some physiologists is attributed to keeping the animals in an enclosed medium whose oxygen they gradually exhausted.) As with other animals, heat-variations in the surrounding medium has a marked influence on the chemical phenomena of respiration. Taking 2° and 30° as the limits of bearable external temperature, the quantities of the absorbed oxygen vary (other conditions being equal) in the ratio of 1 to 10. Among other causes which may have an influence on the vigour of breathing (apart from those connected with species) the most important, after temperature, are the state of hunger and digestion, the amount, and the greater or less intensity of muscular action. In the experiments there was sometimes a slight development of nitrogen, sometimes an absorption. No definite opinion could be expressed with reference to this point.

GEOGRAPHICAL NOTES

EXPLORING COLONIES.—The Société des Colons Explorateurs, lately organised in Paris, is developing a most healthful degree of activity. Its purpose is to organise a systematic method of exploration and colonisation, based on the same principles as those which animate the newly-founded international society for the exploration and civilisation of Africa, but embracing in its field all the undeveloped portions of the globe. The Society has formed two councils to direct its operations. In the first, which is charged with the scientific, geographical, and exploratory sections, we notice the names of Malte-Brun, de Lesseps, de Quatrefages, Milne-Edwards, Admiral La Roncière le Noury, &c. The second, devoted more especially to agriculture, commerce, and industry, embraces Michael Chevalier, Tisserand, Col. Solignac, F. Garnier, and other well-known names. The plan adopted by the Society for the attainment of its objects is eminently practical. A colony is formed from representatives of various classes and occupations, who are well fitted to investigate and develop the resources of a new country; it is provided with a complete equipment, and despatched to a promising locality. Here a firm foothold is established, and the new settlement made, as soon as possible, not only self-supporting, but a centre for geographical and general scientific investigation. The band of permanent colonists are accompanied by a certain number, who, after obtaining a degree of familiarity with the difficulties to be overcome in a new settlement, are ready to form the nucleus of a new colony. In this manner not only will the various colonies increase the sphere of their activities at a rapid rate, but drill at the same time groups of hardy explorers well fitted to extend the circle of the Society's undertakings. The first experimental colony has already been started on the coast of Sumatra, and embraces in its *personnel* graduates of the leading technical and professional schools of Paris. If this simple practical programme is carried out successfully, it is evident that the new Society, increasing the extent of its operations in arithmetical progression, will soon become a most important factor in the slow process of civilising the world.

SUMATRA.—In the January session of the Dutch Geographical Society it was announced that Lieut. Cornelissen had been appointed to take charge of the Sumatra exploring expedition, lately deprived by death of its commander, M. Schow-Sandvoort. He leaves in March to assume the direction of the explorations. During the past three months 14,000 guilders have been contributed for the Sumatra exploring fund.

NIAS ISLAND.—In Petermann's *Mittheilungen* for February is a very full account, with map, of the Island of Nias, on the west of Sumatra, by Dr. A. Schreiber. The island now belongs to the Dutch, and by them has in recent years been pretty thoroughly explored. The island is hilly, the highest summit being 2,000 feet, the formation being mostly sandstone and coral.

ARCTIC EXPLORATION.—Admiral La Roncière le Noury in his capacity of president of the Paris Geographical Society, M. Quatrefages, and M. Maunoir, general secretary, have written an official letter to Capt. Howgate, U.S.A., conveying to him their approbation of his scheme for establishing a polar colony in Lady Franklin Bay. They trust this document may induce the Congress to vote the required credit for starting the contemplated expedition. They express, moreover, their gratitude for the sending out of Capt. Tyson's preliminary expedition, and they trust Capt. Howgate will soon be in a position to take advantage of the means which his hardy lieutenant has been sent to collect. Capt. Howgate has written to the Danish Government, asking them to send instructions to the Disco authorities, authorising them to place the Government storehouse at the disposal of Capt. Tyson, if he has failed in collecting a sufficient number of furs during the present winter season. Mr. S. R. Van Campen has been asked by the Hon. B. A. Willis, of the Committee on Naval Affairs in the United States Congress for a report on the Arctic expeditions abroad, and has complied with the request. Besides speaking particularly of the proposed expeditions of Holland and Sweden, Mr. Van Campen suggests to the Committee, as it has in charge the bill now before Congress for an American expedition, proposed in accordance with Capt. Howgate's scheme, the incorporation of a clause granting rewards upon a graduated scale to individual explorers of whatever nationality, who may reach latitudes or make discoveries in Arctic territory beyond points hitherto attained.

ROHLFS' EXPEDITION.—Herr Gerhard Rohlfs has received no less than 300 applications for participation in his expedition to the Libyan Desert. Of course the great traveller can only consider very few of them. We learn further that he intends also to explore the Shari, Binue, and Ogowai Rivers and their tributaries. We hope he will succeed in accomplishing this, as it will solve many of the questions raised by Stanley's discovery of the course of the Congo. The date of his departure is not yet fixed.

SPITZBERGEN.—A very interesting series of nine maps of Spitzbergen, partly rare and little known, are published in the *Tijdschrift* of the Amsterdam Geographical Society, with an essay by Capt. de Bas, on the geographical names of Spitzbergen. The maps begin with that of Barentz's third voyage of 1596, followed by those of Gerritz, 1612; Edge, 1625; Middlehoven, 1634; Daniel, 1642; two others of 1648, and the latter half of the seventeenth century; that of Johannes van Keulen, 1710, and finally the Dunér-Nordenskjöld map of 1864.

JAPAN.—In the *Monatsbericht* of Petermann's *Mittheilungen* for February, Dr. Behm gives some information concerning recent geographical work in Japan. There is an itinerary by Dr. Schulz, of a journey he made in

August, 1877, from Tokio to Hatsuishi (Nikko), and from Nikko to Takasaki; an account of the observations made by Dr. Naumann last summer during a journey into the little known western part of Nippon, and another at the same time into the north of that island by Herr Gebauer; and some information from the *Tokio Times*, by Y. Watanabe, on Chikuzen, a province in the north-west of the Kiushiu.

NOTES

THE following grants have just been made from the Research Fund of the Chemical Society to aid the carrying out of the following researches:—50*l.* to Dr. Wright, of St. Mary's Hospital Medical School for the continuation of his researches in chemical dynamics; 25*l.* to Dr. Armstrong for an investigation of camphor and allied compounds; 20*l.* to Dr. Carnelly, of Owens College, Manchester, for a research on the hydrocarbons diphenyl, ditolyl, &c., and their derivatives; 10*l.* to Mr. P. Phillips Bedson, of Owens College, Manchester, for a research on derivatives of phenyl acetic acid, and on the constitution of isatin; and 5*l.* to Mr. J. R. Crow, of Owens College Manchester, for a research on the action of zinc ethyl on the chloride of vanadium.

WE regret to announce the death, at Nice, of the celebrated Danish conchologist, Dr. A. L. Mörch.

WE hear with great regret, from an Italian correspondent, that the well-known astronomer, Father Secchi, has been seriously ill for several weeks, and that little hope is entertained of his recovery. The Roman correspondent of the *Lancet* states, however, that Dr. Ceccarelli, who is attending him, does not absolutely despair of his recovery. Father Secchi is not yet sixty, and is of robust frame.

AT the General Monthly Meeting of the Royal Institution on Monday, Dr. Warren De la Rue, D.C.L., F.R.S., in the chair, the special thanks of the members were given to Mr. William Bowman, F.R.S., for his present of an ivory bust of Prof. Faraday, by the late Matthew Noble, M.R.I. In reference to the telephone which Mr. Preece had explained to the members last Friday, the chairman stated that he had made attempts to measure the current produced by the vibrations of the disc of iron in front of the magnet of the telephone, and that he was unable to detect any by means of a most sensitive dynamometer which would render evident the current of a Daniell's cell through 2,000 ohms. Moreover, by other experiments made by other means, he concluded that the current produced did not amount to that which a Daniell's cell would send working through 100,000,000 ohm resistance.

THE New York *Tribune* gives an account of a public exhibition in that city of Eddison's Phonograph, which seems to have been very successful. The tones reproduced by the vibrating disk of the machine were so distinct that they could be heard and understood in different portions of the crowded room. Words spoken in a high key and with forcible emphasis were reproduced with much greater distinctness than those spoken in a low tone, even when the latter were uttered very loudly. A difference in the sound of different voices could be easily discerned. Several fragments of songs were sung in a high key and repeated by the machine with wonderful fidelity. The inventor stated that the machine has yet to be perfected before its full power is developed, and that ultimately it can be used to receive and reproduce the songs of popular singers as they are rendered on the stage.

AT a recent meeting of the Royal Society of Edinburgh, in connection with a letter from New York describing the phonograph, Sir William Thomson gave some explanation of the machine. All previous attempts to record sound were, he said, founded on the motion of a style or marker at a true parallel to the paper. Mr. Eddison's ingenious invention of the electric pen was different. It consisted of a fine point, which, by an excessively rapid vibration perpendicular to the paper, caused by a small electric machine connected with two thin wires to the point, left a trace of any person's handwriting in a row of very fine holes, from which the handwriting could be printed. Mr. Eddison, from this invention, elaborated the phonograph. By the greater or less pressure produced through the action of the alternate condensation and expansion of the air caused by the mechanism of the voice, the diaphragm operated upon the point and recorded the sounds. It was the most interesting mechanical and scientific invention they had heard of in this century. There could be no limit to its application. A man could speak a letter through the phonograph—it would be recorded on tinfoil, sent in an envelope through the post, and his friend, by applying the point of the phonograph to the tinfoil, could reproduce the words and tones uttered. In fact they could take down the singing of a Titiens (had we one), which might be reproduced to a tone two hundred years hence.

BOTH Houses of Legislature have unanimously passed a resolution giving the thanks of the U.S. Congress to Mr. Henry M. Stanley for his achievements in the field of African exploration. Mr. Stanley meets the Geographical Society in St. James's Hall to-night. It would be interesting to know how many applicants beyond the 2,000, which the hall will hold, have been disappointed. The officials of the Society have had a trying time of it in attending to the loads of applications they have received. Mr. Stanley will be entertained at dinner by the Society on Saturday.

IN connection with the recent election of Prof. Simon Newcomb as a foreign member of the Royal Society, it was stated that previous to that Prof. Asa Gray was the only living American who enjoyed that honour. We find, however, among the list of foreign members the name also of Prof. Benj. F. Peirce, of Cambridge, Mass.

WE have received an interesting volume: "Estudios sobre la flora y fauna de Venezuela," by A. Ernst. The author, in two ably written articles, gives a general idea of each of the two large kingdoms as they appear in Venezuela, and further adds some details on the fungi, orchids, molluscs, and birds of that country. The book is published at Caracas, and consists of over 100 quarto pages.

THE first meeting of the Institute of Chemistry of Great Britain and Ireland was held on Friday afternoon at the rooms of the Chemical Society, Burlington House. Prof. Frankland, F.R.S., the first President, read an address in which he gave an account of the origin of the institute. At a dinner given to Prof. Canizzaro on the occasion of his visit to London in May, 1872, Prof. Frankland drew attention to the increasing importance of chemistry in relation to the wants of communities, and suggested the usefulness of an institute that should be to chemists what the Colleges of Physicians and Surgeons are to the medical profession, the Institute of Civil Engineers is for civil engineers, and the Inns of Court are to the legal profession. Although the need of experts in connection with water and gas analysis, legal proceedings, and nuisances was recognised, and the application of chemistry to agriculture and manufactures was known to be of great importance, the suggestion was not taken up in a practical way until the beginning of 1876, when a meeting to consider the subject was held at the rooms of the Chemical Society on April 26. A committee was appointed to draw up a scheme for the constitution of the institute, which was laid before a meeting held in November. At one time it was thought that the objects might be effected by

establishing a separate branch of the Chemical Society with the Fellows of the Chemical Society. After much discussion the formation of the present institute was decided on. The institute has power to appoint examiners as to the fitness of candidates for its membership. Prof. Frankland, in the course of his address, drew attention to the fact that under the Pharmacy Act of 1868 no one, not even the President of the Chemical Society, may call himself a chemist unless he is duly registered as a pharmaceutical chemist. There are already 225 members and 142 associates, and a fund of over 1,000*l.* for the new institute.

THE Naturwissenschaftliche Gesellschaft of Jena celebrated the twenty-fifth anniversary of its foundation on January 17 last. Upon that occasion Mr. Charles Darwin, Prof. M. I. Schleiden, of Wiesbaden, and Prof. Oscar Schmidt, of Strasburg, were named honorary members of the society.

FURTHER information shows that the earthquake of Monday, January 28, was felt at several places in London, at Ryde, Osborne, Southampton, and Lyme Regis. Shocks were felt in Neumarkt at 10 A.M. on the 27th and 5 A.M. on the 28th. At Judenburg (Upper Styria) two different shocks were felt on the 27th, at 10.6 A.M., and on the following day at 4.32 A.M. At Waldshut, on the Rhine, in the Grand Duchy of Baden, an earthquake was felt on the evening of January 16 shortly before midnight. The shock lasted about a second and seemed to proceed in the direction from south-west to north-east. Subterranean noise was plainly audible. The same phenomenon was simultaneously observed at Alb, Karsau, Beuggen, Schopfheim, and other places in Baden, as well as in all the north-westerly cantons of Switzerland.

THE recent investigations of Sergius Kern, resulting in the discovery of davyum to which we have had occasion frequently to refer, are being submitted to a careful examination in the Heidelberg laboratory under the direction of Prof. Bunsen. The results so far coincide with those of the Russian chemist, and it is to be hoped that the entire research may stand the crucial test of the leading authority on the platinum metals.

M. PICTET delivered, during the past week, two very interesting addresses in the laboratory of the École de Médecine, before the chemists of Paris, in which he gave a very complete and detailed description of his late experiments on the liquefaction of gases. He is a young man of scarcely thirty, an easy and fluent speaker, and made a pleasant impression on his Parisian auditory. A *brochure* of 100 pages, which he has just issued, with drawings, gives a very elaborate description of the whole series of experiments on the compression of gases.

THE Hon. Rollo Russell sends us some notes on experiments he has made which go to prove that there is no need to insulate the wires connecting a pair of telephones, at least when used for short distances. No. 18 uncovered copper wire was laid along grass and trees 418 yards, the two lines being kept well apart. Articulation and a small musical box were very well heard. The same wire buried for three yards in wet clay, the lines being about 5 ft. apart and the telephones 20 yards apart, gave good results, and it appears that the bare wires may be taken under roads, &c., without diminution of the audible effect. With the same wire taken across a pond, the lines being submerged in water about 40 yards, and lying on the grass the rest of the distance about 28 yards—the wires were about a yard apart in the water—conversation in low tones was distinctly heard when not overpowered by the noise of a strong wind blowing at the time. Probably No. 18 copper wire, uninsulated, might be laid across rivers and straits and used for telephonic purposes without appreciable loss of sound, as Mr. Russell, not in any of the above cases, noticed a feebler effect than with insulated wires.

INTERESTING antiquities have recently been excavated at Neumagen on the Moselle. The Roman poet Ausonius mentions in his "Mosella" that the Emperor Constantine possessed there a "beautiful castle," which was doubtless destroyed about the middle of the fifth century when Treves was several times ravaged by the Franks. About a century afterwards the famous castle of Nicetia was built by the Archbishop Nicetius, who probably utilised the foundations of the old Roman structure. Nicetia was rased to the ground in the year 881 by the Normans. Many of the old foundations are now being again excavated and are tolerably well preserved; the materials of which they are constructed are sandstone, marble, and limestone.

M. MARIÉ DAVY has published, through Gauthier Villars, the Montsouris Observatory's Meteorological *Annuaire.* The volume contains a number of important improvements.

THE first ordinary meeting of the newly-established Physical and Chemical Section of the Bristol Naturalists' Society was held on January 22 in the Library of the Bristol Museum. A paper was read by Mr. W. W. Stoddart, F.C.S., F.G.S., "On a Remarkable Occurrence of Indican in the Human Body." A paper was then read by Mr. S. P. Thompson, B.Sc., B.A., of University College, Bristol, "On Vortex Motion in Liquids." The paper was illustrated by experiments showing the production of smoke rings in air and of rings of coloured liquid in water. The author had lately tried the action of electro-magnetism upon the rings projected through water and had observed their retardation and partial destruction in passing through a powerful magnetic field. His experiments are at present incomplete.

THE third volume of C. L. Michelet's "System der Philosophie als exacter Wissenschaft" (Berlin: Nicolai) will shortly be published. It will contain the philosophy of the mind. The fourth volume will treat of the philosophy of history and will close the interesting and elaborate work.

EXPERIMENTS with a new telegraph apparatus have lately been made at Vienna, by means of which some 100 or 120 messages may be sent by a single wire in the remarkably short space of one hour. Under certain conditions this number may even be raised to 200 or even 250 messages. The inventor of the new apparatus is Herr August Eduard Granfeld, an Austrian telegraph official. At the end of December he presented to the Austrian "Telegraphenanstalt" eight working and two principal apparatus of his invention for practical trials. The experiments were crowned with complete success.

A NEW watchman-controlling clock has been constructed by Messrs. Fein at their telegraph works at Stuttgart, which on a single dial records the times at which a watchman visits any given number of stations however far apart, as well as the succession in which they are visited, and thus also the intervals which elapse while the man is proceeding from place to place. The same firm has constructed an automatic alarum for uninhabited or locked localities.

IT is stated that such enormous quantities of snow are now lying in the Austrian "Salzkammergut" as have not been seen there for the last fifty or sixty years, and a sudden thaw is dreaded extremely, as it would unavoidably cause enormous inundations. News from Pesth reports that on January 27 the Danube broke through the dykes at Dömsöd, and caused a vast inundation in Kumania, for a distance of some fifty miles, as far as Baja. Nine villages are under water. Other inundations are reported from the valley of the Vesdre River in the eastern part of Belgium.

AN exceptionally mild winter is reported from the north-western states of North America. In the districts near St. Paul, Minnesota, the farmers ploughed their fields in Christmas

week. On Christmas Day excursions were made by steamer on the Mississippi River. In former years the river was generally frozen over on that day.

THE Vienna Society for the Protection of Animals offers a prize of thirty ducats in gold for the best pamphlet recommending the protection of animals. The little work must be of general interest and must be written specially for teachers. It must be in the German language and is not to exceed six sheets in print. Competitors must send in their manuscripts, on or before July 1 next, to the Committee of the Society at Vienna (Johannesgasse, 4).

IN the *Geographical Magazine* for January and February will be found Language Maps of India and Further India, including the Indian Archipelago, with accompanying text, by Mr. Robert Cust. Mr. Cust announces that he is collecting materials for a language map of Africa. Such a map already exists in Stanford's "Compendium of Geography—Africa," constructed by Mr. A. H. Keane, who, besides, gives there material for such a map to which, we should think, it would be scarcely possible to add. Is not Mr. Cust's work one of supererogation?

THE *Geographical Magazine* or February contains a curious and interesting autobiography of an Eskimo, Hans Hendrik, who served in the Arctic expeditions of Kane, Hayes, Hall, and Sir George Nares. It was written in Eskimo and translated by Dr. Henry Rink, who writes an introduction.

ETHNOLOGISTS will be interested in a paper in the February number of the *Geographical Magazine*, by Fr. A. de Röepstorff, on the inland tribe of the Great Nicobar. The author concludes that this tribe is certainly not Negrito, the specimen he saw having Mongolian characteristics.

THE mathematical reader will peruse with interest the eleventh number of the *Bulletin* of the Belgian Academy of Sciences (vol. xliv.), where he will find a paper by M. Ghysens, on the determination of volumes and superifices, being the application of an ingenious and new general formula to several difficult and interesting problems; an interesting note by Prof. Catalan, on a new principle of subjective probabilities; and a first paper by Prof. Folie, on the extension of the notion of the anharmonic relation.

THE movements of sediments in the sea it has been common to regard as exclusively an effect of wave-motion. M. Fuchs has recently pointed out that while this is an obvious cause, it is not the only one. Another factor (and one which is probably more powerful in its action), consists in the accumulations which the water undergoes periodically, partly through the flood-tide, partly through winds prevailing on the coasts. Suppose the sea on a coast heaped up ten to thirty feet (and this is not uncommon), the hydrostatic equilibrium must be thereby greatly disturbed, and a current must arise in the depths from the point of greater to that of less pressure, *i.e.*, from the coast to the deeper parts. If a calculation be made of the excess of weight caused by such accumulations of water, such enormous sums are obtained that it is easy to see how the current generated will be strong enough to move not only fine detritus, but large blocks, towards the depths.

THE additions to the Zoological Society's Gardens during the past week include a Banksian Cockatoo (*Calyptorhynchus banksii*) from New South Wales, presented by the Lady Ellesmere; a Common Badger (*Meles taxus*) from Scotland, presented by Lord Saltoun; a Brown Bear (*Ursus arctos*) from North Europe, presented by Mr. J. N. Allen; a Yaguarondi Cat (*Felis yaguarondi*), two Yarrell's Curassows (*Crax yarrelli*), two White-bellied Guans (*Ortalide albiventris*), a White-fronted Guan (*Penelope jacucaca*), a Common Trumpeter (*Psophia crepitans*), a Sun Bittern (*Eurypyga helias*), an American Kestrel (*Tinnunculus sparverius*), all from South America, purchased.

AMERICAN SCIENCE

THE eighth paper of Prof. Loomis' interesting series of "Contributions to Meteorology (*American Journal of Science and Arts* for January, 1878), treats of the origin and development of storms, violent winds, and barometric gradient, the data being obtained from the United States Signal Service observations. Of forty-four different storms recorded between September, 1872, and May, 1874, twenty-one (nearly a half) appear to have originated on or very near the chain of the Rocky Mountains (the others were of various origin). More than two-thirds of the whole originated north of latitude 36°. (We refer to this subject elsewhere.)

This number of the journal also contains some observations by Capt. Belknap, of the *Tuscarora* (during her cruise in the Pacific) proving once more that a cold stratum may exist in the ocean between two warmer ones above and below. The case occurred off the Kurile Islands, between 49° and 52° N. lat. and 158° and 167° E. long. The upper part of the stratum in one place, showing a temperature of 33°·7 F., was only twenty fathoms below the surface, while at ten fathoms below the surface the temperature was 41°. At a depth of 100 fathoms the temperature was 32°; below that curve to a depth of 200 fathoms the range of temperature was from 34°·5 to 38°·7. The width of the cold stratum gradually narrowed to a point in an easterly direction from the coast, or as the edge of the Japan stream was approached. (Several data are furnished regarding the currents in that region.)

An able revision of the atomic weight of antimony has lately been carried out by Mr. Josiah P. Cooke, jun., and the first portion of his paper to the American Academy on the subject is here given in abstract. A new mineral, pyrophosphorite, an anhydrous pyrophosphate of lime from the West Indies, is described by Prof. Shephard, jun. Prof. Rockwood furnishes notices of some recent American earthquakes; and Maria Mitchell, observations on Jupiter and his satellites, with the equatorial telescope at the observatory of Vassar College. Attention may also be called to a summary of the field work of the United States Geological and Geographical Survey of the Territories, under the charge of Dr. Hayden, for the season of 1877. The surveys in Colorado having been completed during the previous year, the parties prosecuted their work in a belt of country lying mainly in the western half of Wyoming, but also embracing adjacent portions of Utah and Idaho. Among other important results, Dr. White has demonstrated the identity of the lignitic series of strata east of the Rocky Mountains in Colorado with the Fort Union group of the Upper Missouri River, and with the great Laramie group of the Green River basin and other portions of the region west of the Rocky Mountains. The botany of the Survey was represented (it is known) by Sir Joseph Hooker and Prof. Asa Gray. Mr. Jackson has visited the strange ruins found in Northern New Mexico and Arizona, and procured the necessary data for plastic representation of the pueblos, or communal town dwellings, of Taos and Acorna, models of which he has constructed. Contact with Europeans has somewhat modified their ancient style of building, but one can readily see that they are constructed after their ancient prototypes, the dwellings of the forgotten people; forgotten, because the builders of the modern structures are as ignorant of the ancient builders as we are ourselves.

The first number of the *American Journal of Mathematics* will be published early this month, with contributions by Prof. Simon Newcomb, Mr. G. W. Hill, Mr. H. T. Eddy, Cincinnati, O., Dr. Guido Weichold, Zittau, Saxony, Prof. Cayley, Mr. H. A. Rowland, Prof. Charles S. Peirce, Prof. Sylvester, and Mr. William E. Story.

We recently announced that the *American Naturalist* has been removed to Philadelphia for publication under the management of Prof. Cope. This, with other new conditions, has given dissatisfaction to a number of the old contributors. This dissatisfaction has taken definite form and is expressed in a circular as follows:—"The undersigned, who have in past years contributed articles and by other means helped to support the *American Naturalist*, protest against the continued use of their names in the same connection under the new conditions advertised in the December number of 1877." The circular is signed by Profs. Agassiz, Gray, Whitney, Hagen, Shaler, Allen, Farlow, Dana, Marsh, Verrill, Newberry, Grote, and Lockwood.

PRIZES OF THE PARIS ACADEMY OF SCIENCES

THE following is a complete list of the prizes awarded by the Academy at its annual meeting, January 28.

The two great prizes in mathematics and physics were not awarded this year.

In mechanics the Poncelet prize was awarded to M. Laguerre for his mathematical works; the Montyon prize to M. Caspari for his work on Chronometers; the Plumey prize to M. Freminville for his improvements in steam-engines; the Fourneyron prize to M. Mallet for his tramway engine.

In astronomy the Lalande prize was given to Prof. Asaph Hall, the discoverer of the satellites of Mars; the Vaillant prize to M. Schuloff for his method of detecting the small planets; the Valz prizes to MM. Paul and Prosper Henry for their star maps.

In physics the Lacaze prize was awarded to M. A. Cornu for his researches on the determination of the rate of light.

In chemistry the Jecker prize was awarded to M. A. Houzeau for his researches on the production of ozone; the Lacaze prize to M. Troost for his many valuable chemical researches.

In botany the Barbier prize was divided between M. Galippe for his toxicological studies on cantharides, MM. Lepage and Patrouillard for their services to medicine and pharmacy, and M. Manouvriez for various physiological researches. The Desmazières prize was divided in part between Dr. Quélet for his work on the fungi of the Jura and the Vosges, and M. Bagnis for his memoir on the puccinia. From the Bordin prize an encouragement of 1,000 francs was awarded to M. Charles Eugène Bertram for his work on the lycopodiaceæ; another Bordin prize was awarded to the same botanist for his work in connection with angiosperms and gymnosperms.

In anatomy and physiology the Shore prize was awarded to M. Jousset de Bellesme for his researches on the physiology of insects.

Among prizes in medicine and surgery, one of 2,500 francs was given to Prof. Hannover, of Copenhagen, for his work on the retina of man and the vertebrates; 1,500 francs to Dr. Topinard for his work on anthropology.

In physiology the Montyon prize was divided between Prof. Ferrier and MM. Carville and Duret. The Lacaze prize was given to M. Dareste for his researches on the artificial production of monstrosities.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—The Smith's Prizes have been adjudged as follows:—First prize, John Edward Aloysius Steggall, B.A., Trinity, second wrangler, 1878; second prize, Christopher Graham, B.A., Gonville and Caius College, third wrangler, 1878. By this award it will be seen that the senior wrangler has failed to secure either of the Smith prizes, an unusual circumstance, which has only occurred seven times since the foundation of the prizes by Dr. Smith in 1769, viz., in 1770, 1830, 1859, 1867, 1874, 1875, and again this year.

EDINBURGH.—The Falconer Memorial Fellowship in Palæontology and Geology, of the annual value of about 100*l.*, tenable for two years (and, under certain conditions, for a longer period), is now vacant, and is open for competition by graduates in Science or Medicine of the University, of not more than three years' standing at the time of the competition. Names of candidates must be sent, on or before the 1st of April, to the Secretary of the University, from whom further details may be obtained.

The Shaw Fellowship in Mental Philosophy, of the annual value of about 170*l.*, tenable for five years, will be open to competition in December next by graduates in arts of either of the four Scottish Universities, of not more than five years' standing at the time of the competition, and by all students of the said Scottish Universities, who, although they have not graduated in arts, have successfully passed all the examinations necessary for graduation in arts within the period of five years before the time of the competition. Candidates must give their names and addresses to the Secretary of the University before December 1.

PRUSSIA.—The ten Prussian universities cost yearly 7,146,000 marks (357,000*l.*), of which sum about two-thirds is contributed directly by the State. This amount is divided as follows:—Berlin, 1,334,700 marks; Bonn, 712,500; Königsberg, 668,600; Breslau, 620,300; Kiel, 478,800; Marburg, 430,400; Halle, 368,800; Göttingen, 268,600; Greifswald, 135,600, and Münster, 102,500. The Saxon Government has difficulty in inducing its parliament to bestow its usual annual grant of 700,000 marks on the University of Leipzig, the argument being advanced that only one-third of the students were natives of the kingdom.

FRANCE.—The new Ministry is making rapid strides in the direction of general education. In a law lately laid before the Chamber of Deputies, we notice an appropriation of 120,000,000 francs, which is intended to serve for the erection or purchase of over 27,000 new school-houses, as well as their equipment.

SCIENTIFIC SERIALS

Annalen der Physik und Chemie, No. 12, 1877.—On the laws according to which gases spread in liquid, viscous, and solid bodies, by M. v. Wroblewski.—On the galvanic resistance of selenium, by M. Forssmann.—On the relation of the electric conductivity of selenium to heat and light, by M. W. Siemens.—Influence of light on the electric resistance of metals, by M. Hanseman.—On the significance of polarisation for the electric behaviour of liquids, by M. Herwig.—On a mode of inference employed by M. Clausius in the electrodynamic theory, by M. Zöllner.—Supplement to a paradox of the mechanical theory of heat, by M. Ritter.—On the Crookes's radiometer, by M. Hankel.—On the perception of colours, by M. Weinhold.—On the composition of æschynite and samarskite, by M. Rammelsberg.—On the inventor of the plate of the air-pump, by M. Gerland.—Supplement to "Studies on Chemical Volumes," by M. Ostwald.

Reale Istituto Lombardo di Scienze e Lettere, Rendiconti, vol. xi. fasc. xvii.—Memoir of Prof. Giuseppe Ferrari.—Reports of the classes, announcement of prizes, &c.

Fasc. xviii.—Experiments with regard to the action of heat on the radiometer, by M. Hajech.—Researches on differential equations, by M. Casorati.—On seismic movements in the valley of Chiana and their influence on the hydrographic condition of the valley of the Tiber, by M. Verri.—On a peculiar horny growth, by M. Sangalli.

Journal de Physique, December.—Telephones, by M. Niaudet.—On a new apparatus for measuring the frequency of periodic movements, by M. Marey.—Continuity of the liquid and the gaseous state of matter, by M. Bouty.—Study on the formation of the negative photographic image, by M. Lermontoff.

Morphologisches Jahrbuch, vol. iii., Part 4.—R. Wiedersheim, on the cranial skeleton of Urodela, ninety pages, four plates, dealing with Siredon, Amblystoma, Salamandra, Chioglossa, Triton, and the Salamandridæ generally.—W. Salensky, on the budding of Salpæ, fifty-four pages, 3 plates.—W. Rauber, on the last spinal nerves and ganglia.

SOCIETIES AND ACADEMIES

LONDON

Geological Society, January 9.—Prof. P. Martin Duncan, F.R.S., president, in the chair.—Ephraim Brunt, T. W. Cowan, and Henry Fox were elected Fellows of the Society.—The following communications were read:—On the great flat lode south of Redruth and Camborne, by Dr. C. Le Neve Foster, B.A.—On some tin-mines in the parish of Wendron, Cornwall, by Dr. C. Le Neve Foster, B.A.—On some of the stockworks of Cornwall, by Dr. C. Le Neve Foster, B.A.—The precarboniferous rocks of Charnwood Forest, Part II., by the Rev. E. Hill, F.G.S., Fellow and Tutor, and the Rev. T. G. Bonney, F.G.S., Fellow and late Tutor of St. John's College, Cambridge. The authors described the result of the microscopic examination of a considerable series of the clastic rocks of Charnwood. Many of these, even among the finer beds, prove to be of pyroclastic origin. The coarser are generally composed of a ground mass of pulverised felspar, with viridite and some iron peroxide, full of larger fragments of felspar crystals (generally both of orthoclase and plagioclase) and lapilli. The structure of these is often distinct, some are certainly andesites, others some kind of trachyte; slaty fragments are also present, and occasional grains of quartz. The authors express their opinion that all the larger felspar crystals, and most, if not all, the quartz grains, are of clastic origin, even in the more highly altered varieties. Some of the

larger fragments in the breccias were examined, and referred in part to devitrified trachytes not very rich in silica. The igneous rocks were then described. The syenites of the southern and northern districts were shown probably to belong to one system of intrusion. The hornblendic granite of the Quornden district was also described, and the microscopic structure of the different varieties of it and the above investigated. A number of igneous rocks generally forming dykes in these was described; some appear to be altered basalts, others andesites, one is a felsite, another a diorite. A group of outlying igneous rocks in the vicinity of Narborough was described. Of these, one is a quartz feltsite with some hornblende; another varies between this and a quartziferous syenite; the rest are syenites, and one contains so much plagioclase as to be almost a diorite. One of the above, near Enderby, is seen to be distinctly intrusive in an altered slaty rock, which the authors have no doubt belongs to the Forest series. This discovery proves the igneous character of these rocks also, and extends the area of the slaty series five miles further south than was previously known. A section was devoted to the faults of the Forest region. Here the principal fault runs along the anticlinal axis, with a downthrow on its eastern side which diminishes from 2,500 feet at the north end to 500 feet at the south end. East of this the beds seem undisturbed, but on the west they are shattered by many faults, whose course cannot be traced. These are most numerous near Whitwick. The anticlinal fault is pre-carboniferous. In conclusion, the age of the clastic and of the igneous rocks was discussed. The authors inclined to the opinion that the former are of the same age as the Borrowdale series of the Lake district (lower Silurian), but admitted that the recent discovery of agglomerates in the pre-Cambrian rocks of Wales, and in the probably pre-Cambrian ridges of the Wrekin district, weakens the arguments for this correlation. They do not think that there is any reason for supposing them Cambrian. If the Charnwood series is lower Silurian, they think it most probable that the syenites and the Quornden granite were intruded in some part of the old red sandstone period, and that the later dykes were very probably post-carboniferous but pre-triassic.

Meteorological Society, January 16.—Mr. H. S. Eaton, M.A., president, in the chair.—The Council, in their Report, express their gratification at the increase in the number of the Fellows and stations of the Society, the greater size of the *Quarterly Journal*, and the higher value placed on it by foreign scientific societies, the augmentation of the library, and the addition to the sum hitherto contributed by the Meteorological Council, as well as at other evidences of vigour and progress manifested during the year. The number of Fellows now amounts to 417. The president then delivered his address:—During his tenure of office the alliance between the Meteorological Council and the Society had been further cemented, the Society supplying the Government with certain statistics, and getting some assistance from the Council in return. This arrangement had been completely successful, and the president considered it calculated to foster the growth of climatic meteorology under the auspices of the Society, and likely to remove any jealousy on the part of the public towards a governmental department so peculiarly constituted as the Meteorological Council. After criticising some of the work undertaken by the last-mentioned body, Mr. Eaton exhibited curves of the results of the hourly observations of the barometer and thermometer for the year 1876 at Valentia, Armagh, Glasgow, Aberdeen, Falmouth, Stonyhurst, and Kew, these being the stations established in 1868 for determining the meteorological constants of the British Isles. The curves showing the combined diurnal and semi-diurnal variation of atmospheric pressure might be referred to one of two distinct types. In one of them the minimum of pressure was most pronounced in the morning, in the other in the afternoon. The former type was found at the maritime stations of Valencia and Falmouth, the latter at inland stations such as Kew. The diurnal range of the temperature of the air was closely related to the pressure. It was least at the maritime stations, reaching only 4°·8 at Falmouth, and attaining a maximum of 9°·3 at Kew.—The following gentlemen were elected Officers and Council for the current year:—President: Charles Greaves, M.Inst.C.E., F.G.S. Vice-Presidents: Henry Storks Eaton, M.A., James Park Harrison, M.A., Robert James Mann, M.D., F.R.A.S., Charles Vincent Walker, F.R.S. Treasurer: Henry Perigal, F.R.A.S. Trustees: Sir Antonio Brady, F.G.S., Stephen William Silver, F.R.G.S. Secretaries: George James Symons, John W. Tripe, M.D. Foreign Secretary: Robert H. Scott, M.A., F.R.S. Council: Hon. Ralph Abercromby, Arthur Brewin, F.R.A.S., Charles Brooke, F.R.S., Edward Ernest Dymond, William Ellis, F.R.A.S., Rogers Field, B.A., M.Inst.C.E., John Knox Laughton, F.R.A.S., Rev. William Clement Ley, M.A., Richard Strachan, Henry Samuel Tabor, Capt. Henry Toynbee, F.R.A.S., George Mathews Whipple, B.Sc.—A resolution was passed to the effect that ladies be admissible as Fellows of the Society.

Physical Society, January 19.—Prof. G. C. Foster, president in the chair.—The following were elected Members of the Society:—J. Angell, Lieut. G. S. Clarke, R.E., T. F. Iselin, M.A., J. W. Russell, M.A.—Mr. W. H. Preece read a paper on some physical points connected with the telephone. This instrument may be employed both as a source of a new kind of current and as the detector of currents which are incapable of influencing the galvanometer. It shows that the form and duration of Faraday's magneto-electric currents are dependent on the rate and duration of motion of the lines of force producing them, and that the currents produced by the alteration of a magnetic field vary in strength with the rate of alteration of that field; and further, that the infinitely small and possibly only molecular movement of the iron plate is sufficient to occasion the requisite motion of the lines of force. He pointed out that the telephone explodes the notion that iron takes time to be magnetised and de-magnetised. Mr. R. S. Brough has calculated that the strongest current employed in a telephone is $\frac{1}{10000000000}$th of the C. G. S. unit. Mr. Preece explained that the dimensions of the coil and plate depend on the strength of the magnet, but the former should always consist of fine wire and be made as flat and thin as possible. The adjustment of the position of the magnet (as near as possible to the plate without touching) is easily effected by sounding a vowel sound *ah* or *o* clearly and loudly; a jar is heard when they are too near together. After briefly enumerating the attempts which have been made to improve the instrument, he mentioned the various purposes to which it can be applied. In addition to being useful in the lecture room, in conjunction with several well-known forms of apparatus, it forms an excellent detector in a Wheatstone bridge for testing short lengths of wire, and condensers can be adjusted by its means with great accuracy. M. Niaudet has shown, by employing a doubly wound coil, that it can be used to detect currents from doubtful sources of electricity, and it is excellent as a means of testing leaky insulators. Among the facts already proved by the telephone may be mentioned the existence of currents due to induction in wires contiguous to wires carrying currents, even when these are near each other for only a short distance. Mr. Preece finds that if the telephone wire be inclosed in a conducting sheath which is in connection with the earth, all effects of electric induction are avoided; and further, if the sheath be of iron, magnetic induction also is avoided, and the telephone acts perfectly. A great number of experiments on the use of the instrument on telegraphic lines were then described, from which it appears that conversation can be carried on through 100 miles of submarine cable, or 200 miles of a single wire without difficulty, with the instrument as now constructed. The leakage occurring on pole-lines is fatal to its use in wet weather, for distances beyond five miles. An interesting series of telephones was exhibited, and by means of one of very large dimensions Mr. Preece showed that the currents produced by pressing the centre of the plate sensibly affect a Thomson galvanometer, and that the motion of the needle ceases in a remarkably instantaneous manner as soon as the pressure is removed, a necessary condition in order that the receiving-plate should accurately reproduce the motions of the sending-plate. In the discussion which followed, Mr. R. Sabine suggested that the failure of all attempts at improving the instrument by increasing its dimensions might be due to the *damping* action of the permanent magnet on the plate, the strain on it being proportional to the size of magnet and rendering it less sensitive to the sonorous waves. Mr. Coffin pointed out how interesting it would be if, instead of employing a receiving-instrument, the currents could be communicated directly to the auditory nerves, and Prof. Adams explained the relation subsisting between the character of the vibrations of the disc and the character of the electric currents to which they give rise.—Dr. Lodge described a simple form of apparatus for determining the thermal conductivity of rare substances, such as crystals, which cannot be obtained in slabs or rods. It consists of two small tin cans with a copper arm about eight inches long projecting horizontally from each, the external ends being clean and flat. They are

placed in a straight line with the crystal between them, and held together by a slight horizontal pressure. Holes are drilled in the copper rods for thermometers, and the curves of temperature being given by these, that for the intermediate crystal can be at once calculated.

Victoria (Philosophical) Institute, January 21.—Prof. Lias, of St. David's College, read a paper on Matthew Arnold and modern culture.

February 4.—The Right Reverend Bishop Cotterill read a paper upon scientific thought and religious belief.

Institution of Civil Engineers, January 22.—Mr. Bateman, president, in the chair.—The paper read was on some recent improvements in dynamo-electric apparatus, by Dr. Higgs, Assoc. Inst. C.E., and Mr. Brittle, Assoc. Inst. C.E.

MANCHESTER

Literary and Philosophical Society, December 11, 1877.—Mr. E. W. Binney, F.R.S., president, in the chair.—Note on the daguerreotype portrait taken of the late Dr. Dalton, by J. B. Dancer, F.R.A.S.—Note on metallic niobium and a new niobium chloride, by Prof. H. E. Roscoe, F.R.S.—On the retention of saline impurities by hydrated ferric oxide, by Mr. Charles Frederick Cross, Dalton Scholar in the Owens College. Communicated by Prof. H. E. Roscoe, F.R.S.

December 26.—Mr. E. W. Binney, F.R.S., president, in the chair.—Notice of a large boulder stone at Old Trafford, Manchester, by E. W. Binney, F.R.S.—On the geometrical representation of the equation of the second degree, by Charles Chambers, F.R.S., Superintendent of the Colaba Observatory, Bombay. Communicated by J. A. Bennion, F.R.A.S.

EDINBURGH

Chemical Society, January 16.—Mr. W. Inglis Clark, B.Sc., vice-president, in the chair.—A paper was read by Mr. John Gibson, Ph.D., F.R.S.E., on yttrium and erbium, being the second part of an historical sketch of the rarer elements.

January 30.—Mr. Newton Burns presiding.—Papers were read by Mr. G. Carr Robinson, F.R.S.E., on the solid fatty acids of cocoa-nut oil, and by Messrs. Robinson and Thomson on the composition of gases from lime-kilns.

PHILADELPHIA

Academy of Natural Sciences, July 3.—Prof. G. A. König, on protovermiculite, a new micaceous mineral from Arkansas, $R'_3R^{vi}Si^{iv}_3O'_{12} + H_2O$, related to jefferisite and colsagecite.

July 24.—Mr. J. A. Ryder, on colour variation in mammals. The distribution of colour in wild and domestic animals was compared, showing that bilateral symmetry of colouring is interfered with in some way by domestication, wild animals almost invariably being symmetrically coloured.

July 31.—Dr. Rothrock, on the poisonous properties of the Leguminosæ.

August 14.—Prof. G. A. König, on strengite, from Virginia. This mineral, $Fe_2P_2O_8 + 4H_2O$, was discovered in cavities in dufrenite.

August 28.—Dr. D. J. Jordan and W. S. Brayton, on Lagochila, a new genus of catostomoid fishes, known in Georgia as the Hare-lip Sucker.

WELLINGTON

Philosophical Society, August 4, 1877.—W. T. L. Travers, F.L.S., M.H.R., president, in the chair.—Dr. Hector drew attention to several interesting additions to the museum, which were arranged on the table. Among the most important was a handsome bird from New Guinea, the *Goura victoriæ*.—Mr. Kirk also called attention to a log of black maire, a species of olive (*Olea apitita*), sent by Mr. Elliotte, of the Pakuratahi, which, on account of its great hardness, is much used as blocks and cogs in machinery.—Capt. Edwin read a letter from Mr. Rawson, on the reciprocity of seasons, the character of the seasons in Europe being followed by a similar season in Australia and New Zealand. Mr. Carruthers thought that even in New Zealand the seasons did not agree; and Dr. Hector said it would be necessary to fix upon the one place for comparison, as seasons were not uniform, a moist season on the east coast being frequently a dry season on the west coast. Dr. Newman considered that the seasons were affected all over the world by sun-spots.—Mr. McKay then read a paper on gold found in the Mackenzie district of Canterbury; on which subject Dr. Hector added some interesting information regarding the occurrence of gold generally in that district, and pointing out that Mr. McKay's observation that the gold and associated quartz were found only in the last formed moraines and alluviums, confirmed his theory that the retirement of the glaciers was chiefly due to the erosion of the mountains.

VIENNA

Imperial Academy of Sciences, December 13, 1877.—On the present state of the water question, by M. Wex.—On the anatomy of the African elephant, by M. Mojsisovics.—The electrical after-currents of transversally magnetised iron bars, by M. Streintz.

December 20.—The protoplasm of the pea, by M. Tangl.—On a new apparatus for direct volumetric determination of the moisture of the air, by M. Schwackhofer.—Elementary deduction of the complete formula for determination of the tone of vibration of a mathematical pendulum, by M. Pscheidl.—Contribution to knowledge of cupric chloride, by M. Rosenfeld.—On the air-pressure at Vienna, with supplemental remarks on the temperature of Vienna, by M. Hann.

GÖTTINGEN

Royal Academy of Sciences, November 3, 1877.—Attempt at a theory of electric separation through friction, by M. Riecke.

December 1.—Report of the Secretary (126th anniversary).—Obituary notice of von Baer.—Announcement of subjects for prize competition, &c.—On the formation of the volcano of Fuego in Guatemala, and account of an ascent of it, by M. von Seebach.—On the origin of language, by M. Benfey.

December 26, 1877.—New geometrical and dynamical constants of the earth, by M. Listing.

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THURSDAY, FEBRUARY 14, 1878

MR. STANLEY

SIR SAMUEL BAKER spoke the truth on Thursday night last in St. James's Hall when he told Mr. Stanley that the Prince of Wales might be regarded as the spokesman of the nation when he addressed the great explorer in warm words of welcome and admiration. Not for many years has there been so much excitement in London as there was on Thursday in connection with the wild rumours on the state of affairs in the East; it seemed as if people could not possibly have a shred of attention to bestow on any other matter, but nearly two hours before the time at which the meeting of the Geographical Society was to commence the doors of St. James's Hall were besieged by an eager crowd; and many hundreds, if not thousands, had to be left out in the distribution of tickets. The welcome which Mr. Stanley received could not possibly have been more enthusiastic. In view of the many hard words that have been spoken of Mr. Stanley's conduct under certain trying circumstances, the sight on the platform of the stately figure and genial face of the venerable missionary and explorer, Dr. Moffat, father-in-law of Livingstone, was exceedingly gratifying, showing, as we think it did, that so humane and experienced an "African" as he does not consider that Mr. Stanley has greatly sinned.

That Mr. Stanley should be received with all the enthusiasm of hero-worship by the civilised world is just what might have been expected. It is seldom, however, that a hero receives the glory due to his heroism so promptly as Mr. Stanley has done, especially when that glory has been earned in the field of exploration. Hitherto it has only been through the tardy medium of a book that the public at home have learned of an explorer's work; but in Mr. Stanley's case we have been able to watch his progress step by step by means of the eagerly-looked-for letters he sent home from the heart of Africa, like spectators watching the progress of an assault against a hitherto impregnable stronghold. Thus when Stanley emerged once more into "the light of common day" the very first white man at Emboma into whose hands that memorable appeal for help came knew at once that one of the greatest deeds of all time had been accomplished.

The exact grade to be allotted to Mr. Stanley among the *dii majores* of explorers must be left to a future generation, but this we may be sure of that when the man and his work shall stand clearly out against the "azure of the past," when all the accidental circumstances that accompanied the ever-memorable journey shall have been forgotten, Mr. Stanley will take his place among the foremost of pioneer explorers, as one of the greatest benefactors to humanity and science. He, indeed, has shown that there is work in the world for many generations of men of science, and it will be long after the region has been opened up to commerce ere science will have obtained an adequate knowledge of its treasures.

Mr. Stanley has been termed "the Bismarck of African exploration;" as Bismarck has united into one great empire the fragmentary states of Germany, so has Stanley by the work he has accomplished united into one great whole the *disjecta membra* of African exploration. But the likeness between the two men extends further than this; in the one case as in the other there has been a well-defined purpose carried out by means of a clear and cool head, firm nerve, unflinching will, and (perhaps more important than all) an iron constitution.

What then has Mr. Stanley done to justify the enthusiasm with which he has been universally received by high and low, by learned and unlearned?

One of the most remarkable characteristics of his work is the unprecedented rapidity with which it was accomplished, considering the rich harvest of results. As he told his followers at Zanzibar he meant to do, he shot across the continent like an arrow. In two years and a half, with many zigzags and subsidiary explorations, Africa was crossed from Bagamoyo to the mouth of the Congo. The great work of the expedition, the exploration of the Lualaba from Nyangwe to the sea occupied only five months; looking at it in all its aspects, no explorer ever did so great a work in anything like the time.

For thousands of years has the Nile been a mystery which civilised humanity has never ceased to seek to penetrate; no other geographical problem, not even the pole itself, has had such a fascination for Europe. Many and many a life has been sacrificed in the attempt to find the source of the sacred stream, and it was in seeking this goal that Livingstone wandered away south to find "the fountains of Herodotus," only to find a grave on the marshy shore of Lake Bangweolo. The glory of virtually settling the problem has remained to Livingstone's discoverer and pupil, Stanley. In his march northwards from Ugogo to Lake Victoria Nyanza, the explorer came upon a river which flows into the south of that lake, the river Shimeeyu, about 350 miles long, which may be regarded as one of the most remote, if not the most remote, of the sources of the old Nile. Further, into the west side of the lake flows Speke's Kitangulé river, which Mr. Stanley has re-baptised the Alexandra Nile; this river the latter explored with much thoroughness while staying at the court of the gentle Rumanika. He found it to be a broad lake-river, giving off many lagoons, one of them Speke's Lake Windermere, and having its source in Speke's Lake Akanyaru (now the Alexandra Nyanza) which again has, Mr. Stanley believes, a river of considerable length flowing into its west side, and another coming from the south, having its origin on the east of Lake Tanganyika. Here then, no doubt, we have the ultimate sources of the Nile, which have been sought for since history began. Mr. Stanley, we believe, has virtually set the question at rest, though we are sure he will willingly share the credit of the discovery with Speke, whose geographical instinct was astonishing, and the essential accuracy of whose discoveries have been throughout confirmed by his successor.

For the first time we have, through Mr. Stanley's exploration, an approximately accurate idea of the outline and extent of Lake Victoria Nyanza. Any map of Africa published two years ago shows this lake in a triangular shape, with an offshoot in its north-east corner. Mr. Stanley has broadened it out into an irregular square, with a coast-line of about 1,000 miles in length, studded with islands, many of them inhabited, and its shores peopled by many different tribes.

The geography of the region between Victoria Nyanza and Albert Nyanza may now be plotted with considerable

fulness after the work of Mr. Stanley, who, however, was unable to carry out the plan of doing for the Albert what he did for the Victoria. Through one of the valleys which run north and south between the mountains of this region flows another tributary of the Alexandra Nyanza, and on Mount Gambaragara dwell those mysterious fair-skinned people that Speke heard of, but specimens of whom Mr. Stanley actually saw. About the time of Mr. Stanley's visit, we may remind the reader, Signor Gessi explored the Albert Lake, and we believe, to judge from his narrative, was unwittingly driven to its southern shore, about 1° S. lat. Quite recently, as we recorded at the time, Col. Mason has sailed round the lake, and reports it to be comparatively small and land-locked, with no important affluent other than the Victoria Nile.

On Lake Tanganyika Mr. Stanley completed the work of his predecessors. He circumnavigated the lake, and for the first time accurately plotted the outline of its southern part, adding considerably to our knowledge of the people and products of its shores. We have already spoken at some length of his examination of the Lukuga, which Cameron set down on the middle of the western shore as the long-sought-for outlet of the lake. Stanley examined the Lukuga with great care, and concludes that at present it is only a creek, but that as the waters of the lake are encroaching on the shore, either by the rise of the former or subsidence of the latter, the Lukuga will, in a very short time, actually become an outlet. What Mr. Stanley has told us of the lake and the surrounding region is well calculated to whet the curiosity of the geologist and physical geographer. We have already alluded to Mr. Stanley's theory of the past physical history of the region; but even if his knowledge of geology were adequate to the formation of an acceptable theory, he had scarcely time enough to collect the necessary data. Here, at any rate, is a splendid field for the geologists of the future.

Had Mr. Stanley returned home after his exploration of Tanganyika, or had the toss between himself and poor Pocock been "tails to go south" and leave the problem of the Lualaba unsolved, no one would have blamed him, and his work in the Nyanza region would have added very considerably to his previous reputation as an explorer. But his daring dash down the Lualaba is a *coup* that has immortalised him; it has done for him what the publication of "Pickwick" did for Dickens, it has compelled the world to admit that in his own line he is a genius of the first rank. Indeed we cannot but regard the spirit which animated Stanley at this crisis of his journey in Africa as a really heroic one. He himself happily and aptly expressed it in his address at St. James's Hall by quoting the words which Tennyson puts into the mouth of Ulysses, and which he applied to the position of himself and his followers when they were left by their Arab escort on the broad bosom of the Lualaba, at the very gate of the unknown region:—

"My mariners,
Souls that have toil'd, and wrought, and thought with me,—
. . . . . Come, my friends,
'Tis not too late to seek a newer world.
Push off, and sitting well in order smite
The sounding furrows; for my purpose holds
To sail beyond the sunset, and the baths
Of all the western stars, until I die.
It may be that the gulfs will wash us down:
It may be we shall touch the Happy Isles
And see the great Achilles whom we knew.
Though much is taken, much abides; and tho'
We are not now that strength which in old days
Moved earth and heaven; that which we are, we are;
One equal temper of heroic hearts,
Made weak by time and fate, but strong in will
To strive, to seek, to find, and not to yield."

The mouth of the Congo has been known since the Portuguese, in the fifteenth century, began to creep down the African coast, and Tuckey, in the beginning of the present century, traced it about 150 miles to the lower cataracts. Its origin and course was one of the few, probably the greatest of remaining, mysteries in geography. Long ago the Pombeiros and other travellers came across streams inland from the Portuguese possessions in south-west Africa, which run northwards, and latterly Livingstone made known the great river Lualaba, which, however, against all evidence, he believed to be connected with the Nile. One of the principal streams known, at least since the time of the Pombeiros, is the Casai, a considerable river running northwards, and which some geographers maintained must be the upper course of the Congo. Others again maintained, and the reports of the natives seemed to confirm it, that in the region between Nyangwé on the Lualaba and the sea, was a great lake into which that and other rivers flowed, while some seemed to think that the Lualaba ran southwards, and probably ultimately flowed into Lake Chad. Livingstone, as we have said, thought the Lualaba belonged to the Nile, while Cameron was convinced it was the Upper Congo, but that it flowed almost straight westwards. The solution of the problem was a task well calculated to fascinate a man like Stanley, a task in which all his rare qualities as an explorer would be developed to the utmost, but a task for which he has proved himself easily equal. It is difficult, indeed, to see how the work could have been accomplished for generations except by a man of Stanley's character, and by the method adopted by him. In whatever light we regard this part of his recent work in Africa—whether as a mere exploit, or as a vast addition to geographical knowledge, or in the light of the great results that are likely to follow to civilisation, commerce, and science—it has scarcely, if ever, been surpassed in the history of geographical exploration. We have in previous numbers shown the magnitude and importance of this discovery. In the course of a few months, by the daring genius of one man, there has been thrown open to our knowledge a river of the first rank, watering a region of apparently exhaustless resources both for the man of science and the trader. It is about 3,0co miles long, has many large tributaries, themselves affording many hundred miles of navigable water; waters a basin of nearly a million square miles, and pours into the sea a volume estimated at 1,800,000 cubic feet per second. Such a piece of work is surely enough to immortalise a man.

Such, briefly, is the work accomplished in so short a space by the Commissioner of the *Telegraph* and the *Herald*, a work which he set about as a mere piece of business in connection with his calling of special correspondent, but for which Mr. Bennett had the insight to see he was unusually well adapted. A private business enterprise has thus accomplished what the much-instructed and

elaborately-equipped expeditions of learned societies have failed to do. It would be a pity were Mr. Stanley's exceptional aptitude for the work of exploration allowed to lie fallow. Even in the basin of the Congo much remains to be done, and we doubt if any great results will follow the Portuguese expedition which Mr. Stanley met at Loanda. There is also South America, the centre of which is now more unknown than Central Africa, and which awaits a pioneer like Stanley to show the way to the minute explorer and surveyor. It is stated that Mr Gordon Bennett contemplates equipping a polar expedition, so that we fear he thinks he has done enough for Africa. But whether or not Mr. Stanley again enters the field as an explorer, he has written his name in indelible letters alongside that of Livingstone, on the heart of Africa.

WAS GALILEO TORTURED?

Ist Galilei gefoltert worden? Eine kritische Studie. Von Emil Wohlwill. (Leipzig: Duncker and Humblot, 1877.)

THIS work treats with exhaustive thoroughness a question first raised about a century ago, as early, in fact, as advancing political liberty rendered its public discussion consistent with personal safety, and which has occupied scientific biographers pretty continuously since that time. The author's main object in reopening an issue, which the majority of recent authorities consider as settled in the negative, is to bring into due prominence the bearing on it of fresh evidence rendered accessible only within the last ten years. Up to 1867, though it was known that a detailed official record of Galileo's trial was preserved in the archives of the Inquisition, only a few isolated and questionable extracts from it had been made public. In that year, however, M. Henri de l'Epinois, by permission of the Papal authorities, published *in extenso* the most important of the documents contained in the trial-record. These, supplemented by still more recent corrections and additions, which it is unnecessary to particularise here, supplied a body of new evidence bearing more or less directly on the issue whether the Roman Inquisition, in its treatment of the great astronomer, had recourse in any degree to that test of physical endurance which formed a recognised part of its procedure as of that of contemporary secular courts in cases like his.

It was of course to be expected that in documents drawn up exclusively for the use of the Inquisition itself there would occur a number of technical expressions the exact meaning of which would be far from obvious to a reader unacquainted with the details of procedure in the holy office. This accordingly turns out to be the fact, and interposes no slight obstacle to the interpretation of the fresh evidence thus presented. Wohlwill, in order to overcome it, has put himself through an elaborate course of Inquisitional literature, studying minutely the fixed technical forms for conducting suits in the holy office laid down in manuals and instructions published for the guidance of its own officials. It is obvious how firm is the foundation thus to be secured in comparison with the precarious guessing which would otherwise be inevitable. The tasks both of preliminary inquiry and of subsequent application, have been performed with the utmost diligence, accuracy, and sagacity.

It would be impossible, within the limits of this notice, to enter upon the detailed arguments by which Wohlwill supports his views. All that can here be done is to state the chief results at which he arrives, together, where feasible, with some indication of the line by which he has travelled.

The final sentence delivered by the Inquisition in Galileo's case contains a statement that the court had judged it necessary to proceed against him to "the rigorous examination."[1] Libri had, as early as 1841, asserted, on the authority of various inquisitional manuals, and in particular of one entitled "Sacro Arsenale della S. Inquisizione," that "*esame rigoroso*"[2] was exactly equivalent to "torture," and that this passage of the sentence was absolutely decisive of the whole question. Wohlwill shows, by a complete scrutiny of the "Sacro Arsenale," that a "rigorous" examination in most cases meant one conducted under torture, but that this expression sometimes denoted a less severe procedure. It appears that where the course of the preliminary investigation led the judges to suspect that the accused had not stated the entire truth, three distinct and increasingly intense trials of fortitude and endurance were prescribed for successive adoption. First the prisoner was brought into the ordinary hall of audience and told briefly and sternly that unless he could make up his mind to confess the truth, recourse would be had to the torture. If this produced no result, he was next carried into the torture-chamber, where the use of the various instruments was explained to him, or he was even seized by the attendants, stripped of his clothes, and bound upon the rack, so that nothing remained but to set its machinery in action. In this situation he was again invited to save himself by confession. If he still remained firm, the infliction of the torture at once ensued. The two preliminary appeals to terror were described as the "verbal scaring" (*territio verbalis*), and the "real scaring" (*territio realis*), while the words "rigorous examination" were reserved, strictly speaking, for the final scene of actual agony. It is clear, however, from passages of the "Sacro Arsenale," that in certain cases confessions elicited by the second method of proceeding were described as made under the rigorous examination, though this laxity of expression is explicitly stated not to extend to the first. The text of the sentence against Galileo therefore implies, at the least, that he was carried into the torture-chamber and submitted to some form of the *territio realis*.

The same authoritative document informs us what was the general character of his replies under this ordeal. He answered "in a catholic manner," *i.e.*, denied that he held the reputedly heretical doctrines attributed to him. While stating this fact the Court were careful to insert a saving clause that the answers so given were not to prejudice other points admitted by or proved against the accused. The significance of this clause, which preceding writers appear to have passed unnoticed, is, according to Wohlwill, as follows:—So great was the regard professed by the Inquisition for assertions steadfastly adhered to under the torture, that in regard to whatever formed the actual subject-matter of a rigorous examination, the answers of the accused, if he thus stood by them, had to be

[1] "Giudicassimo esser necessario venir contro di te al rigoroso esame."

[2] Wohlwill has shown that Italian, and not, as has hitherto been assumed, Latin, was the language in which the sentence was promulgated.

accepted as true. It was therefore the interest of the Court carefully to define the limits within which it proposed to allow the accused this chance of escape. The saving clause under consideration was devised for this very purpose, so as to prevent answers made during the rigorous examination from possessing the power of voiding articles of charge or admission not explicitly included in the questions of the interrogating official. Its actual application was, of course, made at the opening of the rigorous examination as a preliminary to the torture, and the fact of the *caveat* being formally recited in the subsequent sentence is held by Wohlwill to confirm his view that Galileo was submitted at least to a *territio realis* in the torture-chamber.

After this examination of the evidence supplied by the sentence, our author next shows, in opposition to a considerable body of influential opinion, that there was nothing in the circumstances of Galileo's case to negative antecedently the application of torture, and no ascertained subsequent fact inconsistent with its having been inflicted. The absence of any reference to it in his few remaining letters of later date than the trial is completely accounted for by the oath of absolute silence imposed upon all who appeared before the tribunal of the Inquisition. The fact that Galileo was released from the custody of the Court three days after his final examination, and ten days later was able to take active exercise, shows only that severe torture was not inflicted, but by no means excludes the milder form (*leggiera tortura*), to which the Inquisitional manuals distinctly refer. The advanced age of the prisoner, who was at this time seventy, does, it is true, afford a certain degree of presumption in this direction, inasmuch as Inquisitional authorities usually incline to stop at the *territio realis* in the case of aged persons; they give, however, the alternative of applying "a kind of torture suitable to old people," so that this indication is, after all, far from conclusive.

This clearing of the ground is followed by a detailed investigation of the minutes of the trial contained in the Vatican record, so far as they bear on the question at issue. It thence appears that on June 16, 1633, Pope Urban VIII. ordered the prisoner to be interrogated as to his object in publishing his dialogues on the Ptolemaic and Copernican systems, threatened with the torture, and, if this failed to elicit a confession, condemned to abjuration and imprisonment during the pleasure of the Congregation. On June 21 this examination accordingly took place. Galileo was asked whether he held, or had held, that the earth was in motion and the sun at rest. He denied having done so since the decree of the Index on March 5, 1616, and though pressed by his interrogator with the contrary indications afforded by the dialogues themselves and repeatedly urged to tell the truth freely, clung to the denial. On being told that if he persisted further recourse would be had to the torture, he simply reiterated his former statement with this addition: "I am here in your hands, deal with me as you please." At this point the report abruptly terminates with a few words stating that nothing further could be done, followed by the signature of Galileo in attestation of his own deposition. Wohlwill points out that the threat of torture here recorded as delivered in the ordinary hall of audience cannot possibly count as a rigorous examination, since, according to the fixed language of the Inquisition, the latter proceeding did not begin until the officials and the accused had taken up their positions in the torture-chamber. There is therefore a direct contradiction between the sentence, which affirms that a rigorous examination was held, and the official minutes, which relate nothing capable of answering to that designation. It is the deliberately expressed opinion of the German investigator that this contradiction points to a fraudulent tampering with the trial-record, perpetrated at a time when it had become advisable, in the interest of the Roman hierarchy, to obliterate, as far as possible, the traces of a mode of treatment adopted towards the great Italian astronomer which, if once allowed to become notorious, would raise a cry of indignation throughout Europe. In support of this view its author has arrayed a very strong body of evidence, many particulars of which are of singular cogency. It is indeed in this latter portion of his work, where he examines the general claims of the Vatican manuscript to be considered a complete authentic and unaltered record of Galileo's trial, that Wohlwill does the most meritorious service. An attempt to determine to what precise stage of barbarity the Inquisition advanced in its dealings with its illustrious prisoner is after all a matter of secondary interest. On the other hand an energetic effort to ascertain how far the only official account we possess of perhaps the greatest event in the whole history of science is genuine and trustworthy, must be admitted to be an undertaking of signal importance. Enough, and far more than enough, has been achieved in this direction in the present work to excite the gravest suspicions and fully to justify the warning which at its close Wohlwill addresses to the Roman authorities, that in the present condition of affairs only two courses remain open to them; either to appear as accomplices in atrocious frauds, or to bring the whole truth to the light of day Nothing less than a thorough examination of all the remaining original records by competent and trustworthy palæographers can possibly settle the issues now definitely raised.

SEDLEY TAYLOR

The current number (January 16) of the *Rivista Europea*, which reached me after I had completed the above notice, brings a review of Wohlwill's work by Dr. Scartazzini, containing original matter due to his own independent research. The Italian critic has made strenuous use of the latest, and incomparably best, edition of the Vatican manuscript, that by Herr v. Gebler, and arrived at conclusions in regard to the falsification of its text considerably more sweeping than those based by Wohlwill on the less complete information accessible prior to the appearance of v. Gebler's edition. As far as the two writers cover the same ground they essentially agree in their verdict; the difference between them merely being that the Italian theory is more extensive than its German predecessor. It is gratifying to me to find the eminent position among historical critics to which the depth, clearness, and high originality of Wohlwill's writings on this subject in my judgment entitle him, claimed for him with equal confidence by Scartazzini. I regret that the exceedingly technical nature of the new arguments now advanced makes it impossible to give any idea of them here. They aim at pointing out the exact

nature of the excisions, transpositions, and other devices by which the Roman forger set to work to eliminate from the manuscript all trace of Galileo's having been, as Scartazzini stoutly maintains that he was, submitted to the actual torture. S. T.

THE AGRICULTURAL SOCIETY

The Journal of the Royal Agricultural Society of England. Part II., 1877.

THE current number of the Royal Agricultural Society's *Journal* is chiefly occupied with reports of the agricultural exhibitions held during the present summer at Liverpool and at Hamburg, and with reports on farms in Lancashire, Cheshire, and North Wales, which obtained the Society's prizes for good management at the Liverpool meeting. Another report deals with prize farms in Ireland in connection with the competition for small farms instituted by Earl Spencer. Besides these we have two lengthy papers on the American export meat trade, by Prof. Sheldon, of Cirencester, and by Prof. Alvord, of Easthampton, Massachusetts; three papers on village clubs, by Sir E. C. Kerrison, and Mr. Lawes; a paper on the impurities of clover seed, by Mr. Carruthers; and a short report of some investigations on foot-and-mouth disease, conducted at the Brown Institution.

The international exhibition at Hamburg was one of considerable importance: it was devoted exclusively to dairy husbandry. Lying, as Hamburg does, in the immediate neighbourhood of the great dairy countries of Northern Europe, an excellent opportunity was afforded of noting the advance made in dairy work during the last few years. The great improvement which has signalised this period is undoubtedly the use of ice in cream-setting. This invention dates from 1864, and is the work of J. G. Swartz, a Swedish farmer. In the ordinary method of cream-setting the milk is placed in very shallow pans, and stands for thirty-six hours or more while the cream is rising. The milk during this time usually turns sour, and the cream becomes contaminated with free fatty acids, with partially decomposed albuminous bodies, and with other products injurious to the flavour or keeping qualities of the butter. In Swartz's plan the milk, as soon as it reaches the dairy, is placed in deep metal pails, standing in a vessel full of ice. Not only does the low temperature reduce the process of change to a minimum, but, quite unexpectedly, it also greatly facilitates the rising of the cream; so that in pails having sixteen inches' depth of milk the cream is nearly all obtained in twelve hours. The butter churned from this sweet cream is not only very pure in flavour, but has remarkable keeping qualities. This plan, which is rapidly spreading in the north of Europe, and in the United States, is at present scarcely known in England. One obstacle to the general use of the method is undoubtedly the difficulty of procuring a sufficient supply of ice in such a climate as ours. This difficulty has been greatly diminished by the investigations of Prof. Fjord, of Copenhagen. He has shown that snow, if collected after thawing has begun, may be easily trodden into as small a compass as ice, and may be used with equal economy. The collection of snow is also far less laborious than the carting of ice, as the snow may be gathered in the immediate neighbourhood of the homestead. Let us hope that English dairy farmers will not be slow to adopt the scientific methods of their continental brethren.

Statistics regarding the meat-producing capabilities of the United States and Canada are fully given by Profs. Sheldon and Alvord. The number of cattle in the United States is at present about 28,000,000, or three times as many as those in Great Britain and Ireland. The proportion of cattle to population is, in the United States and Canada, about 67 : 100; while in the British Isles the proportion is about 29 : 100. The total area of the farms in the United States is about $8\frac{1}{2}$ times that of the farmed land in the British Isles, while vast tracts of country yet remain to be cultivated. In 1875 the number of acres under Indian corn in the United States all but equalled the whole number of acres under cultivation in our own country.

With such enormous capabilities of production, the only condition wanting for a large export trade is a cheap and efficient means of transit. That such a mode of transit has now been established is proved by the quantities of meat already exported to England. We received in 1876, from New York and Philadelphia, 19,838,895 lbs. of fresh beef; and the trade has so rapidly extended, that in the first four months of 1877 the imports exceeded the whole import of the preceding year, and amounted to 22,812,128 lbs.

The means adopted to preserve so perishable an article as fresh meat during the long journey from America to England is artificial cold. The cattle are slaughtered at the port of embarkation. At the establishment in New York an ox is killed, and the skin and offal removed in the space of three minutes. The carcase is then cooled to 40° F. in a room through which a constant current of cold air is maintained from an ice chamber. After forty-eight hours the carcase is cut up, and placed in the refrigerators of the steamer, and thus conveyed to England. During the voyage a temperature of 37°—40° is maintained, a stream of dry cold air being circulated through the meat-chamber.

The source of cold has hitherto been ice, but a new cooling agent of great power and adaptability promises soon to supersede the use of ice. The invention is due to Messrs. Giffard and Berger, of Paris. In their process air is condensed by a steam-engine, the heat evolved on condensation being removed by a stream of cold water. The cool condensed air is then conveyed to the chamber which is to be refrigerated, on entering which it is allowed to expand again to atmospheric pressure. The cold thus produced is intense. The ease with which the cooling power can be conveyed to distant places, and the fact that ventilation, as well as cold, is accomplished, will probably procure numerous applications for this valuable invention.

For the extension and success of the American meat trade we now only require to erect suitable refrigerating stores, and to provide refrigerating railway-cars, for the safe conveyance and preservation of the carcase after it has reached our shores.

We have no space to refer in detail to the remaining articles. Those who feel an interest in the improvement of the agricultural labourer will find much suggestive matter in the papers on village clubs, while the kindred

subject of the improvement of peasant farmers is ably discussed in Prof. Baldwin's report on the Irish prize farms. R. W.

OUR BOOK SHELF

Oregon: its Resources, Climate, People, and Productions. By H. N. Moseley, F.R.S. (London: Stanford, 1878.)

THIS little manual is the result of a visit paid in July and August last by Mr. Moseley to Oregon. Mr. Moseley gives not only the results of his own observations, but has taken the trouble to consult carefully and give the gist of official publications on the state, the result being a thoroughly satisfactory, full, and trustworthy account of the present condition of Oregon. Mr. Moseley has done a public service in undertaking this task, and we recommend his book to all who contemplate emigrating. It will answer nearly every question an intending emigrant is likely to ask, and gives, moreover, very definite advice as to the kind of people for which the state at present is suited. The book contains an excellent map of the state.

A Handbook of Common Salt. By J. J. L. Ratton, M.D., M.C. Madras College. (Madras: Higginbotham and Co., 1877.)

THIS work is not to be judged as a scientific treatise, but as a practical guide to the manufacture of common salt from sea-water. The author has fulfilled the purpose which he set before himself in compiling the book. Starting with a brief historical introduction, he proceeds to lay before the reader a concise statement of the principal chemical and physical qualities of salt. The occurrence of salt as a mineral is then shortly discussed; the analysis of natural salt occupies a small chapter, which is succeeded by others upon the hygienic value of salt, and upon the agricultural uses of the same substance. The principal rock-salt deposits are described, and the mining operations sketched.

After these chapters, which must be considered as introductory, the composition of sea-water is discussed; the leading facts concerning evaporation of solutions of mixed salts, and fractional precipitation of the saline substances, are clearly laid down, and upon these the theory of salt manufacture is shown to be based.

Details of the salt manufacture are then given, followed by descriptions of the growth of "spontaneous salt," of the manufacture of salt from brine springs, of "earth salt," and lastly, of salt lakes. The final chapter is devoted to a discussion of the bearings of taxation upon the salt trade.

The book is written from the Indian view-point, and is rich in local illustrations of the manufacture; but the author has endeavoured to make, and we think has succeeded in making, the work a really good manual of general applicability.

The author is to be praised for the carefulness with which he has gathered together and arranged a large mass of facts; the result is a most useful and convenient little book of reference. M. M. P. M.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

The "Phantom" Force

THE famous principles of conservation and dissipation of energy, which have done so much to promote the progress of physical science in recent years, were undoubtedly first inferred and generalised from certain similar laws in the theory of forces which, as we find noticed by Prof. Tait in NATURE (vol. xiv. p. 462), were first propounded by Newton.[1] If in any mechanical system, Newton observes, urged by any forces, to which we must add those which arise from friction, the action of a force reckoned *as a gain* in the system is measured by the product of its impulse and the space through which it is pushed back, or as a loss in the system when the product relates to a space through which the force *is allowed to act*, and if as action of the same kind in the system we also count its gains and losses of actual energy of motion, the whole amount of action in the system remains unchanged during the motion. Viewed from the standpoint of the laws of motion, force, and matter, which Newton starts with in the "Principia," and keeping in mind the special definition here given (coinciding with the modern term "potential increase") of the "action" of a force, obviously the reverse of what would vulgarly be called the action of a force *in increasing* a body's energy of motion, this proposition at first looks like a truism; but the idea of potential energy here coined by Newton[2] is really an essential one; and it besides allows the mode of action of some forces of very common occurrence in nature to be described more simply than they could be without it. The force of gravitation, of attraction and repulsion between two bodies permanently electrified or magnetised, and all dual forces or actions and reactions directed along, and depending only on the distance between two bodies, and not at all upon the time, are of this kind. The force can be completely described in these cases (and it may be looked upon in the first instance as only a measure of convenience) by the permanent gradient of energy-variation everywhere; and hence also by the permanent change of energy from one distance to another, when, as is supposed in this example, the dual force pair acts along the line of centres; since then the changes of actual energy which it produces (acting alone upon the bodies) are independent of the rotation of this line, and may be regarded either as produced with the natural motion of this line's rotation or by the same forces acting along a fixed line of centres. When two such bodies approach, or recede from each other, whatever time elapses or whatever course they may pursue about their centre of mass, not only are the momentary transfers between actual and potential energy equal in energy value at every moment of the motion (for this is *general*, and by this condition *only* the bodies returning twice to the same distance from each other might have very different energies of motion at the two returns); but the whole energy of motion which can be gained between two distances is a definite one, and as this would not be so if the bodies returned twice to the same distance with different actual energies, nor if they returned twice to the same distance with different potential energies, it follows at once that not only is the sum of the actual and potential energies at any one distance invariable with the lapse of time and with any intervening motions of the bodies, but since the gain of actual energy from this distance to any other *is the loss* of potential energy, the sum of these two energies is also the same at one distance as it is at another, and it therefore varies neither with the time nor with the distance of the bodies from each other.

In this illustrative example of two bodies (otherwise unimpelled) exerting upon each other a permanent action and reaction, several points connected with the use of the term "potential energy," as just described, require attention. In the first place, whatever the real forces are (acting in "absolute space"[3]) upon the two bodies, the Newtonian laws of motion

[1] On reading the passage again (which I here described from memory) I find that its statement is verbally but not substantially different from what I wrote above, and that in Newton's statement the signs are merely taken oppositely. Newton thus describes an "acceleration" (a gain of actual energy) as a "resistance" (*i.e.*, a force) overcome, with a corresponding loss of action in the system. This is the modern view of equivalence between potential and actual "action" or energy, but *with the signs of* these actions changed.

[2] Newton, in fact, anticipated D'Alembert's principle; and if we apply D'Alembert's principle to the motion of a single particle, the way in which it likewise coincides with the modern definition or recognition of potential energy will presently be understood, although it also reverses the signs of both of the energies concerned.

[3] The term "absolute space," or the simpler word "space," used in Newton's enunciations of the laws of motion as the field of action of "force" is nothing more than a space whose origin is either the centre of mass of all the bodies under actual observation, or any space in which that centre is moving uniformly in a straight line. If we extend our observation to new bodies found not to be moving uniformly in the original space, the old space must be given up, and a new one must be adopted (recognising the new masses), to enable us to state all the forces and to describe the motions completely, of all the bodies under observation (which is the sole problem and

establish that their whole effect in altering the energy of motion of the two bodies at any instant is divisible into two parts, that which the forces, removed to the centre of mass of the pair, and acting there on their joint mass, will have on the joint mass in absolute space, and that which is represented by the sum of the bodies' changes of actual energy reckoned in a space which has this centre of the masses for its origin. If we call the latter changes their local changes of energy, and professing ourselves entirely ignorant of motion and position in absolute space, confine our attention to describing the motions of the bodies in the specified or local space, the abstract laws of dynamics again tell us that in this local space the motion of the bodies is what arises from an equal and opposite action and reaction exerted mutually between them. Suppose this to be of the permanent kind above described (which occurs frequently in natural actions, as already mentioned), then as regards the local motion and its forces (now equal and opposite, and quite distinct from what they were abstractly), the above proposition may be predicated of them which asserts that the local energy of motion and local potential energy together have a constant sum. In our circumscribed sphere of observation the energy of motion is entirely known, or in other words, if we follow the bodies along any course from one point to another, not only all the changes and the sum of the changes of their actual energies, but also their energies at first, and therefore their energies at last, are known by a successive process of integration. We know from the permanency of the energy-gradients along the line of centres that the sum of the energy changes between the two given points is independent of the course or lapse of time in which the final point is reached. Instead, therefore, of making a new successive integration for every course, one such integration for all expresses the total change of energy between the points, and as this is possible for all points or configurations which the bodies can reach from their first configuration, if a scale of such energy changes reckoned from some starting one is made out for all the different distances from each other at which the bodies can be, the scale value will be nothing at the starting distance, and will have determined values at all other distances. We would use the scale by saying that the actual energy at any distance only differs from the scale value by the starting-energy to be super-added; or the excess of the actual energy above the scale value is everywhere constant, and everywhere equal to the actual energy at the initial point. This concise description of the motion, as far as the actual energy at any moment is concerned, accords with the mathematical usage of collecting variable quantities thus simply related to each other and to constant quantities on one side, and constant quantities on the other side of an equality; but a further simplification of its expression is effected if those scale values which mean increase of energy from the starting-point are called "negative," and those denoting loss or decrease of actual energy are called "positive"; for having constructed a new scale on this convention (which we may call the negative scale), to use it we must first change the sign of any value in it before applying the last proposition. As that expression tells us that the remainder, on subtracting the former scale value from the actual energy at any point, is constant, this operation of subtraction, after altering the sign of the new scale value, is simply equivalent to adding the new scale value without altering its sign. With this convention, therefore, that an increase of actual energy is a negative increase, or, in other words, a decrease of the negative scale value, we may put the sentence describing the actual energy in every part of the motion in these much simpler words. The sum of the actual energy and of the negative scale value is everywhere constant and equal to the actual energy at the starting-point of the scale, which we may call the initial actual energy. When increase of actual energy coincides with decrease of "negative scale value" (as we have just seen), and also as it is usual to express it with "work done by a force," increase of negative scale value represents work done against a force as it is expressed in the new phraseology of the science of energy, or with "potential work." The actual energy of the material couplet is everywhere fixed and determinate (when it is once started), but if we speak of the negative scale value as "potential energy" the amount of this at various distances depends upon the distance chosen as the initial one, when it is zero. Thus if we reckon the potential energy of a swinging pendulum, drawn by gravitation towards the centre of the earth (whose motions of rotation and of oscillation relatively to the common centre of the globe and of the pendulum-bob may be disregarded, so that, with the exception of gravity, only a force perpendicular to its motion guides the bob in a space, referred to the common centre as origin, which we may identify with the place of the experiment) from the top of the arc, where the actual energy of the bob is zero, this must be the sum of the values of the actual and potential energies throughout the motion, and consequently at the highest point the potential energy is zero, and everywhere else it is negative, while at the lowest point of the arc, where the actual energy is a maximum, the potential energy reaches its greatest negative value. If, on the contrary, we select the lowest point of the arc as the starting-point, and call the potential energy at this point zero, making the sum of the two in all parts of the motion thereby equal to the greatest value which the actual energy can have, the potential energy must elsewhere supply the deficiency as the actual energy abates, or have positive value in all other positions of the bob, and at the highest points of its swing, when the actual energy entirely disappears, it will reach its greatest positive value, equal to the greatest value of the actual energy at its lowest point. By one such system, therefore, the motion is as perfectly described as by the other, and by a different choice of zero-points the individual amount of the potential energy is thus evidently disposable at pleasure, while its difference between two points yet always remains the same. But by taking the zero point where the actual energy has its greatest value, the advantage is obtained, as in the last arrangement for a pendulum, that the potential, like its partner, actual energy, will never be less than nothing, and its values will always be positive. With its zero point so taken, and with a special choice of mass in the moving body attracted or repelled, whose course is followed, the series of negative scale values or of potential energies just described is termed "the potential" or the "potential function" of the force upon it; but its definition for any permanent force-pair supposes the total absence of all such constraining forces as these of the pendulum string, and the bodies must be left perfectly free to approach or recede from each other to the centre, or to the furthest imaginable distance unimpeded by any forces foreign to the pair. In such material couplets it is also sometimes customary to reckon their combined energies actual and potential in a space having for its origin one of the bodies themselves instead of the centre of their mass. The motion of the standard body then disappears, and that of the other body becomes the relative motion of the two, while at the same time a certain mean mass must be supposed centred in the moving body, so that when the product of this, multiplied by its new acceleration, is taken, its impulse relatively to the stationary body (which is now the rate of change of energy of the pair with the distance between them) may not undergo any alteration by the change of origin. Reckoned in this way, either of the bodies may be said to have energies of motion and configuration in the space relative to the other body, whose sum is constant.

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(*To be continued.*)

purpose of mechanics). If we continue this process until all the bodies of the material universe are brought, with a knowledge of their masses, under our observation, we reach that abstract field of force, or force-space, which is contemplated in Newton's enunciations. This space may be identified with absolute space, because the centre of mass of the universe by which it is defined is as perfectly abstract and metaphysical an idea as any that we can form of absolute space, on the simple ground that we have no reason to attribute to matter a less boundless and limitless extent in the universe than we ascribe to space itself. To define one metaphysical idea by another is not unscientific, nor is the description of force which Newton gives more repugnant to the eyes of common sense than the ideas which we form, though quite indefinite, of the extent of the material universe, and of the boundless realms of space. A special office, it may also be suggested as very probable, may be assigned to force, to avoid the occurrence of superposition and mingling of matter in the same points of space, or to give matter impenetrability.

Aid of the Sun in Relation to Evolution

IT is not often that it will fall to the lot of the physicist to harmonise such important theories as those of evolution and the nebular hypothesis, and much credit is due to the boldness and the originality of Dr. Croll's attempt to do this. At the present time the great majority of scientific men hold the truth of both of these hypotheses in spite of the fact that serious difficulties exist in them which admit of only doubtful explanation, so that it is certain they would be considerably strengthened if it were found possible to dovetail them one to the other without unduly straining the conditions of either. That Dr. Croll has effected this important service is, I think, very questionable, although I fully believe it is attainable.

In advocating his own views in NATURE (vol. xvii. pp. 206, *et seq.*), and in his other publications Dr. Croll has anticipated

two formidable objections which he foresaw would be brought against them, namely, the improbability of two bodies endowed with enormous energy in the form of rapid motion coming into actual collision with one another, and secondly, the want of experience of like movements in the universe. It is but seldom that a theory, however ingenious, can be upheld against *two* antecedent improbabilities, but granting Dr. Croll all he asks, even to the existence of non-luminous bodies moving through space with a velocity of 1,700 miles per second, there may still be brought more serious objections than either of the above. Our knowledge of the actual motions of the stars in space has recently been greatly extended, and it is now well known that proper motions exceeding thirty miles per second are very rare, and that probably there is no well-authenticated case of a velocity greater than forty miles per second. It has long since been ascertained also that the proper motion of our own sun in space is at the rate of four miles per second only. It is, of course, possible or fortunate that the two bodies from whose collision the solar nebula originally derived its vast stores of heat might be of such equal masses and velocities that the motion of translation should be so nearly destroyed, and the whole converted into heat, but it is inconceivable that amid all the diversity of dimensions of the heavenly bodies it should invariably happen that the resultant movement of the combined masses should be reduced to such insignificant figures as the above.

It is strange that it should not have occurred to Dr. Croll that the heat generated by the impact of two bodies in such rapid motion cannot be considered as remaining constant for nearly the length of time he computes, because the rate of radiation from so intensely heated a sun will be enormously greater than it is now. Indeed the origin of the solar heat does not materially affect the question at issue, which is rather of the means of continuous and equable supply than of the primary source. The contraction theory of Helmholtz addresses itself to meet this difficulty, but alone it is probably insufficient. In the *Popular Science Review* of January, 1875, I have directed attention to other possible and supplementary means of heat supply, which, being continuous, will tend to prolong the period during which the radiation of heat from the sun shall be nearly constant, and hence favourable to the development of organic life. Without advocating any peculiar views of my own which recent discoveries have necessarily somewhat modified, I content myself with pointing out what appear to me to be grave difficulties in the way of accepting the theories and explanations of Dr. Croll.

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Faraday's "Experimental Researches"

DOUBTLESS many of your readers will have observed an advertisement of a well-known antiquarian bookseller professing to be able to supply "a perfect copy" of Faraday's "Experimental Researches" at a price not too exorbitant for a complete original copy of that priceless work.

Any who may have applied for the work will, perhaps, share with me the indignation with which they discover that the so-called *perfect copy* is only such in virtue of being a "facsimile reprint" (*sic*) not twelve months old, though dated on the title-page 1839. But perhaps scientific men are too innocent of the ways of antiquarian caterers to receive with calm contentment the assurance that they have not been deceived.

SILVANUS P. THOMPSON

University College, Bristol, February 5

CLAUDE BERNARD

IN rapid succession we are compelled to chronicle the recent serious losses by death to French science. To the names of Leverrier, Becquerel, and Regnault, we regret to add that of the equally famous physiologist, Prof. Claude Bernard, who died in Paris on the evening of February 11. He was born at St. Julien, near Villefranche, in the Rhône department, July 12, 1813. After completing a course of study in the Paris faculty of medicine he was appointed hospital-surgeon in 1839. Two years later he became assistant to the well-known physiologist, Prof. Magendie, in the Collége de France, and continued in close connection with him for thirteen years, during the last half of this time lecturing himself as *privat-docent.* A series of notable discoveries made during this period caused his election, in 1854, to the Academy of Sciences, and his appointment to the newly-founded professorship of general physiology in the Collége de France. This he exchanged in the following year for the chair of experimental physiology, a position which he occupied up to the time of his death.

As an original investigator, Bernard stands among the foremost of the century. He entered upon his career at the epoch when Magendie, the chief founder of the modern French school of physiology, had completely altered the character of this study by the introduction of a variety of experiments on living animals, such as the action of the alkaloids, &c. Bernard entered with enthusiasm on the new field of experimental activity opened up by his master, and by a swift succession of remarkable discoveries with regard to the changes taking place in the human organism, guided the young science into a completely new channel. Of these the most important were connected with the phenomena of digestion, and especially the relation of the nerves to these processes. Perhaps the most valuable was the exhaustive investigation into the functions of the pancreatic juice (in 1850), in which he showed that this fluid was the only one in the digestive apparatus capable of so modifying fatty matter that it can be absorbed by the chyle ducts, and that the digestion of this portion of the nourishment introduced into the system was its sole purpose in the animal economy. Another discovery at this period, which attracted universal attention, was that of the saccharine formation in the liver. Bernard found that not only was sugar a normal constituent of the liver, but that while the blood, on entering into this organ, was completely free from saccharine matter, large amounts of the latter could be detected after it left the liver to pursue its way to the heart. Interesting as this fact was, it was eclipsed by the discovery of the two remarkable connections between this function of the liver and the nervous system.

It was ascertained, first that this normal formation of sugar in the liver could be totally interrupted by severing the pneumo-gastric nerve in the neighbourhood of the heart; and secondly, that by wounding a certain place on the fourth ventricle of the brain, near the origin of the eighth pair of nerves, it was possible to cause such an abnormal formation of sugar that an animal within two hours after such an operation showed all the symptoms of diabetes. In recognition of these brilliant experiments the physiological prizes of the French Academy were bestowed upon Bernard in 1851 and 1853. In a continuation of this line of research in 1859 he made the important discovery that the sugar for the embryo is prepared in the placenta, and not in the liver. Shortly previous to this time he published the results of extensive observations on the temperature of the blood, in which he showed that remarkable alterations in the degree of warmth take place on the passage of the blood from one organ to another, especially in the different parts of the digestive and respiratory systems. The absorption of oxygen by the blood formed the subject of a memoir in 1858, from which it appears that the coefficient of absorption diminishes gradually with an increase of temperature, and becomes zero at 38°—40° C. in the case of mammals, and 40°—44° C. in the case of birds, viz., at the temperatures at which death sets in. The respective amounts of oxygen in the arterial blood, and red and black venous blood were likewise carefully estimated, and the chemical causes for the differences in colour revealed. Among the other leading researches of Bernard must be mentioned those on the comparative properties of the opium alkaloids; on the poisonous properties of curarine; on the sympathetic nerves in general; as well as numerous investigations on the individual processes in the act of digestion. Many of these discoveries, as well as the results deduced from them, have formed subjects for long-continued controversies. With rare exceptions, however, not only

Bernard's experimental correctness, but the soundness of his theoretical deductions, have been universally recognised by leading physiologists.

As an author Bernard was not so fertile as most of the scientists of the present day in France. The few works emanating from his pen are regarded as standard even outside the limits of his own country. This is especially true of his "Leçons de Physiologie Expérimentale Appliquée à la Médecine" (1865), a work valuable not only for the exceedingly thorough, systematic, scientific treatment of the subject, but also on account of the numerous indications for the application in medicine and surgery of the results gained by physiological research. His other works are "Leçons sur les Éffets des Substances Toxiques et Médicamenteuses," 1857, "Introduction à l'Étude de la Médecine Expérimentale," 1865, and "Leçons de Pathologie Expérimentale," 1874.

As a lecturer Prof. Bernard was not only peculiarly successful in the professor's chair, but was also distinguished among the *savans* of Paris for his able and lucid presentation of scientific facts to general audiences. He was busily engaged in the fulfilment of his professorial duties when the short sudden disease preceding his death interrupted the courses of lectures, and put an end to a life of rich and varied scientific activity.

As a mark of the universal respect and honour in which he was held, the authorities of the French Republic have decided that his funeral shall be at the expense of the nation. T. H. N.

A PHYSICIAN'S EXPERIMENT

AT a public lecture at Salisbury Hall, Oxford Street, recently, Dr. T. L. Nichols, of Malvern, related particulars of a "Dietetic Experiment" upon himself, which he had made with a view to solving a difficulty as to the quantity of food per diem which would best sustain health. He had always been temperate, his only excess being to be overworked. He rose between five and six, and worked well through the day, but avoided night-work. He seldom knew pain, never took medicine, and had excellent health. He usually ate twice in the twenty-four hours, at nine and five, because, for him, long rest for the stomach was better than shorter intervals. He appeared to sleep better for not eating after four o'clock. Every one should sleep upon, at least a quiet stomach. He had carefully noted the "dry weight" of the food he had taken, oatmeal, &c., he counted as dry weight. The weight of water forming a large portion of all food had not been reckoned, because it did not supply nutrition. Eggs and milk were perfect foods, but were largely composed of water. Milk was the most perfect food, though not the best for adults. He began on November 5, his food being chiefly bread, fruit, milk, and vegetables. During the experiment he had taken no flesh meat, wine, beer, spirits, tea, coffee, or tobacco. With regard to smoking, if it were the good thing people said it was, why not encourage their wives and daughters to smoke? Medical authorities differed as to the quantity of food that should be eaten, and it was a common belief that the more food we ate the greater would be our strength.

The first week, the lecturer stated, he lived on bread, milk, fruit, and vegetables, the total weight being 3lb. $9\frac{1}{2}$oz., costing 3*s.* 1*d.*, *i.e.*, a daily average of $8\frac{3}{14}$oz., costing $5\frac{2}{7}d.$; this was slightly below his standard of 6*d.* a day. He felt better, and clearer, and brighter than usual. The second week he studied quality rather than cheapness, his food being Food of Health, milk and fruit. Total weight 4lb. $4\frac{1}{2}$oz., cost 3*s.* 8*d.*, average per diem $9\frac{5}{7}$oz., costing $6\frac{2}{7}d.$, and nothing could have been better, physiologically, than the effect of that food upon him. His digestion was simply perfect, and the action of the whole system as good as it could be. He then discontinued milk as unnecessary. For the third week the total amounted to 3 lbs. 2 oz. = 1*s.* 9*d.*, giving an average of $7\frac{1}{7}$ oz. of food costing only 3*d.* per day. Milk was not so cheap for food as Gloster, Dutch, and American cheese; because they had to pay for the water it contained. Doctors recommended 2 or 3 lbs. of food daily to repair the waste of the system; but he asserted that the waste of brain atoms and nerve force could not be measured. The food eaten had to be disposed of at great cost of life and strength, and he believed the wisest plan was to eat the smallest quantity that would properly support the body. The fourth week, his food being similar, weighed 3 lbs. 6 oz., costing 1*s.* $2\frac{1}{2}d.$, giving an average of 8 oz. = 2*d.* per day. He considered *8* oz. the *minimum* and *12* oz. the *maximum* quantity of food that should be taken per day. The total weight of his food during the four weeks was 14 lbs. 6 oz., costing 9*s.* $8\frac{1}{2}d.$, average per week 3 lbs. $9\frac{1}{2}$ oz.; per day $8\frac{1}{2}$ oz., costing per week 2*s.* 5*d.*, and per day $4\frac{1}{7}d.$ He then added soups, puddings, eggs, &c., and the fifth week his food weighed 3 lbs. $12\frac{1}{2}$ oz., costing 3*s.* 4*d.*, being at the rate of $8\frac{4}{7}$ oz., a $5\frac{5}{7}d.$ per day. For the sixth week the figures were 63 oz., at 2*s.* 1*d.*, or 9 oz. at $3\frac{4}{7}d.$ per day. He had taken the diet without stimulants and had experienced a constant increase of health and strength and power to work, and his weight had remained at about 12 st. 2 lbs., except that at the end of the fourth week there had been a slight decrease which had since been recovered. The experiment had been fairly made upon an average subject and the results were satisfactory. He was convinced that they ought to give rest to the stomach, and that this would cure all cases of dyspepsia. The diet question was at the root of all diseases. Pure blood could only be made from pure food. Proper attention to diet would reduce the rate of infant mortality and remove many diseases. If the drink of a nation were pure and free from stimulating qualities and the food was also pure the result would be pure health.

SOCIAL ELECTRICAL NERVES

OUR modern Mercury since the year 1846, when the first system of electrical highways was laid down from the metropolis to Norwich, Southampton, Crewe, and Exeter, has not been idle. The wonderful development of the laws enunciated by Wheatstone which regulate the transmission of electric currents through solid conductors has resulted in some very remarkable inventions. At the date at which we write, from a crude beginning when with difficulty electric speech could be conveyed to such limited distances as Manchester and Norwich, we are now able to record the transmission of the Queen's speech to the confines of the empire in a few minutes.

Since the first introduction of private and social telegraphy in 1861, when Reuter first proposed to connect the Reporting Gallery of the House of Commons with the editor's room of each of the leading metropolitan newspapers, the electrical wire has become the means of reducing the cost of newspapers and of sending the news almost simultaneously over the country. Before that time the press paid large sums for "special correspondents," and papers were exceedingly jealous of each other's privileges.

Year by year the public have reaped additional advantages. Submarine telegraphy now includes within its grasp New Zealand, Japan, and the western shores of South America. The private wire system of alphabetical telegraphy between offices and works, carried out over the chief centres of the United Kingdom by Holmes in 1861–5, is in still further process of development. The express speed of the Wheatstone automatic system, duplex and quadruplex telegraphy, and the telephone of Bell, with its delicate electrical sound-wave indications, have all passed into practical existence and become the property of the civilised globe. Still, notwithstanding the advances indicated, much remains to be done.

A recent remarkable advance in the arrangements necessary for utilising the transmitting power of the electric fluid over the metallic nerves of speech we propose to bring briefly under notice.

In every electrical circuit, so far, the limit of usefulness has been restricted to the number of speaking stations or instruments that could effectively be placed in circuit upon the wire, and by the interference and confusion that arises when more than one instrument is used at the same time on such a circuit. To place upon an electrical circuit more than eight or ten instruments has been practically found impossible, the resistance of the instruments themselves being no small element of trouble, while the interference and interruption from multiple speaking has hitherto been found an insuperable difficulty, and one that has greatly tended to clip the wings and usefulness of our modern Mercury. A system that will obviate this trouble and enable any number of instruments to be placed in connection upon the same circuit without the possibility of interference or confusion, opens up a new era in the usefulness of the telegraph as applied to social purposes. It is such a system that will now be described, a system that promises to revolutionise the systems that at present spread over our chief manufacturing cities, and guard the security of property.

A simple illustration will explain the principles of this auto-kinetic system. Let us suppose a tramway to be laid down through the streets and suburbs of any of our large manufacturing centres; the two rails will thread the thoroughfares in every direction, and at each junction, or point of deviation down a bye street or other divergence, a set of points are laid. There is practically no limit to the number of these points that may be placed along the line; they may be one or one thousand. They remain quiescent and of no value as far as the effective running of the car upon the tramway is concerned until the car passes over the special set of points that happen to be required in the transit of the car from its starting-point to its destination. The other nine hundred and ninety-nine sets of points remain ready for use whenever the car has occasion to pass over them, and their presence does not in any way impair the usefulness of the tramway. The one set of points brought into use has been effective in so far that they have enabled the car to reach its destination, and, having been used for a moment, they have again reverted to their original position; while the fact of their being used has in no way affected the utility or efficiency of the remaining points should any be required to pass a car.

Again, suppose two or three cars to be running over various sections of the tramway at the same time, each car could pass over its points on its journey without detriment to the others, although all the cars might be passing points upon the tramway at the same instant of time; the using of these two or three sets of points would not interfere with the remaining 990 odd sets of points which at any moment might also individually be called into requisition. Now the system of electric circuits to be described may be likened to that of the tramway-line, with its accessory junctions and points. A system of two parallel wires is carried through a town. These wires in pairs may be supposed for the purpose of the present explanation to ramify continuously in every direction from a central station up this street and down that, and to embrace within their area the entire commercial and social community. Like the points in the tramway system, so upon the metallic circuit laid down, speaking instruments may be placed at various points and stations along the route, one or 1,000, because in the auto-kinetic system under notice, no instrument is in circuit unless it is, like the points on the tramway-line, being used. A car going over the points makes those points for the time being a portion of the tramway-line. So the circumstance of using the instrument upon the auto-kinetic system makes that instrument for the time being a portion of the electric circuit, and the wires are alone occupied by this transmission.

Should any second or third instrument in other portions of the circuit be brought into requisition at the same interval of time, no interference can take place. As no two cars could run over the same points on the tramway at the same moment, so no two instruments in the system under notice can speak at the same time, but the second or third instrument will automatically succeed the first in the order in which they stand along the line from the central station; just as two or three cars would pass the tram points in the order in which they had been placed upon the line.

The value of this new system of arranging metallic circuits and the instrumental connections, whereby the instrument is only a part of the electrical circuit so long as it is speaking, being thrown off immediately upon the cessation of the speaking current, cannot be estimated or appreciated except by a special reference to its practical development as regards the public and social telegraphy of a large city. This will be fully demonstrated in a subsequent paper by reference to the system of police, fire, and social telegraphs proposed to be shortly carried out for the Corporation of Glasgow, a system at once the most comprehensive and complete that has as yet been devised for affording multiple speaking stations upon the same conducting wires without possibility of interference or confusion.

(*To be continued.*)

OUR ASTRONOMICAL COLUMN

THE STAR LALANDE 19,034.—It is somewhat singular that this star, which was observed by Lalande, on March 21, 1797, and then rated $4\frac{1}{2}$m. should have been so little observed since that year. It is not in Piazzi or Taylor, but it was observed three times by Argelander in the Bonn southern zones, viz., Z. 283, March 6, 1850, when it is called 6m.; in Z. 358, February 16, 1851, where we find it estimated 4m., and again in Z. 400, March 8, 1852, where it is 5m. These circumstances taken together appear to point to considerable variability. The star is in an isolated position on the borders of the constellations Hydra and Antlia. The mean of the Bonn observations gives for its position 1850·0, R.A. 9h. 34m. 26·40s., N.P.D. 112° 54′ 41″·0. Lalande's R.A. is one minute less than Argelander's—yet it looks right in the *Histoire Celeste*. Perhaps one of our meridional observers may find opportunity to revise its position and the star may be further recommended to attention on the score of probable fluctuation of light; though it should be remarked that there are other cases of discordant magnitudes in the Bonn southern zones for stars not yet entered on the list of variables, as in η Canis Majoris for instance, for which in three observations the magnitudes are 5, 3, and 2.

VARIABLE NEBULÆ.—Prof. Winnecke in directing attention to the nebula H. II. 278 as probably affording the first indications of *periodical* variability of a nebula, refers to the one discovered in Taurus on October 11, 1852, by Mr. Hind, as affording the single case where astronomers generally have been agreed as to variation. That nebula was detected on the morning of October 11, in one of the most magnificent skies experienced in the Regent's Park, being caught at once in slow sweeping, with the low power-comet eye-piece of the 7-inch refractor. Towards the end of the year 1876, in a fine sky with the same telescope and eye-piece, not a vestige of it was perceptible, and the same result attended several attempts to discern the nebula in 1874 and 1875. Prof. Winnecke mentions that it is not at present visible in our most powerful telescopes.

MINOR PLANETS.—Observers who are still engaged in the exploration of the region of the ecliptic have given

signs of much activity of late. First we hear of a small planet detected by M. Perrotin at Toulouse, on January 29, position at 10h. in R.A. 8h. 43m. 13s. N.P.D. 71° 41 twelfth magnitude, which appears to have been independently discovered by Herr Palisa at Pola, on February 1 : by an observation at Pola, January 27, it seems this object is not to be confounded with *Rhodope*, No. 166, of which a corrected ephemeris is given in the *Circular* of the *Berliner Jahrbuch*, No. 84, but it is there conjectured that it may be *Urda* No. 167, found by Prof. Peters 1876, August 28. Calculating from the elements of *Urda* in *Circular* No. 64, for the time of the Berlin observation of M. Perrotin's planet on February 3, it results that with the correction $\delta M = + 5° 24' 9$ the computed and observed longitude will agree, but there is a difference of $- 1° 38'$ from the observed latitude which, in the present case, throws doubt upon the presumed identity. Again, on February 2, M. Cottenot, at Marseilles, detected a planet, tenth magnitude, position at 13h. 2m. in R.A., 10h. 2m. 29s. N.P.D., 78° 51′, which was also found by Prof. Peters at Clinton, U.S., on February 4 ; this object is probably new. Finally, on February 6, Prof. Peters met with another planet, also of the tenth magnitude, in R.A. 10h. 16m. N.P.D. 76° 17′, which he notified to the Paris Observatory through the Smithsonian Institution by cable ; it is probable, however, that the presence close to this position of his previously-discovered planet *Antigone*, No. 129, has escaped his attention, and as its brightness would also be about equal to that of stars of the tenth magnitude, it is most likely to be the object observed.

The number of minor planets appears now to have reached 180, and possibly 181.

The Supplement to the *Berliner Jahrbuch*, for 1880, contains ephemerides for the present year, of the small planets to No. 172 inclusive, excepting only *Dike* and *Scylla*, for which the necessary materials are not available. Polyhymnia in opposition on August 30, in 11° S. declination, is distant from the earth 0·88 ; Atalanta in opposition October 27, declination 37° N., is distant 0·98, and Felicitas in opposition November 11, declination 30° N., is distant 0·92 ; these are the three cases of nearest approach during the year. Of the minor planets discovered since 1845, Hebe attains the greatest brightness – 7·4 m. in the middle of November, 1878, while in the neighbourhood of ϵ Eridani.

METEOROLOGICAL NOTES

ATMOSPHERIC MOVEMENTS.—A first paper on this subject, by Mr. Ferrel, has been published by the United States Coast Survey Office, in which the inquiry is limited to an investigation into the mechanics and general motions of the atmosphere which are dependent on wide-spread and periodically-recurring disturbances. In consideration of the enormous difficulties in the way of investigating the effects of friction, the author adopts the only course open to him, viz., to introduce unknown functions into the equations representing the resistances from friction in the direction of the co-ordinates, leaving these to be determined approximately from a comparison of the final results deduced from the equations with observation. From a mathematical examination of the question it is concluded that in whatever direction a body moves upon the surface of the earth, there is a force arising from the earth's rotation tending to deflect it to the right in the northern but to the left in the southern hemisphere ; and that this deflecting force is exactly the same for motions in all directions, so that if any sensible effects of this sort arise in the case of rivers or of railroads running north or south, the very same effects must take place where they run east or west or in any other direction. The amount of this deflecting force is exactly double of that which is obtained in accordance with the principle adopted by Hadley. An elaborate examination is made of the distribution of temperature over the earth, the most important of the results being that the mean temperature of the whole surface of the earth is 60°·2, the mean for the northern hemisphere being 59° 5, and that for the southern hemisphere 60°·9. With reference to this result Mr. Ferrel remarks that if important data collected by Dr. Hann for the extreme southern latitudes had been at hand while he was engaged with the investigation, the results obtained for the mean temperatures of the two hemispheres might have been nearly equal. This result, which is essentially different from the commonly received opinion, has, it is obvious, important bearings on many questions of terrestrial physics. The distribution over the globe of atmospheric pressure is similarly examined with results of great importance in their relations to meteorological theories. The coefficient of the annual inequality of pressure in North America amounts to only about one-third of that of the interior of Asia, from which the important conclusion is drawn that the difference between Asia and America in this respect does not depend so much upon the difference in the extremes of temperature of the two continents, which is inconsiderable, as upon the difference in the extent of the two continents. The annual maximum of barometric pressure for the United States, except the Pacific coast, occurs about December 23, which is about sixteen days earlier than in Europe. In both continents the time is considerably earlier than the time of the minimum of temperature. The distribution of temperature and pressure and the prevailing normal winds of the globe are shown on seven well-executed maps. In succeeding papers Mr. Ferrel intends to investigate those disturbances in the distribution of temperature and humidity which are of a comparatively local character, and which result in the locally developed phenomena of cyclones and other storms ; and finally to apply the principles of atmospheric mechanics thus developed to the explication of oceanic currents.

CLIMATE OF INDIA.—We notice in a recent number of the *Isvestia* of the Russian Geographical Society, an interesting paper by M. Wojeikoff, being a sketch of the climate of India according to the recent works of Mr. Blanford, the reports of Mr. Wilson, and some notes taken by the author during his recent visit. M. Wojeikoff describes very clearly the main features of the climate, and accompanies his description by some tables which illustrate the prevailing and characteristic directions of the winds. Besides, by a comparison of the temperatures of some places in India and South America, situated the one in parts devoid of forests, and the others in places where the forests are yet preserved, M. Wojeikoff shows what a great influence forests have on climate, and he arrives at the conclusion that the absence of great heats and a continuous humidity of air are always met with at those places which, however far from sea, are situated in forest lands. He concludes, therefore, as to the importance of preserving the forests in India, and expects that detailed observations would yet more show their importance as well as the beneficent influence of the irrigation on climate.

LOW BAROMETRIC READING IN THE HEBRIDES, NOVEMBER 11, 1877.—We have received from Mr. Buchan, Scottish Meteorological Society, a communication on this subject. The following readings of the barometer, reduced to 32° and sea-level, were made by Mr. Youngclause, the Society's observer, at Monach Lighthouse (57° 31′ N. lat., 7° 42′ W. long.), on November 11, at 9 A.M., 28·330 ; 11 A.M., 28·120 ; 12.43 P.M., 28·008 ; 1.30 P.M., 27·912 ; 4.20 P.M., 27·861 ; 8 P.M. and 9 P.M., 27·752 ; and at 9 A.M. of the 12th, 27·968 ; and at 12·43 P.M., 28·038 inches. Thus for nearly twenty-four hours the barometer at this place was under 28·000 inches, and fell to 27·752 inches, the observer remarking that the rise which followed proceeded at a very slow rate.

At Monach, on November 15, at 12.43 P.M., the barometer was 29·703; 9 P.M., 29·051; 11 P.M., 28·807 inches; after which it began to rise, and at 9 A.M. of the following morning it had risen to 29·828 inches, a fluctuation of nearly two inches having taken place during the twenty hours ending 9 A.M. of the 16th. The storm accompanying this depression of the barometer rose at 10.30 P.M., about the time of lowest pressure to the force of a true hurricane, the worst the observer had ever seen during his twenty years' service as a lightkeeper. At the same dates, at Thorshavn, Farö, the readings of the barometer were—lowest at midnight of the 11th, 28·119 inches; 15th, at 9 A.M., 29·002 inches, and at 9 P.M., 29·350 inches, the barometer thus rising a third of an inch in Farö during the time that it fell about an inch in the outer Hebrides, accompanied by a storm of extraordinary violence, being the heaviest storm experienced in the north-west of Scotland generally for very many years.

CUMULATIVE TEMPERATURES.—To simplify the difficulty of obtaining sums of temperature (a highly important climatological factor, particularly in its application to agriculture) for any district, from the ordinary instruments, M. von Sterneck has recently proposed to obtain these indirectly by observation of the sums of actions produced by the temperature. A suitable apparatus for this we have in the pendulum-clock. The course of this represents the sums of the heat-changes, since it represents the sum of the changes of length of the pendulum, produced by different temperatures, which changes cause variations in the time of oscillation. As the laws of pendulum vibrations and the expansion of substances through heat are known, the true sums of temperature can be deduced from the going of the clock. While the watch-maker is concerned to obtain as uniform working as possible, and uses arrangements to compensate the changes in length of the pendulum, the present case requires that these changes should be brought into prominence; so the pendulum is made of some substance (like zinc) which expands greatly through heat. The clock will reveal the variations of temperature by its slowness or fastness, and by comparing its indications, at certain times, with those of a uniformly-going clock, the sum of divergences of the temperature from any given temperature will be ascertained. The principle of this method can also be applied (as the author shows) to determine the variations in atmospheric pressure and in the intensity of magnetism.

GEOGRAPHICAL NOTES

BRAZIL.—Mr. Herbert H. Smith has returned to Baltimore, U.S., after an absence of several years employed in scientific explorations in Brazil. Leaving the United States in January, 1874, for Pará, he ascended the Amazon to Santarem, where he was engaged for two years in collecting and studying the insect fauna of that region. Subsequently he extended his explorations to the north side of the Amazon and on the tributary rivers, as far as the base of the great northern table-land. A collection of insects made by him during this period amounted to 12,000 species, with 100,000 specimens, accompanied by copious notes on the habits, geographical distribution, &c. During 1876 and the early part of 1877 he was connected with the Brazilian Geological Commission in examining the geological structure of the country. He succeeded in making a section of the Devonian rocks of the Amazon region, and discovered a rich carboniferous bed on the north side of the Amazon, in the vicinity of Alenguer. The results of this labour are in the course of publication by Prof. Harrt, of the Geological Survey. Several months of his absence were spent in the southern part of Brazil, near Rio de Janeiro and Minas. Mr. Smith has been able to make some interesting inferences in regard to the geological distribution of Brazilian animals. Bates and Wallace have pointed out that the Amazon forms a limit to the migration of many animals. Mr. Smith is of the opinion that the flood plains of the valley, which average forty miles in width, constitute a far more effectual barrier than a body of water of the same breadth. Birds and insects of powerful flight pass this distance without difficulty, and are generally found on both sides; but the sluggish species, especially the wingless forms, like spiders, are generally confined to one side or the other. This is especially shown in those hymenopterous species in which the females are wingless, as the mutillarias, pezomactri, &c. Here the distinction between the northern and southern groups is very striking. The broad alluvial belt through which the Amazon flows constitutes a very distinct zoological province, in which many of the forms appear to have been derived from those of the high land. The contributions of Mr. Smith to geographical knowledge have not been inconsiderable. His maps of the physical geography of the Lower Amazon and of three important tributaries, the Curna, the Mæcurú, and the Jaurucú, are especially noteworthy. The last-mentioned has been entirely lost sight of by modern geographers, though referred to by earlier travellers. This enters the delta of the Xingú close to the Amazon, and is apparently navigable for steamers to a distance of 150 miles from its mouth. Mr. Smith returns to the United States for the purpose of making arrangements for continuing his explorations for several years longer, so as to accumulate a sufficient body of facts to work out satisfactorily the entire problem of the derivation and the geographical distribution of the insects of Brazil.

AFRICA.—Herr Schütt, who has been despatched by the Deutsche afrikanische Gesellschaft to equatorial Africa, has safely arrived in San Paul de Loando, and starts at once for the interior to complete the work of exploration commenced by Eduard Mohr, whose untimely fate we lately recorded. The series of geographical lectures in Berlin, delivered under the auspices of the Afrikanische Gesellschaft, was opened on January 23, by Dr. Nachtigal, who gave a graphic description of the African kingdom Darfur, which was conquered in 1874 by the Egyptians.

ARCTIC EXPLORATION.—We learn from *L'Explorateur* that Mr. Gordon Bennett, of the *New York Herald*, intends to equip an expedition for polar exploration.

THE ANGARA.—At its last meeting, February 5, the Section of Physical Geography of the Russian Geographical Society discussed the question of the expedition to be sent for the exploration of the Angara and of the water-divide between the Obi and Yenisei rivers, where, it is expected, a water communication could be established between the two main rivers of Siberia. An elaborate report was read, being a sketch of the present state of our knowledge of these tracts, and of the recent explorations of the water-divide; the route the expedition will have to follow was also discussed.

AN AZIMUTH INSTRUMENT.—Capt. Mouchez has presented to the Geographical Society of Paris a portable instrument for taking azimuths and altitudes in travelling. The weight is only a few pounds, and the experiments made at Montsouris show that the latitude can be taken with an error of a few minutes. This instrument is to be used by some travellers that the Paris Geographical Society is sending out to Africa. A single man can carry the apparatus and use it without losing much time. A complete observation requires less than a quarter of an hour.

NOTES

AT the meeting of the Linnean Society on Thursday last, it was unanimously resolved to send a congratulatory letter to von Siebold on the occasion of his jubilee. This graceful act, however, brings into prominence the neglect of the Society to take

any notice of the Linnean centenary, the celebration of which in Sweden, Holland, and Germany, were recently noticed in our columns. Of course the excuse may be urged with some force that such formalities are foreign to English habits, but perhaps an exception might have been allowed in the case of a Society which bears the name and jealously guards the collections, books, and manuscripts of the great naturalist. Perhaps, however, another reason may be found in the fact that the constitution of the Society places the initiative in every case in the hands of the officers whose tenure of office is practically indefinite, and who are not very accessible to any impulses of enthusiasm from the general body of the Society even if there were any permissible way by which expression could be given to them. Some disquieting rumours as to the present condition of the Society's business affairs, coupled with its rather troubled history during the past few years, seem to point to the desirability of some changes in its mode of government which would bring the executive into closer relation with the general body of Fellows.

We gave last week a list of the grants just made from the research fund of the Chemical Society; we are glad to state that since making these grants the fund has been increased by the following donations and subscriptions from the "Alkali Manufacturers' Association." The donations, amounting to 229*l*., are from Messrs. Charles Tennant and Co., 45*l*.; Messrs. J. and L. Pattinson and Co., 35*l*.; Messrs. R. Bealey and Co., 15*l*.; Messrs. Roberts, Dale and Co., 5*l*.; Messrs. James Muspratt and Sons, 35*l*.; Mr. A. G. Kurtz, 50*l*.; Mr. Henry Baxter, 25*l*.; Mr. C. J. Schofield, 5*l*.; Mr. Thomas Walker, 9*l*.; Mr. D. McKechnie, 5*l*. The following are the annual subscriptions to be continued for five years:—Messrs. Gaskell, Deacon, and Co., 11*l*. 14*s*.; Messrs. Chance Brothers and Co., 4*l*.; The Netham Chemical Company, 4*l*.; W. Pilkington and Son, 7*l*.; Mr. James McBryde and Co., 3*l*.; W. Gossage and Son, 4*l*. 10*s*.; Watson, Kipling, and Co., 2*l*. 18*s*.; amounting altogether to 37*l*. 2*s*.

The President of the Institute of Chemistry of Great Britain and Ireland offers two prizes of 50*l*. each, to be awarded by the Council of the Institute on February 1, 1879, for the two best original investigations involving gas analysis, and conducted by an associate of the Institute. The investigations must have been made within two years of the date of the award, and must not have been published, if at all, more than six months previous to the award. The prizes will not be awarded unless, in the opinion of the Council, the work is of sufficient merit to qualify the candidate for Fellowship of the Institute.

In his interesting communication on the analogy between chemistry and algebra in our last number, Prof. Sylvester attributes the conception of *valence* or *atomicity* to Kekulé. No doubt the theory in its present developed form owes much both to Kekulé and Cannizaro; indeed, until the latter chemist had placed the atomic weights of the metallic elements upon a consistent basis, the satisfactory development of the doctrine was impossible. The first conception of the theory, however, belongs to Frankland, who first announced it in his paper on Organo-metallic Bodies, read before the Royal Society on June 17, 1852. After referring to the habits of combination of nitrogen, phosphorus, antimony, and arsenic, he says, "It is sufficiently evident, from the examples just given, that such a tendency or law prevails, and that, no matter what the character of the uniting atoms may be, *the combining power of the attracting element*, if I may be allowed the term, *is always satisfied by the same number of these atoms*." He then proceeds to illustrate this law by the organo-compounds of arsenic, zinc, antimony, tin, and mercury. In conjunction with Kolbe, Frankland was also the first to apply this law to the organic compounds of carbon; their paper on this subject, bearing date December, 1856, having appeared in Liebig's *Annalen* in March, 1857, whilst Kekulé's first memoir, in which he mentions the tetrad functions of carbon, is dated August 15, 1857, and was not published until November 30 in the same year. Kekulé's celebrated paper, however, in which this application of the theory of atomicity to carbon was developed, is dated March 16, 1858, and was published on May 19, 1858. On the other hand, the "chemicographs," or graphic formulæ, which Prof. Sylvester has so successfully applied to algebra, were the invention of Crum Brown, although Frankland has used them to a much greater extent than any other chemist.

At the General Meeting of the Royal Astronomical Society, on February 8th, the Gold Medal was awarded to Baron Dembowski for his double-star measurements.

We learn from the *Diario de Campinas* of the death in that town, on December 20, 1877, of Joaquim Corrêa de Mello, a Brazilian botanist, who was well known as a correspondent to many scientific men in the Old World.

The Rev. Andrew Bloxam, M.A., rector of Harborough Magna, Rugby, formerly incumbent of Twycross, Leicestershire, died on February 2, aged 76. He was well known to British botanists, especially as a diligent student of brambles and roses.

A subscription has been opened at Paris with the view to erect a monument to the late M. Raspail.

Among the exports of Corsica it is said that there are annually between 350,000 and 400,000 blackbirds (*merles*) which are sent to this continent. They visit Corsica in vast numbers each winter to feed on the berries of the myrtle and arbutus, with which the mountains are covered. In the month of December they become very fat, and the flavour and perfume given to their flesh by their food cause them to be much esteemed by the *gourmets* of Paris. A *pâté de foie de merle* is a great delicacy.

Mr. Francis Day writes that in our notice of Dr. Bleeker last week, seven volumes of his "Atlas" are said to have appeared, whereas the first part of volume 9 has been issued to subscribers, and the second part will be shortly. The number of volumes which the work was intended to fill was twelve, the whole of the MSS. for which has been left complete, as well as most of the figures, and we may hope that they may yet be published.

We are glad to learn that Prof. Abich is preparing a complete edition of his numerous and well-known works on the Caucasus, under the title of "Forschungen in Kaukasus-Ländern." The first fascicule will contain a new paper on the coal-measures of the middle parts of the Araxus valley, with numerous plates; and the second, a description of the Trialet mountain-range and of its volcanic rocks and mineral waters, with a geological map on a large scale.

Prof. Leuckart has just issued, in Berlin, the first part of his "Bericht über die wissenschaftlichen Leistungen in der Naturgeschichte der niederen Thiere" for 1872–75, the continuation of the reviews which he has hitherto been accustomed to compile at intervals in this department of zoology.

The Société Centrale d'Apiculture et d'Insectologie has had constructed a pavilion in the Champ de Mars for the purpose of exhibiting in 1878, in the most complete manner, everything relating to the education of useful insects, especially bees, and the means of preservation of all kinds against noxious insects.

Quite recently we had a band of Nubians in London; a small band of Eskimo are at present encamped in Paris, and now, we hear, that shortly Europe will have an opportunity of viewing a group of Aborigines from the opposite side of America. A number of Tierra del Fuegans are to b brought to Brussels,

where they will be installed in the Zoological Gardens. The enterprising authorities of the Paris Zoological Gardens contemplate, moreover, importing some specimens of North American Indians, their plan evidently being to keep up a kind of anthropological review of the various civilised and semi-civilised peoples of the globe.

THE German War Department has recently carried out some experiments on a large scale with the electric light at Metz, in order to test its practicability for military purposes. One of the largest known electric lanterns was used for the trials, and it was found possible to distinguish small detachments out of rifle-shot with sufficient accuracy to direct on them artillery fire.

THE alarming rapidity with which shortsightedness is increasing among German students formed the subject of a recent debate in the Prussian Parliament. From extended observations made in the gymnasia, it appears that the number of the shortsighted increases from twenty-three per cent. in the first year to seventy-five per cent. in the ninth or last year. The too-frequent custom in Germany of forcing lads to study during the evenings with insufficient light, in ill-ventilated rooms, is undoubtedly a main cause of this widespread evil.

"HEROES of South African Discovery," by M. D'Anvers, that we referred to in our last number, will be published, we understand, next week by Messrs. Marcus Ward and Co. This volume will contain an account of Stanley's expedition, and the accompanying map will show the route taken by that discoverer.

THE Société d'Hygiene of Paris is making arrangements to establish, in the cities and towns of France, chemical laboratories for the purpose of examining articles of food and detecting adulterations or unhealthful constituents. In this respect France is, like Germany, following the example of England, where the value of public analysts has long since been satisfactorily demonstrated.

WHILE of course the thermo-electric pile is the most useful measuring apparatus in investigations on radiant heat, it is possible, M. Violle suggests (*Journal de Physique*) to repeat easily all fundamental experiments with the radiometer; by moving it along the spectrum one may readily show (even with the Drummond light) the distribution of the heat in the luminous part and in the infra-red region. The action of coloured glasses, the absorption of heat by water, in layers of different thickness, and all similar phenomena, can be shown without any difficulty. The beam of light employed falls directly, or after passage through the absorbent substance, on the radiometer, the image of which is, by means of a lens, thrown on a screen. The experiment is very distinct and pretty; it may be rendered more precise by adopting an arrangement for counting the number of turns of the radiometer. M. Violle says he has had constructed by M. Alvergniat a small radiometer for the purpose; it is placed on a Duboscq projection apparatus; and the turns can be easily counted on the screen.

IN order to determine the ratio of the specific heats of air at constant pressure and constant volume (a value so important for the doctrine of heat), M. Kayser has recently made fresh experiments on the velocity of sound in tubes. He adopted Kundt's method; in tubes of different diameter, air waves were produced by means of a transversely vibrating rod, and the length of these was measured by the dust figures remaining on the tube. Five tubes of different width were used, and three different steel rods. The results of the inquiry are these: (1) The velocity of sound in tubes depends on their diameter and on the pitch of the tones, and the retardation of the sound is inversely proportional to the diameter of the tubes, and the square root of the number of vibrations. (2) The velocity of sound in unconfined space is accordingly at any rate greater than in tubes; these experiments showed it to be greater than 331·646 m. (3) The velocity of sound in free space can be calculated from that in tubes when two tubes of different width are used; from these experiments the value obtained for it was 332·5 m. (4) From this the ratio of the specific heats of air at constant volume and constant pressure is inferred to be = 1·4106.

THREE experiments, made with a view to find how weak induced currents in the telephone would still suffice to give distinct perceptions by ear, have lately been described to the Vienna Academy by Prof. Sacher, of Salzberg: 1. The closed circuit of the telephone was, for a length of 20 metres, placed parallel with the insulated wire (cloth and wax) of an ordinary telegraph apparatus. The (Morse) signals were given first by means of six, then three, Smee elements. The induced currents gave a distinctly audible effect in the telephone, so that the messages could be understood. 2. The insulated wire was laid bare at two points 20 metres apart, and the ends of a telephone wire 120 metres long, and equally thick, were connected to it at those points. Only a small portion of the current could have passed through the thin wire in the telephone. Yet the tapping was heard with sufficient clearness to enable one to understand the message. (It is an advantage to use a telephone at each ear.) 3. A telephone wire about 40 metres long was connected with the inner thick wire of an ordinary induction coil, and a second telephone line, about 120 m. long, with the outer thin wire. To Prof. Sacher's surprise it was found possible to communicate through the first to the second telephone, and also (somewhat better, it seemed) in the opposite direction; and this nearly as well as with direct connection. The words were perceived more distinctly when two induction-coils were inserted in the same way. The experiment did not succeed with a Ruhmkorff.

THE improvement of the air-pump, which consisted in dispensing with the flask-like receptacle (with stop-cock) as employed by Otto v. Guericke and Robert Boyle, and introducing the much more convenient plate, is generally attributed to Papin. This is shown by M. Gerland (*Pogg. Ann.*, No. 12, 1878) to be a mistake. In Papin's first paper, "Nouvelles Expériences du Vuide," &c., which appeared in Paris in 1674, and which in 1686 had become rare (the only two copies of it now extant are in possession of the Royal Society, and in the British Museum library), he describes and gives a figure of the machine with which the experiments were made, and says:—"Monsieur Hugens (*sic*) fit faire çette machine, ensuite celle de M. Boyle, et il apporta divers changemens qu'on remarquera en comparant leurs figures." This machine (whose figure M. Gerland reproduces) is the first which has a plate. Additional proof that Huygens has the credit of the device is furnished by a letter of Huygens himself, and the date at which the improvement was introduced is shown to have been 1661.

THE Paris Jardin d'Acclimatation has just received a pair of those peculiar Siberian hares, which are grey in summer and white in winter, for the purpose of studying the effects of a temperate zone on the changes of colour.

THE first telegraph line of the Chinese Empire has recently been established between the arsenal of Tian Tsin and the house of the provincial governor. The constructor was Mr. Betts, the director of the School of Mines of Tian Tsin. Although the line is only some ten kilometres in length yet its construction marks a new epoch in the administration of the Empire. The Great Northern Telegraph Company, in spite of repeated efforts made at Foo Chow, have not succeeded in obtaining the permission of connecting this port with Amoy by a telegraph cable, and after vainly trying for two years have finally given up the idea. The line of Tian Tsin has, however, been constructed by order of the Chinese Government; and the population offered not the least resistance wherever the telegraph poles were erected. A cable

was required for the Pi-ho river, which intersects the line. The apparatus used are Morse's die-writers worked by Leclanché elements. Mr. Betts and some of his assistant pupils have been invited to visit Formosa in order to construct a line on the west coast of this island, viz., between Kee Lung and Tay-wan-foo. It is also proposed to establish another line at Tian Tsin, connecting that city with the capital of the province Paou-ting-foo.

DURING the year 1877 the Parisian press numbered no less than 836 different newspapers and serials (against 754 in 1875). Of these, 51 daily and 14 weekly papers are political, 49 serials are theological (37 Catholic, 10 Protestant, and 2 Israelitic); 66 are dedicated to law, 85 to political economy, 20 to geography, 74 to *belles lettres*; 20 are pedagogic, 52 literary-scientific, 56 artistic, 68 treat of fashions, 77 of technology, 75 of medicine; the contents of 43 are mathematical and natural-scientific, of 22 military, of 31 agricultural. Besides the above there are 16 sporting papers, 13 of various contents, and 4 dedicated to Freemasonry.

MANY alloys of tin and other soft metals hardened by addition of antimony, copper, &c., do not give a clear tone on being struck, but a lead-like, dull one. It has been found by M. Lilliman (*Pol. Notizblatt*) that the power of sounding clearly may be imparted to them, by immersing them for a half to one minute in a paraffin or oil bath, heated to a temperature 5° to 5°·5 below the boiling-point, then taking out and allowing to cool. This does not produce any diminution of density, but a considerable increase of the hardness and rigidity.

THE *Proceedings* of the Bristol Naturalists' Society (vol. ii. part 1, new series) contains, as usual, some papers of more than average value. There are three papers on the microscope by Dr. Fripp, two on the Bristol coalfield by Mr. W. W. Stoddart, besides two other geological papers by the same author, a paper by Mr. W. Evans on the scientific aspects of tanning, and other matters of importance. The *Transactions* of the Bedfordshire Natural History Society for 1876–7 contains a number of good papers on local natural history.

THE gasworks at the Grasbrook at Hamburg have recently been covered with a gigantic iron roof, constructed by the "Essener Union." Its weight is 51,500 kilogrammes, its length 84 metres. With the exception of the roof on the Liverpool gasworks, it is the largest in Europe.

AT the meeting of the Royal Academy of Sciences at Berlin, on January 24, Prof. Du Bois Reymond, as President of the Committee of the Humboldt Institution for Naturalists and Travellers, read a detailed report of the activity of this institution during the past year. The first undertaking was that of Herr J. M. Hildebrandt, and referred to the exploration of the snow-clad mountains of Equatorial Africa, viz., of the Mt. Kenia and of the Kilima-Ndjaro. The well-known traveller, although he approached the former mountain to within a few days' march, could not reach it altogether on account of the unconquerable difficulties placed in his way by the enmity of the native tribes, but he will again take up his plan after having recruited his health at home. Herr Hildebrandt, however, has brought home rich scientific collections from his journey, and has presented them to the scientific societies at Berlin; his geological collections are of special interest. The second traveller sent out by the Humboldt Institution, Dr. Karl Sachs, continued and terminated his investigations on the electric eels (*Gymnotus electricus*) at Calabozo, an important town in the Llanos of Venezuela. He succeeded in adding to our knowledge of *Gymnotus* considerably, so that of this species now quite as much is known as of *Torpedo;* he failed, however, to throw any light upon the development of *Gymnotus*. Dr. Sachs is now occupied in writing a treatise on this subject, as well as a description of the country and the people of Venezuela and his own experiences while travelling.

IT is very unsatisfactory to hear that the consignment of soles and turbot which left the Southport Aquarium on January 3 for the purpose of stocking the Bay of Massachussetts has turned out almost a total failure, one pair of the former only having arrived at their destination in safety. Prof. Baird, United States Commissioner of Fish and Fisheries, is so anxious to introduce the above-named fishes into American waters that another journey to England is contemplated about May next. Much experience has been gained in the transit of live fish across the Atlantic, which will be of considerable importance in facilitating future arrangements. It is highly probable that the bony pike and other American fishes, many of which are remarkable for their brilliancy of colour, will ere long find a home in English aquaria.

THE additions to the Zoological Society's Gardens during the past week include a Common Swan (*Cygnus olor*) from Holland, presented by Mr. John Colam, F.Z.S.; two Crested Guinea Fowls (*Numida cristata*) from West Africa, presented by Mr. Collingwood; two Canadian Geese (*Bernicla canadensis*) from North America, presented by Mr. Edward J. Philpot; four Reeves's Terrapins (*Clemmys reevesi*) from China, presented by Mr. A. Thomson; a Brazilian Tortoise (*Testudo tabulata*) from Cartagena, presented by Capt. King; a Poitou Donkey (*Asinus vulgaris*) from the south of France, deposited; an Azara's Fox (*Canis azaræ*) from South America, purchased.

D'ARREST'S SPECTROSCOPICAL RESEARCHES

WHEN the late Prof. d'Arrest was called to superintend the building of the new observatory in Copenhagen and the erection of a large refractor (16 feet focal length by 11 inches aperture), he took advantage of the opportunity thus offered to enter into more extensive researches on the nebulæ, than he had been able to undertake at Leipzig. He intended at first to observe all the nebulæ which were visible in his refractor, but he soon found that a work beyond human power, and that in fact the nebulæ are infinite in number. Working hard for six years he was only able to collect the eighth part of the observations required for laying down approximate positions of all those nebulæ which are distinctly visible in the Copenhagen refractor, and whose places could be exactly determined. These observations were published as "Siderum nebulosorum observationes Havnienses," in 1867, for which the gold medal of the Royal Astronomical Society was awarded to him in 1875. Prof. d'Arrest died eight years after the publication of his great work, his health broken down by constant night-watches. These years were spent mostly on spectroscopical researches, which were partly published in the *Astronomische Nachrichten*, partly in a separate paper, "Undersögelser over de nebulose Stjerner i Henseende til deres spectralanalytiske Egenskaber," in 1872. This latter paper does not appear to be so widely known as it deserves, and an abstract in the columns of NATURE might therefore be acceptable to many.

It took D'Arrest several years to get sufficiently acquainted with the use of the new apparatus—so different from those usually handled by astronomers of the old school. Various forms of spectroscopes are employed according to the subject to be examined. To observe the protuberances or their lines the greatest possible dispersion is required in order to weaken on one hand the sun's light, and on the other hand the diffuse atmospherical light which forms the background on which the lines are projected; while prisms of small dispersive power are employed when for instance the bright lines of comets or nebulæ are examined. D'Arrest's spectroscope was not intended for any extreme application; it was a so-called Janssen's, after Amici's principle composed *à vision directe* of three crown and two flint-glass prisms from Merz.

The solar light has lately been made to go twice through the system of prisms, and the dispersive power thus doubled has rendered many more bright lines visible than were known

heretofore. Besides the principal lines C, D_3, and F, discovered in 1868, only three or four feeble secondary lines of unusual occurrence were known in the spectrum of the sun's chromosphere, until Prof. Young, in the autumn of 1871, succeeded in raising the number of the visible bright lines to 103 in the course of only four weeks by the above method. These lines are almost uniformly distributed over the whole spectrum from wave-length 706 to 410. The lines are, however, of very varying brightness and frequency. But that artifice is of no good for investigating the planets or planetary nebulæ, for which instruments of the greatest possible amount of light are required.

D'Arrest did not make any profound study of the SUN'S protuberances, but convinced himself of most of the many peculiarities that have been discovered since autumn 1868. He mentions especially the pointed extremities of C and D_3, and the broad basis and *fine point* of F. This is explained by a lowering of temperature and density at a distance from the sun's surface, but it is certain that this phenomenon, with its physical consequences, appears with very different intensity by C and by F, from what it does by $H\alpha$ and $H\beta$ of hydrogen. It appears remarkably enough most distinctly by the feeblest of the two. The lines $H\gamma$ and $H\delta$ are in themselves far more insignificant, and their extension no doubt smaller. D_3 is of another unknown origin. He often observed, besides, the oblique position and distortion of the F-line in protuberances, which were evidently produced by violent eruptions, but he never saw distortions so violent that the line shoots branches to both sides, and at last is altogether dilacerated. Lockyer has represented many such cases in vol. xviii. of the *Proceedings* of the Royal Society. Secchi does not mention them in his book on the sun (Paris, 1871), and they have perhaps only been seen by Young besides. As to the explanation, we meet with a difficulty similar to that above; the phenomenon shows itself principally and nearly exclusively in this single line. D'Arrest never noticed such a thing in $H\alpha$. It is explained by the rapidly rotating mass of hydrogen towards or from the slit of the spectroscope, the wave-length of the light being thus alternately lessened and increased. He calculated a velocity of fifty or sixty geographical miles in the second from the greatest displacement he noticed by the F-line. The direct consideration of the occasional explosive alterations of protuberances leads to similar conclusions. It is beyond doubt that the velocity is so enormous. Much smaller displacements could besides hardly be ascertained by means of the spectroscope.

The spectra of the *sun-spots* have been examined ably by different investigators, and a rich material exists which shows the most probable assumption to be that the sun-spots are the results of cooling. It is in fair accordance with this interpretation that the increased absorption of light which the spot-spectrum shows by augmentation of the lines in breadth and darkness is considered a proof of the condensation of the gases, to whose absorption the dark lines in the normal sun-spectrum owe their origin. According to d'Arrest's opinion, this pervading *élargissement* must be mainly attributed to the circumstance that the lines are seen on a darker background where the irradiation is greatly lessened, and he believes that the whole theory, which is founded on the supposition of elective absorption of the spots is not quite to be trusted as yet. He never saw bright lines in any spot-spectrum, and mentions that other assiduous observers have likewise failed in this respect. The normal dark lines in the sun-spectrum are of very different degrees of darkness and breadth; some exhibit sharp borders, while others are winged, &c. These intrinsic relations he remarked did not change in the spot-spectra from what they were in the neighbouring region. He found, for instance D_2 to enlarge more than D_1 in proportion to their different breadths in the normal spectrum. From the lessened irradiation, moreover, some sharp lines of the normal-spectrum may become somewhat foggy in the spot-spectrum, as Secchi (*Compt. Rend.* 1869, p. 520) states is the case with the principal lines of sodium, a circumstance which, however, d'Arrest did not confirm any more than the remark by the same astronomer that the lines of magnesium are hardly enlarged in the spot-spectrum. Lockyer says (*Proceedings*, Royal Society, vol. xvii. p. 352) that they are thicker when observed in a spot than usual. Vogel has remarked a similar thickening of easily visible dark lines in Jupiter's spectrum in those parts of the spectrum which correspond to the dark bands on the planetary disk.

D'Arrest does not consider his observations as sufficient to establish anything as to the encroachment of the gas lines in the spot-spectra which occasionally has been observed by the so-called light-bridges in the interior of the spots; he remarks that a similar phenomenon may be produced spontaneously by looking at the image of one of the gas-lines of the protuberances, when the slit is not placed exactly in accordance to the refrangibility of this particular line, and investigators may not have been sufficiently attentive to this circumstance. The light concentrated in the few protuberance-lines is of course stronger than the light of the continuous spectrum of the border, and when the slit is even very slightly displaced the protuberances appear distinctly to reach within it. The same is the case with protuberances inside on the disc of the sun, where they mainly betray themselves by partial reversal of some lines from dark to bright.

The whole of astronomical spectrum analysis is founded upon the law that the source of the light of a continuous (with or without dark lines) spectrum containing rays of every refrangibility, is a solid or fluid substance, and that the source is a glowing gas whenever the spectrum is discontinuous and reduced to separate bright lines. This must within certain limits of pressure be considered as raised beyond doubt, although most skilful chemists disagree as to the nature of spectra of different orders. Plücker and Wüllner state that the same substance gives different spectra at different pressures and temperatures. Dubrunfaut, Reitlinger, and, above all, Angström deny this. The special use which has been made of Geissler's tubes in astronomical observatories is at any rate rather doubtful, since chemists have shown the true nature of the compound spectra which such tubes may furnish—for instance of hydrogen and nitrogen.

Dr. Huggins examined for the first time on August 29, 1864, one of the brightest planetary nebulæ (H., iv. 37) and found the spectrum concentrated in three short bright lines. This discovery proved the nebula to consist of glowing gas under a feeble pressure. Thus also for the first time was obtained the means of distinguishing between true nebulæ and conglomerations of stars. The latter, by far the most common, show the continuous spectrum, the former the linear. This question would hardly ever have been definitely answered by aid of any telescope. First Huggins, then Rosse and Secchi examined almost all those nebulæ in the northern sky, which were visible in their apparatus, and only one or two observers have since made further investigations on the single objects. Capt. J. Herschel examined (1868) in India the southern nebulæ spectroscopically. Most gaseous nebulæ are planetary. D'Arrest had already in his smaller catalogue in 1855 remarked about H. iv. 18: "bluish quiet light, as all planetary nebulæ seen by me show it," and in 1866 in "Obs. Havn." about H. iv. 37:—"Unica prope inter nebulas et prorsus singularis. Ellipsis est egregie cærulea cet." We now know both these to be gaseous nebulæ, analysis showing the light concentrated into three lines near each other in the green and blue regions of the spectrum.

The exact determination of the place of the lines in the normal spectrum was connected with great difficulties on account of their feeble light. It was therefore at first uncertain whether the three lines were identical in the different spectra, but there can now be no doubt as to this, and d'Arrest found by a discussion of the observations of Capt. Herschel, Secchi, and especially Vogel the following wave-lengths for the lines. The line Neb. (3) has by Huggins and Miller, Secchi, and lately Vogel, been proved to coincide with the F-line ($H\beta$) and d'Arrest assumes in consequence its wave-length after Angström:—

	Wave-length.	Vibrations in 1 second.
Neb. (1) ...	500·40 mill. millim. ...	596·64 billions,
Neb. (2) ...	495·66 „ „ ...	602·35 „
Neb. (3) ...	486·06 „ „ ...	614·25 „

Beyond Neb. (3) is occasionally (by H. iv. 18 and the Orion nebula) perceived a fourth line $H\gamma$, but it is very difficult to see it.

The spectra of the different objects are, however, very unlike each other on account of the different *intensity* of the bright lines. There is even occasion to presume that the *mixed gas spectra* do not ever continue unchanged with regard to the relative intensity of the lines, which is very likely, as the relative brilliancy of both the green lines of glowing H and N depends upon the mixture of the gases.

We know that air when under a feeble pressure heated by an induction-current, exhibits the line Neb. (1); it belongs to *nitrogen*.[1] Lockyer and Frankland (*Proceedings*, Royal Society, vol. xvii. p. 454) have shown that the in reality very complicated spectrum of nitrogen, under certain circumstances of pressure and temperature, is reduced to this bright line with but feeble traces of the

[1] D'Arrest mentions that the above wave-length agrees perfectly with Huggins's observation, when he identifies Neb. (1), not with the middle of the double line, but with the least refrangible of the two.

others. It is, besides, the brightest of them all. Extensive investigations published on this subject cannot, however, be said fully to elucidate the question why the other lines of nitrogen do not appear in the spectrum, nor do physicists agree as to the temperature and density which, under these circumstances, must be supposed in the nebulæ. It is, besides, precarious to draw from phenomena observed in Geissler's tubes conclusions as to circumstances prevailing in the vast nebulæ (Zöllner, *Berichte der k. sachsischen Gesellschaft d. Wissensch*, for 1870, p. 254). It appears less important that nobody has been able to comply with Angström's demand when he says ("Recherches sur le Spectre solaire," p. 37) :—"This line is double. . . . It appears, therefore, that we ought to be able to show this duplicity in the corresponding line of the nebular spectrum." To their separation is required too narrow a slit for the feeble light of the nebulæ. All considered, nitrogen is at present very likely one of the constituents of nebulæ.

The origin of *Neb.* (2) is not known. The idea at first occurred to Huggins of one of the many barium-lines, but he soon gave this idea up. One of the iron lines holds exactly its place ; it is a dark line, but not one of the principal of the rich spectrum ; of course this coincidence is accidental. This line is again met with in the spectrum of many red and variable stars. The measures of Vogel (*Ber. d. k. sachs. Gesellsch. d. Wissensch.*, 1871, December 17) agree well enough with the gaseous line when the great difficulties of the cases are taken into account. *Neb.* (2) does *not* occur in the spectra of comets.

Neb. (3) is identical with the line $H\beta$ of hydrogen, whose existence in gaseous nebulæ was proved when Huggins discovered Neb. (4), which is $H\gamma$, that was so long sought for in vain. Hydrogen is everywhere found as one of the constituents of the heavenly bodies, but the comets contain no traces of it. The hydrogen-lines appear even in the spectra of many fixed stars, at least through $H\alpha$ and $H\beta$, but sometimes $H\beta$ and $H\gamma$ are the strongest (β Lyræ), and three hydrogen lines are distinctly seen in the spectra of α Aquilæ and α Lyræ.

D'Arrest then gives in his paper a list of all the nebulæ which have been spectroscopically examined by himself or others.

He speaks first of the gaseous nebulæ, of which H. iv. 37 is the most remarkable ; then he mentions those whose spectra are continuous, and thus proved to be mere conglomerates of stars. The latter are by far the most difficult to examine, the feeble light being distributed over a large space, and generally minima visibilia. An astronomer well versed in the use of the spectroscope is, however, often able to decide whether the spectrum is continuous, even if it be not visible by glimpses. Already the absence of the spectrum may occasionally hint about the true nature of the body. He estimates the number of nebulæ known in the middle of 1872 to be about 6,000 ; of these 150 have been examined with the spectroscope. It is, therefore, only the fortieth part, which is bright enough to be seen through the system of prisms. Although it is hardly possible to draw conclusions from so small a fraction of the whole, still d'Arrest thinks it possible, on account of the critical revision he has given the observations, to arrive by induction at a few results. He finds that of a given number of nebulæ about a fourth give the discontinuous spectrum, while three-fourths give the continuous.

Gas nebulæ are, with but few exceptions, known by their green-blue light, their sharply-defined, round, or elliptic discs with annular bright condensations inside. There are, however, large, extensive, irregular, and complicated nebulæ, which also consist of the three gases, nitrogen ever foremost, though the gases are mixed in different proportions. The very feeble continuous spectrum which appears in many planetary nebulæ can in most cases be shown to arise from the consolidated nucleus, the fluid or solid central mass. The distribution of brightness in extensive nebulosities is very irregular, and the heat in certain regions rises and falls occasionally a little, though no real alterations in the form are known as yet.

The ray-nebulæ are surely mere conglomerations of stars. Those are the long, lenticular nebulæ, often so narrow and fine that such an object may resemble a thin bright line drawn through the nucleus. No such nebula is hitherto known to give a tri-chromatic spectrum.

It was in 1866 that Secchi commenced to examine red stars with remarkable broad bands in the spectra, and he was already, in 1868, compelled to add a fourth class to his three classes of star-spectra. A systematical search after remarkable star-spectra was undertaken in 1873 and following years in Copenhagen. D'Arrest's four papers in *Astronomische Nachrichten* contain only the most remarkable of those he found, and only such as had not previously been mentioned. That most are above the eighth magnitude is evidently only founded on the difficulty of seeing spectra of smaller stars.

The circumstance which Secchi remarked in 1868, that yellow and red colours are so often connected with prominent spectra, seems certainly to be of importance, but the many exceptions should warn us from here expecting any great cosmical law. Neither is their connection with variability a rule without exceptions. There are many strongly coloured stars with very indifferent spectra.

Most of the spectra described are of the third class. These are not uncommon, for when we examine 140 stars we may expect to find one of the third class. They are uniformly distributed over the sky, and found also by white stars. The character of spectra of this class is constant throughout. The positions of the dark bands were also shown by Vogel, in 1872, to be the same for four bright stars. The columns are generally more distinctly separated towards the red end of the spectrum, though the contrary occurs also, and it is even possible to follow the steps from but finely-indicated bands to absolute discontinuity, but the colour has nothing to do with these gradations.

Still more intimately connected with orange colour is the fourth class, and specimens of this class are, in consequence, very uncommon. D'Arrest ascertained that the dark bands in the star-spectra are formed by groups of compressed dark lines against Secchi's experience. He examined spectra of stars with great proper motion, and found, for instance, the spectra of 61 Cygni and 1830 Groomb. to be indifferent, uniform, and continuous. General similarity of the spectra in certain parts of the sky does not exist at all, or has not been proved yet ; for instance, it is not true that red and yellow are wanting in the spectra of small stars in Orion. W. D.

THE PROGRESS OF METEOROLOGY[1]

AT the opening of his address Dr. Neumayer regretted that the general knowledge and public appreciation of meteorology was still very small in comparison with that of other branches of science. The main object of his address was therefore to induce his hearers to do all in their power to effect a more perfect and detailed understanding of this branch of science among their countrymen in their respective spheres of activity. He treated the subject, and particularly the weather-forecasts, mainly from his own point of view as a naval officer, and pointed out how desirable a greater interest in marine affairs would be in all circles of German home life. The course which meteorology in its application to daily life has taken may be divided into two categories of observations, first the uninterrupted systematic meteorological investigations, and second, the atmospheric disturbances or phenomena governed by the laws of winds, as first described some fifty years ago by Prof. Dove. He then gave a sketch of the progress of meteorology in other countries ; of the establishment of the numerous meteorological stations, and the application of telegraphy to this science ; of the enormous help afforded by the introduction of rapid means of communication. He pointed out how the greatest progress was made by the United States of North America ; that England was second in this respect, and was followed by Holland, France, and Denmark. According to the latest news, the yearly budget for meteorological observations in the United States was raised from 250,000 to 450,000 dollars, apart from all personal expenses. The rise in this sum is explained by the necessity of having special telegraph wires and stations solely for the meteorological service and by the increase in the staff of observers. If in Europe the practical results of observations are not quite as satisfactory as might be desired, it is because the European organisation of the meteorological service is far more imperfect than the American one. The German Government has given its full attention to this important science, particularly with regard to the German navy and the coast population. The poor German fishermen in the Baltic and the German Ocean are already deriving great benefit from the numerous meteorological stations which have been established along the German coast-line, although it is only eighteen months since the service has begun. The German "Seewarte" has been established and now performs its share of international work along with the sister-establishments of England, Holland, and France. Agriculturists will

[1] Meteorology in Daily Life. Address delivered at the meeting of the German Association at Munich, by Dr. G. Neumayer, Director of the Deutsche Seewarte at Hamburg.

derive equal benefit from the meteorological service if inland stations are established and care is taken that the general population of the country are taught how to appreciate their work. Dr. Neumayer concluded with the sentence :—It is not only the duty of the State to found beneficial institutions and to organise them efficiently, it is also the duty of the State-citizen to learn to understand and to appreciate these institutions and to enter into this spirit of the work they are called upon to perform.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

St. Andrew's.—The Senatus Academicus of St. Andrew's University have conferred the degree of LL.D. upon Mr. Henry Woodward, F.R.S., of the British Museum, and Dr. W. C. Macintosh, of Murthly, well known for his researches on British annelides.

The Yorkshire College, Leeds.—Mr. Arnold Lupton, F.G.S., has been elected Instructor of Coal Mining, a department recently endowed by the Drapers' Company of London.

Higher Education of Women.—A public meeting composed for the most part of ladies, was held on the 6th instant in the Vestry Hall, Kensington, to inaugurate the system of lectures for the higher education of women, undertaken by the Principal and Professors of King's College with the co-operation of the Women's Educational Union. The object of the lectures, which commenced on the 11th instant, is to supplement and continue school education, and the instruction will have, as far as possible, reference to the examinations open to women in the University of London or elsewhere. The minimum age of students is fixed at 17, except in such cases as receive the special sanction of the committee. The classes will be at present held in the Vestry-hall, High Street, Kensington. The curriculum embraces Holy Scripture and Church history, logic and moral philosophy, modern and ancient history, the English, Latin, French, and German languages and literatures, mathematics, mechanics, and botany. Experimental physics, chemistry, and drawing will also be taught as soon as suitable arrangements can be made. Other classes, if necessary, will also be formed. The fees vary from 10*s.* 6*d.* to 2*l.* 2*s.* per term; for any four complete courses they will 6*l.* 6*s.* For ladies engaged in teaching there will be a remission of 25 per cent.

Prizes in Botany for Young Women.—The Society of Apothecaries of London announce their intention to award prizes to young women students in botany for proficiency in that science, the prizes to be competed for under the following conditions :—The competition will be open to all young women who shall produce from their teachers certificates that their age at the time of examination does not exceed twenty years. The examination will be in general and not medical botany. It will consist of questions both written and oral, in—(1) Structural Botany; (2) Vegetable Physiology; (3) Description of Living Plants; (4) Systematic Botany; so far as these subjects are contained in Sir Joseph Hooker's "Science Primer—Botany," and in Prof. Oliver's "Lessons in Elementary Botany." The first examination will take place in London on the third Wednesday and the third Friday in June, 1878. Candidates will be required to send their names and their residences, at least fourteen days before the examination, to the Beadle, Apothecaries' Hall, Blackfriars, E.C., when they will receive tickets of admission to the examination.

France.—M. Bardoux, the French Minister of Public Instruction has taken preliminary steps for organising at Paris on the occasion of the Universal Exhibition, a great congress of schoolmasters. A delegate from each district will be sent by his fellow-teachers. Not less than 4,000 are expected to be present.

M. Bardoux has again taken up in the French Chamber the proposed erection of new schools in France. M. Waddington was the originator of the scheme. It is intended to build no less than 17,320 new school-houses, and purchase, enlarge, or restore 12,000 others.

SCIENTIFIC SERIALS

Verhandlungen der k.k. zoologisch-botanischen Gesellschaft in Wien (vol. i., 1877).—This volume contains the continuations of, and additions to, several important papers commenced in the volumes for previous years. We mention particularly the mycological researches by Herr Schulzer von Müggenburg.—The other papers, of which some are very elaborate, are :—On *Cecidomyidæ*, by Dr. Franz Löw.—On the Diptera genus *Medeterus*, Fischer, by F. Kowarz.—On the fungus flora of Vienna, by W. Voss.—On some spiders from Madagascar, by Count E. Keyserling.—On some American spider genera from the families *Pholcoidæ*, *Scytodoidæ*, and *Dysderoidæ*, by the same.—On the *Holothuria* fauna of the Mediterranean, by Dr. Emil von Marenzeller.—On *Psyllodæ*, by Dr. Franz Löw.—On the *Chalcidiæ* genus *Olinx*, by Dr. Gustav Mayr.—Coleopterum, species novae, by E. Reitter.—On the passage of *Pastor roseus* (Temm.) through Austria, Hungary, and the neighbouring countries in 1875, by V. von Tschusi.—Ornithological notes, by B. P. Hanf.—On the flora of Southern Istria, by I. Freyn.—On the lepidoptera fauna of the Dolomite district, by I. Mann and A. Rogenhofer.—On the coleoptera fauna of Central Africa, by P. V. Gredler.—The volume concludes with an interesting description of the piscicultural establishment of Herr A. Fruwirth at Freiland, near St. Pölten (Lower Austria), by Dr. E. von Marenzeller.

Memorie della Società degli Spettroscopisti Italiani, May, 1877.—A note on the solar eruptions during 1876, by Prof. Tacchini. Number of days of observation, 106; number of eruptions, 9, two on eastern limb and 7 on western limb.—Note by the same author on the present solar phenomena as compared with those during the maximum spot period; the number of eruptions observed at Palermo in 1871 were 97, while only one was seen in the first four months of this year.—Letter from Father Secchi to Prof. Tacchini on the above subject, also a letter between the same persons relative to Winnecke's comet.—Drawings of solar prominences for January and February, 1876, accompany this number.

June.—Note on a water-prism, by Father Secchi. Path of solar protuberance observed at Rome in April, 1877; same for May.—Note by Prof. Tacchini, on a metallic solar eruption seen in June last; the following lines were visible in the spectrum : b^1, b^2, b^3, b^4, 1474*k*, 4923*A*, 5017*A*, sodium, 5369*A*.

July.—Continuation of the above note.—Note by Prof. Millosevich, on the contact of Mercury with the sun's chromosphere on May 6, 1878.—Drawings of the chromosphere for the months of March, April, and May, 1876, accompany this number.

August.—Note on the zodiacal light, by Prof. Serpieri.—Announcement of the death of Eduardo Heis.—Description of a new form of gravity escapement, by Prof. Young.—The spectroscopic drawings of the chromosphere for June and July, 1876, accompany this number.

September.—A paper on the discovery of oxygen in the sun by photography, and a new theory of the solar spectrum, by Prof. H. Draper. [This discovery, and all matter relating thereto, have already been fully reported in our columns.]—Table of solar protuberances observed at Rome in June, 1877.—Table of solar spots seen at Palermo in July and August, 1877. Four maps, together with a preface by Prof. Heis explaining them; the maps are of a portion of the heavens adjoining the ecliptic, and show stars down to the fifth magnitude, and they are for use in determining the position of the zodiacal light.

Journal de Physique, January.—On the employment of rotating discs for the study of coloured luminous sensations, by M. Rosentiehl.—On the use of the radiometer as an apparatus of demonstration, by M. Violle.—Rheostatic machine, by M. Planté.—Experimental researches on the interferences of light, by M. Righi.

SOCIETIES AND ACADEMIES

London

Royal Society, January 10.—"On the Structure and Development of the Skull in the Common Snake (*Tropidonotus natrix*)," by W. K. Parker, F.R.S.

"Observations on the Nervous System of *Aurelia aurita*," by Edward Albert Schäfer, Assistant-Professor of Physiology in University College, London. Communicated by W. Sharpey, M.D., LL.D., F.R.S.

January 24.—The Cortical Lamination of the Motor Area of the Brain," by Bevan Lewis, F.R.M.S., Pathologist and Assist. Med. Officer to the West Riding Asylum, and Henry Clarke, L.R.C.P. Lond., Med. Officer to the West Riding Prison.

Communicated by D. Ferrier, M.A., M.D., F.R.S., Professor of Forensic Medicine, King's College, London.

January 31.—"Further Researches on the Minute Structure of the Thyroid Gland." Preliminary Communication. By E Cresswell Baber, M.B. Lond. Communicated by Dr. Klein, F.R.S.

"On the Limits to the Order and Degree to the Fundamental Invariants of Binary Quantics," by J. J. Sylvester, M.A., LL.D., F.R.S., Professor in the Johns Hopkins University, Baltimore, U.S.

"Remarks connected with the Number of Figures in the Periods of the Reciprocals of Prime Numbers," by William Shanks, communicated by Rev. Dr. Salmon, F.R.S.

Linnean Society, January 17.—Prof. Allman, president, in the chair.—Specimens of Dipterocarpaceæ collected by Signor Beccari, in New Guinea, were exhibited and commented on by Mr. Thiselton Dyer.—Attention was drawn by Mr. E. M. Holmes to a Japanese book containing sections of native woods botanically named in English, Latin, and Japanese.—Several examples of fasciated stems of the Fuller's Teazel (*Dipsacus fullorum*) were exhibited by Mr. J. R. Jackson, who stated these curiously malformed stems were now successfully introduced for the handles of sunshades; he also made remarks on a bird's nest formed of wool and cotton-pod, sent by Sir Bartle Frere to the Kew Museum.—Prof. Owen then read a paper on *Hypsiprymnodon*, a genus indicative of a distinct family in the Diprotodont section of the marsupials. The animal in question is an inhabitant of the Rockingham Bay district, Queensland, and sparingly frequents the dense, damp scrubs bordering the coast. It is diurnal, and feeds on insects, worms, and tuberous roots, or palm berries, holding these in its fore-paws, and sitting on its haunches, after the manner of the phalangers. They breed during the rainy season, February to May. Both sexes have a musky odour, are nearly alike in size, and somewhat over a foot long. This Rat Kangaroo (*H. moschatus*) Mr. Ramsay, of Sydney, first named and gave a short description of, and Prof. Owen now supplements by a fuller account of its skeleton, &c. Besides peculiarities in dentition and skull, the latter dwells on the structural conditions of the hind foot, a modification between that of the Potoroos and Kangaroos. He thereafter enters into comparisons with the feet of the ostrich group (*Struthionidæ*), and speculates on the modifications of the five-toed feet revealed by palæontology, and as applicable to the living marsupials &c. —Mr. Francis Darwin's communication, experiments on the nutrition of *Drosera rotundifolia*, we gave an abstract of in NATURE, vol. xvi. p. 222.—Notes touching recent researches on the Radiolaria, was the title of a paper by Prof. St. G. Mivart. In this *résumé* the history, progress, and present condition of the subject are elucidated. These remarkable marine surface-swimming organisms the author proposes to arrange after the classification adopted by Prof. Haeckel, but considerably modified. The primary groups are reduced from fifteen to seven as follows:—1. Discida; 2. Flagellifera; 3. Entosphorida; 4. Acanthometrida; 5. Polycistina; 6. Collozoa; and 7. Vesiculata.—Mr. J. Kerswill was elected a Fellow of the Society.

Anthropological Institute, January 29.—Mr. John Evans, D.C.L., F.R.S., president, in the chair.—Anniversary Meeting. —The president, in the course of his address, alluded to the late conference on the "Antiquity of Man," and expressed his opinion that the question might be discussed with as great advantage from a purely English point of view, as from one embracing a larger area, which to some extent held good with regard to the question as to whether the palæolithic implements of the river-gravel might not be referred to an interglacial period. As to the relics of human workmanship thought to have been discovered in beds of pliocene and even miocene age in Italy, Switzerland, and France, Mr. Evans again on this occasion repeated the words of caution he had previously expressed, but nevertheless believed that eventually traces of man would be found of an earlier date than that which can be assigned either to the cave or river-gravels of Western Europe. These traces were to be rather looked for in the east than in the temperate west or colder north. A strong hope was expressed that Indian geologists would ere long solve in a satisfactory manner the date and origin of the so-called laterite deposits of Madras, but Mr. Evans was able to announce that in Borneo there appeared a chance of some cave explorations being carried on which will probably throw light on the date of man's appearance in that part of the globe. Mr. Everitt, whose experience in cave explorations is well known, has proposed to devote a year to further researches, and Mr. Evans having guaranteed the necessary funds appealed to all those who were interested in the early history of man or in palæontology to assist in raising the by no means inconsiderable amount. The following are the council elected to serve for the ensuing year:—President, John Evans, D.C.L., F.R.S.; Vice-presidents, Prof. George Busk, F.R.S., Hyde Clarke, Major-General A. Lane Fox, F.R.S., Francis Galton, F.R.S., Sir J. Lubbock, Bart., M.P., D.C.L., F.R.S., Prof. Rolleston, M.D., F.R.S.; Directors and Hon. Secs., E. W. Brabrook, F.S.A., W. L. Distant, J. E. Price, F.S.A.; Treasurer, F. G. Hilton Price, F.G.S.; Council, J. Beddoe, M.D., F.R.S., James Bonwick, F.R.G.S., C. H. E. Carmichael, M.A., J. Barnard Davis, M.D., F.R.S., W. Boyd Dawkins, F.R.S., Capt. Harold Dillon, F.S.A., Prof. W. H. Flower, F.R.S., A. W. Franks, M.A., F.R.S., Charles Harrison, F.S.A., J. Park Harrison, M.A., Prof. Huxley, F.R.S., A. L. Lewis, R. Biddulph Martin, F. W. Rudler, F.G.S., C. R. Des Ruffières, F.R.S.L., Lord Arthur Russell, M.P., Rev. Prof. Sayce, M.A., M.R.A.S., E. Burnet Tylor, D.C.L., C. Staniland Wake, M. J. Walhouse, F.R.A.S.

Physical Society, February 2.—Annual General Meeting. —Prof. G. C. Foster, president, in the chair.—The president read the report of the Council for the past year. After pointing with satisfaction to the present condition of the Society, the report goes on to show how it is hoped to extend its usefulness in the future. In addition to a second edition of Prof. Everett's work on the C. G. S. system of units, the Council hopes very shortly to publish Sir Charles Wheatstone's papers in a collected form, and it is making arrangements for the publication, at intervals, of translations of foreign scientific papers, especially such as have had a marked effect on the progress of physical science. A portion of the funds of the Society is to be devoted annually to the formation of a library, and an exchange of publications is already made with various learned societies abroad. Special stress was laid on the distinctive object held in view at the formation of the Society, namely the exhibition, when practicable, of the experiments referred to in papers read at the meetings.—The following officers and council were elected for the ensuing year:—President, Prof. W. G. Adams, M.A., F.R.S.; Vice-presidents (who have filled the office of president), Dr. J. H. Gladstone, F.R.S., and Prof. G. C. Foster, F.R.S.; Vice-presidents, Prof. R. B. Clifton, M.A., F.R.S., W. Spottiswoode, LL.D., F.R.S; W. H. Stone, M.B., F.R.C.P., Sir W. Thomson, LL.D. F.R.S.; Secretaries, Prof. A. W. Reinold, M.A., W. Chandler, Roberts, F.R.S.; Treasurer, Dr. E. Atkinson; Demonstrator, Prof. F. Guthrie, Ph.D., F.R.S.; other Members of Council, Capt. W. de W. Abney, R.E., F.R.S., Prof. W. F. Barrett, F.R.S.E., Major E. R. Festing, R.E., W. Huggins, D.C.L. F.R.S., Prof. A. B. W. Kennedy, C.E., O. J. Lodge, D.Sc., Prof. H. M. McLeod, the Earl of Rosse, D.C.L., F.R.S., Prof. W. C. Unwin, B.Sc., R. Wormell, D.Sc. Prof. H. L. F. Helmholtz and Prof. W. E. Weber were elected Honorary Members of the Society. After votes of thanks had been passed to the Lords of the Committee of Council on Education for the use of the physical lecture room at South Kensington, as well as for the other advantages enjoyed by the Society, and to the several officers of the Society, the meeting was resolved into an ordinary one. The following candidates were elected Members of the Society:— M. T. Cormack, C. J. Faulkner, M.A., E. M. Jones, F.R.A.S., C. Leudesdorf, M.A., and C. E. Walduck.—Prof. S. P. Thompson exhibited a method of showing the lines of force due to two currents of electricity running in parallel directions. A plate of glass is perforated by two holes close together, which are traversed by one and the same wire, and this may be so arranged that the current traverses the parallel lengths in the same or opposite directions. If now the plate be held horizontally while the current passes, and fine iron filings be sprinkled on the plate, they will arrange themselves in the well-known forms. In the plates exhibited the filings had been fixed by gum, so that their arrangement could be exhibited to an audience by projection on a screen.

Chemical Society, February 7.—Dr. Gladstone, president, in the chair.—The following papers were read:—The alkaloids of the aconites, Part II.—On the alkaloids contained in *Aconitum ferox*, by Dr. Wright and Mr. Luff. The alkaloid pseudaconitin from *Aconitum ferox* forms crystallised salts with difficulty. Aconitin, from *A. napellus*, on the other hand, crystallises with facility. When acted upon by saponifying agents,

pseudaconitin is converted into dimethylprotocatechnic acid, and a new base, pseudaconin; mineral acids saponify pseudaconitin; tartaric acid forms the anhydro-derivative apopseudaconitin. With glacial acetic, and benzoic acids an acetyl and a benzoyl derivative are respectively formed. The properties, constitution, &c., of the above substances have been investigated by the authors. The nitrate and the gold salt of pseudaconitin were obtained in the crystalline form.—Notes on the tannins, by Dr. Paul and Mr. Kingzett. The authors conclude that (*a*) the supposition that natural tannin from gall-nuts is a glucoside is doubtful, (*b*) the astringent principle common to cutch and extract of mimosa bark is shown to be a glucoside and to yield on decomposition, unfermentable sugar and a peculiar acid distinct from gallic acid.—On the estimation of phosphorus in iron and steel. by E. Riley. The author has instituted a series of experiments as to the relative value of the molybdate and magnesia processes for determining phosphorus; as a general result, he concludes that the molybdate process always gives results which are too low, and that the magnesia method is the only one to be trusted.—An inquiry into the action of the copper-zinc couple on alkaline oxy-salts, by Dr. Gladstone and Mr. Tribe. The action of the couple on these oxy-salts is of an electrolytic nature; nitrites and ammonia are at first formed, but ammonia is the final product, when nitrates are taken: chlorides are formed, when chlorates are decomposed, but no chlorites or hypochlorites could be detected. When ammonium nitrate is acted on at the boiling-point nitric oxide is evolved.—On a new method for the determination of boiling-points, by H. C. Jones. A glass tube 4 mm. internal diameter and 200 mm. long is bent into a U, so that the one end, which is open, projects 15 mm. beyond the other which is closed. The closed leg is filled completely, and the open leg partly, with mercury, and a bubble of liquid manipulated into the closed end of the U. On immersing the U in a paraffin path and heating the latter, the liquid boils and the temperature at which the levels of the mercury in the two limbs are equal is the uncorrected boiling-point of the liquid.

PARIS

Academy of Sciences, February 4.—M. Fizeau in the chair.—Telegraphic determination of the difference of longitude between Paris and the Observatory of the war depôt at Algiers, by MM. Loewy and Perrier.—Portable instrument for determining itineraries and geographical positions in journeys of exploration on land, by M. Mouchez.—On some applications of elliptic functions (continued), by M. Hermite.—New observations on chemical reactions of the effluve, and on persulphuric acid, by M. Berthelot. When binary compounds are acted on by the effluve one part is decomposed while the other forms more complex combinations. Persulphuric acid, as well as ozone and oxygenated water, is gradually destroyed when the external influence, under which it has appeared, has ceased to act.—On definite hydrates, formed by hydracids, by M. Berthelot.—Experimental researches on the fractures which traverse the earth's crust, particularly those known as joints and faults (continued), by M. Daubrée.—The vibrations of matter and the waves of the ether in phosphorescence and fluorescence, by M. Favé.—Transversal vibrations of liquids, by M. Dubois. He puts a little liquid, with vermilion in it, on the branches of a tuning-fork, or on a paper strip, over the open end of a sounding pipe, and studies the striæ formed in it.—On some results obtained in treatment of phylloxerised vines, by M. Boiteau.—Discovery of a small planet at the Observatory of Toulouse, by M. Perrotin.—Ditto at the Observatory of Marseilles, by M. Cottenot.—Note on some consequences of the theorem of M. Villarceau, by M. Lemoine.—On the employment of the graphic method for prediction of occultations and eclipses, by M. Hatt.—On a new note by M. Boussinesq relating to the theory of elastic plates, by M. Levy.—On the formula $2^n - 1$, by M. Pepin.—On the determinant whose elements are all the possible minors of given order of a given determinant, by M. Picquet.—On the similarity of the photographic *réseau* of the sun and the craters of the moon, by M. Lamey. A similar cause is inferred.—On the equation of Lamé, by M. Brioschi.—On the dark lines of the solar spectrum and the constitution of the sun, by M. Cornu. By arranging in order of quantity the elements volatilised at the sun's surface (from the position and relative brightness of the dark lines), he considers the composition of the absorbent layer to be similar to that of volatilised aerolites.—The elements present in the layer of the sun which produces reversal of the spectral rays, by Mr. N. Lockyer.—On the refraction of gases and vapours, by M. Mascart. The results are given for some substances of mineral chemistry. It is shown, *inter alia*, that refraction furnishes a method for determining divergences from Mariotte's law where direct experiments on changes of volume or measurement of densities are difficult.—On the repulsion resulting from luminous radiation, by Mr. Crookes.—Researches on accidental double refraction, by M. Macé.—New direct vision spectroscope, by M. Thollon. This has a central fixed part and two symmetrical movable systems (consisting of metallic plates connected by joints and having prisms fixed on them) capable of turning about fixed axes parallel to the slit.—On the densities of vapour, by M. Troost. The density of vapour of acetic acid takes its theoretical value, correspondding to four volumes, even at temperatures bordering on 120°, if a weak pressure be operated with.—Dissociation of carbonate of baryta, by M. Isambert.—Memoir on the solubility of lime in water, by M. Lamy. This solubility varies with the nature or origin of the lime, its state of molecular aggregation, the temperature of its preparation, its dehydration or recalcination, its duration of contact with water, and previous heating of the milk of lime.—On anhydrous trichloracetic acid, by M. Clermont.—On the combinations of quercite, by M. Prunier.—On the nature of the very volatile products contained in raw benzines, by MM. Vincent and Delachanal. Besides carburets of hydrogen and coal oils, the authors find ordinary alcohol, cyanide of methyl, and sulphide of carbon.—On the employment of rotatory discs for study of colour sensations (continued); harmony of colours, by M. Rosenstiehl.—On use of the polarising microscope with parallel light for determination of the mineral species contained in thin plates of eruptive rocks, by M. Levy.—On the leadhillite of Matlock, by M. Bertrand.—On a new density apparatus, by M. Pisani.—Experiments demonstrating the *rôle* of air introduced into the arterial and venous systems, by M. Feltz. Air introduced, even in a very small quantity, into the aortic system, works great mischief; introduced into the venous system it is almost without danger.—New researches on the function of mucedineæ and their property of inverting cane-sugar (*à propos* of a note by M. Gayon), by M. Bechamp.—Treatment of cancers of the breast by ischæmia of the mammary gland by means of vulcanised caoutchouc, by M. Bouchut.—Barometric differences between neighbouring stations, by M. Renou.—Remarks on M. Faye's note regarding relations between phenomena of terrestrial magnetism and the rotation of the sun, by M. Broun.—On the telephone, by M. Champvallier. It is possible to correspond telephonically on wires carried on posts which also support wires for ordinary telegraphy to an extent of at least ten kilometres, and probably much further.—On the telephone, by M. Breguet. The effect is improved by placing one or more vibrating plates (perforated at the centre) at about one millimetre in front of the ordinary plate of the telephone.—On the earthquake at Paris on January 28, by M. de Gannes.—On the same, by M. Lefebvre.

CONTENTS

THURSDAY, FEBRUARY 21, 1878

THE HEAD-MASTERS ON SCIENCE TEACHING[1]

IT is much to the credit of the head-masters that they should have moved voluntarily in the matter of science teaching. The great majority of them are known to look upon it without hostility, but have hesitated to introduce it into their schools, in ignorance of its educational value, of the time and teaching power necessary, of subjects, methods, cost. Since the Report of the Science Commission all see that it must come, and that it is better for the schools to shape the system to be adopted leisurely and in concert than to wait till it is forced upon them from without. A few schools have already accepted it in principle; a very few have worked it adequately for some years past; to these the Head-Masters' Committee have applied for information, and their published answers are before us.

Questions were issued to the masters of twenty-four schools, of whom nineteen replied. They refer to the time spent on science in actual school work, the percentage of boys taught, the age at which teaching should begin, the subjects included, the methods and texts employed, the intellectual results apparent, the value of laboratory work, the cost of appliances, the influence, good and evil, of university scholarships, the text-books recommended; and it was requested that the answers might convey not individual theories of what might and ought to be, but a record of what had been and was being done in each particular school.

It is evident that the first question, as to time spent in teaching, is vital to the whole, and should determine primarily the comparative weight due to the answers sent from each head-master. Unfortunately the answers to it are in a great measure unreliable. Only one school gives the total number of its actual working hours; some do, and some do not apparently include hours of "preparation" in their estimate; one large school, Clifton, omits to reckon the extra time given to special classes, and probably others do the same; while Harrow, Magdalen, and Dulwich, all valuable witnesses, make no return. Taking the answers as they stand, eleven of the nineteen schools give from two to four hours only as a maximum per week, inclusive of practical work; and in some cases, at least, this is probably correct, representing also many more schools than are included in the list. Such schools have made a good beginning, are feeling their way to more extended teaching, and will hail the information given in these pages. But their maximum would be thought ludicrous in the case of literature or mathematics; it gives no real chance to science either as a storehouse of useful knowledge, or as a weapon of intellectual training; and accordingly the evidence valuable to schoolmasters is contained mainly in the answers sent by the remaining schools.

These may be tabulated as follows:—

School.	Hours per week given to science in different parts of school.	Percentage of boys learning science.
Bradford	10, 4, 3, 2	No return.
Clifton[1]	10, 4	90
Giggleswick	8, 6, 5, 2	80
King's College ...	7, 5, 5	No return.
Manchester	12, 9, $3\frac{1}{2}$	No return.
Newcastle-under-Lyme	7, 4, 2	90
Taunton	10, 8, 4, 3	87
Wellington	6, $3\frac{1}{2}$, $\frac{1}{2}$	73

As regards the age at which the study should commence, Clifton, Taunton, and Wellington think that it cannot begin too early; the rest give years ranging from ten to thirteen. All the schools agree in teaching chemistry and physics; three teach botany, three geology. All test progress by periodical frequent examinations within the school, Clifton and Taunton specifying the period as once in three weeks. All but one speak highly of the use of note-books; five object strongly to examinations from without, two find them useful. Bradford, Clifton, Taunton, Wellington celebrate the good effects of science as a school subject, from its stimulating power, its bringing apparently dull boys to the front, its inculcating a comprehension of physical law. Six schools make practical laboratory work compulsory; one, Clifton, has regard to special aptitude shown by boys; one alone, Bradford, would not enforce it at all.

The evidence as to cost is complicated; the questions were well arranged, but many of the answers give aggregate sums, without saying how many boys the outlay was calculated to supply. It would seem, however, that the costly appliances of Clifton, including chemical and physical laboratories and lecture-rooms with fittings of every kind, cost about 5*l.* per head of pupils intended to be taught; those of Newcastle about 4*l.* per head; of Giggleswick 3*l.*; of Taunton less than 2*l.*: that is to say, chemistry and physics may be taught for ever to one hundred boys with an original expenditure of 200*l.*, and cannot where money is plentiful cost more than 500*l.* For the further consolation of beginners and of poorer schools we learn that a Clifton master's apparatus for three chemical and three physical lectures a week cost 8*l.* once for all, exclusive of air-pump and balance, and that in lecturing for five years he has not spent 3*l.* a year; while the Taunton master announces that a man with leisure and dexterity to make his own apparatus can begin with table, gas, water, a few shelves, and 5*l.*; and adds that his own lectures cost only 6*d.* each.

Valuable opinions are reported as to the influence exercised by the universities on school teaching. All head-masters know that the mischief inflicted on education by the Oxford and Cambridge system is incalculable, and the opinion finds expression in these answers. To gain a science scholarship a boy must abandon during the last two or three years of his school course all subjects except science, with such a minimum of classics and mathematics as may secure him against a pluck in the Little-go; and, *mutato nomine*, the same is true of candidates for either classical or mathematical scholarships.

[1] Appendix to Report of Head-Masters' Committee, 1877. Answers to Questions on Natural Science.

[1] This is from private information. The returns given in the Report are not so high.

Those who think that school education should be general; that literature, mathematics, and science should share it in fair proportions; and that entrance scholarships at the university should be awarded to general excellence, will understand how the present system disheartens every thoughtful educator, who groans over the intellectual development of his best boys distorted in obedience to this tyranny of special prizes, which he nevertheless must win, or forfeit his reputation as a teacher.

An exhaustive list of text-books is given by the various schools. Some of them are valuable to the teacher only; others indispensable to the pupils. With very few exceptions their price is exceedingly moderate, though expensive books such as Watts' "Dictionary of Chemistry," and Weinhold's "Practical Physics," should have their place in the school library as books of reference.

It is clear that the publication of this Report marks a step in advance along the path of scientific education. It contains not opinions, but facts; not theories of what the teaching should be, but records of what it is; and this not scattered through the discursive pages of a Blue-book, but condensed into a pamphlet of thirty pages. Not less instructive is the comparative unanimity with which different schools, swayed by independent traditions, advancing on different lines, and ignorant of each other's movements, have worked out the same practical results and are teaching the same subjects by the same methods. The problem is virtually solved; the difficulties inherent in the recasting of an ancient system have disappeared so soon as they were honestly faced; and the head-masters, who perhaps looked shyly on advice from without, will listen to it, let us hope, when recommended by their colleagues. To this end the contents of the Report should be summarised, and circulated amongst the schools. It would be easy for the head-masters and science-masters of the schools which have answered questions to constitute an informal committee. A small working sub-committee would soon formulate a scheme of science teaching, based on the conclusions of the Duke of Devonshire's Commission, giving accurate particulars as to methods, books, tests, and cost of teaching chemistry and physics, with further information on the subject of museums, workshops, botanical gardens, and observatories; and this paper, drawn up in the simplest and most practical shape, might be sent at once to all first-class schools with the imprimatur of the entire committee. It would hardly fail to gain converts amongst present schools; each new head-master, appointed, as they are appointed now, with an understanding that they shall find room for science in their curriculum, would hail it as of the highest value; and when compulsory legislation comes, as come it must, the necessary details will all be ready to its hand.

W. TUCKWELL

FRANKLAND'S RESEARCHES IN CHEMISTRY[1]

Experimental Researches in Pure, Applied, and Physical Chemistry. By E. Frankland, Ph.D., D.C.L., F.R.S., &c. (London: Van Voorst.)

THE section (II.) that Dr. Frankland devotes to his researches in Applied Chemistry is not the least interesting of the work, though the chief topics are Gas and Water. The author's investigation of White's process for manufacturing hydrocarbon gas by passing steam over red-hot coke, and carbonising the gas in the retort, led to the clear distinction of the illuminating from the non-illuminating constituents of the hydrocarbon gas and of ordinary coal-gas. It was shown that marsh gas is valueless as a light producer during combustion, and that the luminosity of a gas flame is due to the heavier hydrocarbons present, whose illuminating value can be deduced from analysis and expressed in terms of olefiant gas. Such an indirect method of estimating the illuminating value of a sample of gas is certainly interesting, but it is clearly unsafe; for it involves the assumption that the illuminating value is directly proportional to the percentage of a hydrocarbon mixture of unknown constitution, calculated somewhat empirically into equivalents of the well-defined ethylene. Moreover, some recent experiments by Dittmar seem to show that ethylene does not contribute nearly so much to the luminosity of a hydrogen gas flame as benzole vapour. Hydrogen containing as much as 10 per cent. of ethylene gave a very feebly luminous flame, while hydrogen charged with only 3 per cent. of benzole vapour afforded a brilliant light when the gas was burned. Fortunately Dr. Frankland does not wholly rely upon the method in comparing—as he does in his introductory remarks on the gas investigations—the London supply of 1851 with that of 1876; for he has partially employed the photometer as a check. In 1851 the London gas supply contained 7·01 per cent. of olefiant gas, or its equivalent of other illuminating hydrocarbon, while in 1876 the percentage was 7·02. From these data it was concluded that there was no difference in illuminating power although the 1876 gas should be, according to the Act of Parliament, four candles better than that of 1851. Dr. Frankland says:—

"The improvement of the coal gas sold in London has been only imaginary, for no real alteration has been effected. It has been made to appear better, by testing it with improved burners; but, as consumed by the burners almost universally employed, it gives no more light in 1876 than it did in 1851—a conclusion which is confirmed by the results of simultaneous comparative trials made by Mr. Humpidge with two burners, one of them similar to those by which London coal gas was tested in 1851, and the other, the so-called 'gas referee's burner,' at present employed in testing London coal gas. At 4 P.M. on June 6, 1876, the gas supplied by the Chartered Company to South Kensington Museum gave, when consumed at the rate of five cubic feet per hour from the 1851 test-burner, a light equal to 11·1 standard candles, and on June 28, at 3 P.M., a light equal to 10·5 standard candles; but when the same gas was tested at the same hours by the present referee's burner, it gave, when consumed at the same rate, a light equal to 14·3 candles on June 6, and a light equal to 14·5 candles on June 28."

There is no doubt that the photometric determinations in the above cases substantially agreed with the analytical results, which latter may, in consequence, be accepted so far. The general result, however, is eminently unsatisfactory to all persons interested, save the gas company.

The second inquiry undertaken in connection with gas was that on the igniting point of coal-gas. The chief facts elicited possess so much general interest that we may mention them here. They were:—1. That coal-gas ignites at a much lower temperature than marsh-gas, but at a higher temperature than hydrogen or carbonic oxide.

[1] Continued from p. 219.

2. That the admixture of the vapour of carbon bisulphide does not sensibly lower the igniting point of coal-gas, although alone, or mixed with hydrogen or carbonic oxide, this vapour inflames at 400° F. 3. The Davy lamp, which is a protection in explosive mixtures of air and firedamp, is not safe in similar mixtures of air and coal-gas.

The third paper in the section discusses the possibility of making metallic magnesium available as a source of artificial light: and the last paper, relating to the use of illuminating materials, describes the construction of a gas-burner, in which the waste heat of the flame is made to raise the temperature of both air and gas to 500° or 600° F. before combustion, and thus to increase the luminosity of the flame.

The author's group of papers on water examination and purification, and on the treatment of sewage and other refuse, occupies nearly 300 pages of the volume before us. The value of Dr. Frankland's investigations in sanitary chemistry has been variously estimated, and a glance over the pages before us recalls the wordy warfare that has been waged between the upholders of Dr. Frankland's system of water analysis and of the conclusions founded upon the data afforded by it, and those that put their trust in the method devised by Messrs. Wanklyn, Chapman, and Smith. We anticipate that the republication of Dr. Frankland's papers will impart fresh vitality to a controversy that seemed, happily, to be on the wane. If, however, a renewal of the controversy is likely to lead to re-investigation and substantial improvement in the existing methods of water analysis, we shall not regret the re-opening of the subject.

The chief aim of all modern methods of water analysis is the detection and estimation of organic (especially sewage) contamination. Dr. Frankland seeks to attain the end in view by direct estimation of the "organic" carbon and nitrogen in the water, while Messrs. Wanklyn, Chapman, and Smith attempt to estimate the nitrogenous organic matter in water by breaking up the organic bodies and separating their nitrogen in the form of ammonia—"albumenoid ammonia." A rather extended experience in the use of both methods has led us to conclude that Dr. Frankland's plan, though nearly perfect in point of theory, is not as satisfactory in practice; while the rival method rests on a bad foundation, but is not likely to lead to error in excess. We may state this much without trenching too far upon technical ground, but we do so in order to justify the desire above expressed for a re-investigation of the subject, conducted with a view to render the theory and practice of water analysis equally satisfactory.

It is scarcely necessary to mention that we find in this section Dr. Frankland's valuable papers on the development of fungi in potable water; on the deterioration of potable water during its passage through cast iron mains and leaden service pipes; on the methods of softening "hard" water, and on the comparative purity of water from various geological strata, and from different sources such as mountain streams and lakes, rivers, shallow wells, artesian wells, and springs. In addition, experimental data are given showing the extent to which polluted water can be purified by various means and rendered fit for domestic purposes. These papers, together with those on sewage treatment, are of especial value to sanitary engineers, and will no doubt be more freely consulted in their present well-connected form than when scattered through other publications.

In Section III. Dr. Frankland returns to gases and vapours—evidently favourite subjects of study with him since the date of his discovery of the alcohol radicles—but now from the physical side. The first investigations detailed in the section are those upon the effect of pressure on combustion, which led him to conclude that the luminosity of ordinary flames is chiefly due to the presence of incandescent vapours or gases of high density, rather than to solid particles. The author's experiments on the combustion of hydrogen and carbonic oxide under great pressure proved that incandescent gases and vapours emit light in proportion to their density, and that a continuous spectrum can be afforded by dense gas as well as by solid or liquid matter. These observations led to the suspicion that the sun's photosphere consists of gases or vapours only, and ultimately to the commencement of a new line of research in conjunction with Mr. J. Norman Lockyer, who was then engaged on his researches on the physical constitution of the sun. Dr. Frankland was soon obliged to relinquish the investigation, owing to pressure of other work; but in Mr. Lockyer's hands it has since afforded results of the highest interest and value.

Excluding a chapter on climate, and some miscellaneous observations, the last research detailed in the volume before us is a highly important one on the source of muscular power. This inquiry is really complementary to the well-known investigation of Profs. Fick and Wislicenus on the same subject, for Dr. Frankland ascertained by direct calorimetrical determinations the potential energy locked up in muscle and in its chief products of oxidation—urea, uric acid, and hippuric acid—and proved that the store available was much less than would suffice to account for the work done by Fick and Wislicenus in the ascent of the Faulhorn. Frankland's experiments conclusively proved that the muscular force expended by the two professors in the ascent of the mountain must have been chiefly derived from the oxidation of non-nitrogenous matters, since it could not have been produced by the oxidation of muscle or other nitrogenous constituents of their bodies. This investigation is one of the most valuable in the section, and will be re-read with special interest in connection with the Rev. Prof. Haughton's latest researches.

We cannot conclude this short sketch of Dr. Frankland's admirable researches without giving expression to the hope we entertain that the well-arranged volume before us may prove to be but an instalment of the life-work of its distinguished author. J. EMERSON REYNOLDS

FLORA OF TROPICAL AFRICA

Flora of Tropical Africa. By Daniel Oliver, F.R.S., F.L.S., Keeper of the Herbarium and Library in the Royal Gardens, Kew, and Professor of Botany in University College, London. Assisted by other botanists. Vol. iii. Umbelliferæ to Ebenaceæ. Published under the Authority of the First Commissioner of Her Majesty's Works. (London: L. Reeve and Co., 1877.)

THE third volume of Oliver's "Flora of Tropical Africa" includes fourteen natural orders, mostly belonging to the sub-class Gamopetalæ of the Dicotyle-

dons. The two orders, Umbelliferæ and Araliaceæ, both by Mr. W. P. Hiern, and occupying the first thirty-two pages, were printed in 1871 along with vol. ii., the rest being all new. The district included in the present volume is of course the same as that mentioned in the previous ones, extending on each side of the equator for somewhere about fifteen degrees. It is divided into six regions, two on the west side of the continent, distinguished as Upper and Lower Guinea, two on the east side; Nile Land and Mozambique and the intervening region divided into the north and south-central. The district is therefore one of vast extent, and will probably yield many new forms when further exploration renders our knowledge of the country and of its productions more complete than it is at present.

The Umbelliferæ are represented in the flora by twenty-one genera and about forty-four species. The number of genera is small when contrasted with the thirty-four found in Britain. Several familiar British forms are found in this flora, of which it will only be necessary to mention *Sanicula europæa*, *Conium maculatum*, *Anthriscus sylvestris*, *Daucus carota*, and *Caucalis infesta*. Five genera are, however, endemic in Africa. The genus *Peucedanum* is interesting as including two species, *P. araliaceum* and *P. fraxinifolium*, both small glabrous trees. The Araliaceæ are unimportant, and represented by only three genera and fourteen species.

By far the most important monograph in the present volume is that on the Rubiaceæ, by Mr. W. P. Hiern. It occupies over 200 pages, or nearly as many as the monograph of the Compositæ, the joint production of Messrs Oliver and Hiern. The Rubiaceæ form a very extensive tropical and sub-tropical order, most richly represented in America. Tropical Africa, however, possesses seventy-eight genera, about thirty of which are endemic, and three of these are now described for the first time by Mr. Hiern. The genus *Coffea* is fully treated of by Mr. Hiern. The *C. arabica* occurs in four of the districts of the flora, but not in the two Central regions. The new coffee, *C. liberica*, Hiern, which promises to be of so much value, is here noticed. It is the source of the Liberian coffee, and probably of the Cape Coast coffee. The berries are said to be larger, the flavour finer, and at the same time the plant is more robust and productive than the ordinary *C. arabica*. The *C. arabica* has the flowers pentamerous, while in *C. liberica* they are 7- or 6-merous; or, according to the key to the species, 6 to 8-merous. The genus *Sarcocephalus* is an interesting one, the fruits cohering to form a pseudocarp known as the peach, or country fig, of Sierra Leone. The shape and colour of the pseudocarp is that of a strawberry, but in flavour it resembles an apple. Unfortunately if eaten to excess it acts as an emetic. Two familiar species of *Galium* are also included in the flora, namely, *G. aparine* and *G. mollugo*.

The Valerianaceæ are represented by the European *Valerianella dentata* only. The Dipsaceæ by three genera, *Scabiosa* yielding two familiar species. Both these small orders are by Mr. Hiern.

The Natural Family Compositæ, the joint production of Prof. Oliver and Mr. Hiern, occupies the chief place in the volume, as would be expected of the largest order of flowering plants. The number in the tropical region of Africa, 468 species, is small compared with the 1,300 species occurring in the Cape flora. In tropical Africa there are 117 genera, seventeen being endemic, and all of these latter either small or monotypic. Many of the forms are of great interest, and some of the genera very extensive. It is curious to meet with *Erigeron alpinus* in Abyssinia, along with *Senecio vulgaris*, and others, some of them familiar weeds. The genus *Tarchonanthus* forms a small tree, and it was upon a species of this genus that Dr. Welwitsch found the only species of *Viscum* he met with in Angola.

The Campanulaceæ are by Mr. W. B. Hemsley, who reduces the peculiar Abyssinian plants known as *Tupa* to the genus *Lobelia*, and describes two species, *L. rhynchopetalum* and *L. giberroa*. One species of *Lightfootia*, from Lower Guinea, is of interest. In it (*L. welwitschii*) the ovary is almost wholly superior, while in all others the ovary is inferior, the plant thus looking very unlike the other members of the Campanulaceæ. Four genera of Ericaceæ occur, but only one species of *Erica*, viz., *E. arborea*, thus contrasting with the Cape flora where the species are most numerous. The Ericaceæ, Plumbagineæ, and Primulaceæ are by Prof. Oliver, but are small and unimportant. The Myrsineæ and Sapotaceæ are by Mr. J. G. Baker, and the Ebenaceæ by Mr. Hiern, who has already published a monograph of this group.

The greater part of the volume is by Mr. Hiern, who contributes about 270 pages, while, along with Prof. Oliver, he contributes 207 pages more. About twenty pages each are contributed by Messrs. Baker and Hemsley, while eight only are from the pen of Prof. Oliver alone. This handsome volume of about 550 pages adds another to the long series of "Floras" now so well known and so highly appreciated that have from time to time issued from Kew.

W. R. McNab

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Marine Fossils in the Gannister Beds of Northumberland

I take the earliest opportunity to announce, through your columns (if you will allow me to do so), that on the 9th of this month (February), while conducting the usual weekly field-work in connection with this college, I had the good fortune to find marine fossils in the lower coal measures, or gannister beds of Northumberland. The locality is about half-way between the Stocksfield Station, on the Newcastle and Carlisle Railway, and the village of Whittonstall. As I have more than once insisted on the fact that hitherto no marine forms had been found in this series in this country, I wish to be the first to destroy the effect which that negative evidence may have produced. The importance which the study of the gannister fauna has assumed since the publication of Prof. Hull's recent paper on the Classification of the Carboniferous Rocks in the *Quarterly Journal of the Geological Society*, need not be dwelt on.

Full details of the find will be published elsewhere.

G. A. Lebour

College of Physical Science, Newcastle-on-Tyne, February 12

Liquids having a Specific Heat Higher than Water

In Nature, vol. xvii. p. 252, it is stated: "Hitherto water has been regarded as possessing a greater specific heat

than any other body excepting hydrogen. E. Lecker has shown to the Vienna Academy that mixtures of methylic alcohol and water have a specific heat higher than that of water, and accordingly take the second place," &c. Can you spare me a corner to point out that in 1869 the specific heats of some mixtures of ethylic alcohol and water were proved by Dr. A. Dupré and myself to be considerably higher than that of water, *e.g.* a mixture containing 20 per cent. alcohol, has a specific heat of 104·3, water = 100 (*Phil. Trans.*, 1869, 591 ; Watts's Dict., 2nd Supplement, 475). Moreover, we especially mentioned "that our experiments furnished the first example of a liquid having a specific heat higher than that of water." Since 1869 Dr. Dupré has estimated (*Proc. Roy. Soc.*, xx. 336) the specific heats of mixtures of methylic alcohol and water without finding any such mixture to possess a specific heat above that of water.

University College, London F. J. M. PAGE

Age of the Sun in Relation to Evolution

I HAVE read Mr. Plummer's letter along with his article in the *Popular Science Review*, and am surprised at his objections to my paper on the "Age of the Sun's Heat."

It matters not whether the sun's heat was derived from the contraction of his mass or from the falling in of meteorites, or of comets, as advocated by Mr. Plummer in the article referred to, we could only have had a supply of heat sufficient for twenty or thirty million years, at the present rate of radiation. Probably not much more than half of this would be available for the formation of the stratified rocks, and the development of life on the globe ; a length of time irreconcilable alike with geology and evolution. We are therefore compelled to look for some other source than gravity as the origin of the sun's heat. It will not do to lengthen this period by assuming that the rate of radiation was less during past ages than at present, because we should have to assume that the temperature in the past was also less, a conclusion at variance with the known facts of geology and of palæontology. I never supposed that the rate of radiation in the past may not have been greater than now. Nor did I ever suppose that there is any antecedent improbability whatever in the collision of stellar masses. What I maintained (*Quart. Journ. of Science* for July, 1877) was that the formation of a sun is an event which, on an average, can only be witnessed once in about 15,000 years, or the number of visible stars would be greater than it actually is. And this, I think, is a sufficient reason why we should not expect any historical record of such an event. Further, it does not necessarily follow that the two bodies coming into collision should possess equal mass or velocity in order to have their motion of translation converted into heat. If it be true that the stars derived their heat from loss of motion then this may quite well explain why their motions are so small. In a word, the energy which the sun has been dissipating into space through past ages, always existed in the form of motion. Collision only changed it from one form to another, viz., from the motion of translation to molecular motion.

JAMES CROLL

The "Phantom" Force [1]

IT might be supposed that permanent and entirely local or "internal" force-pairs of this kind acting on innumerable material couplets in a system would so disturb the individual energies of their motions that no general conclusion as to the total change of energy during the progress of such a system's motion could be drawn ; but the simple law that impulses act independently of each other and of existing motions soon shows that the whole gain of energy in the system is the sum of the separate gains in the several mass-couplets due to their absolute or several actions and reactions at every instant of the motion, and that when these abstract force-pairs are all permanent, the above constancy of the sum of their actual and potential energies is possessed by the whole system as perfectly as would be the case by one only of its couplets, or component pairs. That this is not merely an abridged expression for the resulting actual energy in all the possible different phases that such a system may go through, briefly stated for any initial and final configurations and initial motion of the system by means of the negative scale or potential function of all the several component force-pairs supposed known ; not merely, that is to say, a logical consequence of arbitrary and fanciful definitions, but a conclusion full of importance and of real natural signification depends, firstly, upon the fact that the thing defined as "impulse," or the gradient of the scale, which is here independent of the time and depends only on the mutual configuration, is not permanent by a very rare occurrence, but that it is often so, and under very various circumstances ; and again that this impulse, or flux of momentum, or gradient of energy, occurs in many other motions with conditions of equal simplicity ; and lastly, and above all, on the fact pointed out at the beginning of these reflections, that while we are able to use, and of our free will to call into existence force in innumerable ways, we learn from our experience that this impulse is invariably *caused or dictated by* a certain special efficacy or compulsion, which our power of exercising it as we please so as either to annul, to modify, or to increase it at will with the consequence of obtaining with it any effective impulses that we desire, shows us to be a different kind of quantity from the impulses that we either thus obtain, or that we see it producing in surrounding nature. Newton's second law of motion in fact recognises this specific difference between the magnitudes of a force and of its effect, when it asserts that forces *produce their whole effects* (that is to say, remain unaffected in their efficacies) whatever may be the state *of rest* or of motion of the bodies upon which they act. As it is found that forces or compulsions (measured as they are in statics by additions, subtractions, and oppositions to a standard force) are always proportional to the free impulse, or undisturbed acceleration of a mass-unit which they can produce, so that by taking the impulsive effect and the active compulsion of any one standard force as the units for measuring these quantities respectively, they are then said to be numerically equal to each other in every force ; it yet follows from their specific independence that they are not identical in kind as they are in measure. The same is true of the products obtained by multiplying them separately by any small space through which a force acts ; and it would be an obvious misstatement to assert that the sum of the works of a compulsion, and of the free impulse which it produces taken negatively, is constant when a force acts freely ; because this would be confusing in one sum two different quantities ; a result which it seems must arise from the simple fact that our part in mechanical "compulsions" distinguishes and removes them from the category of impulses to which they would otherwise belong, and leads us to regard them as *the causes of* the impulses which we observe. The language adopted by Newton (and used also by D'Alembert) in the proposition quoted at the beginning of this letter is that in a proper mechanical system, *compulsions equal to* the observed impulses reversed, will (as is obvious) arrest in their origin all changes of motion in the system, and will (with the immutable force conditions proper to the system) hold in balance, or give a complete account of all the forces (other than those immutable ones) acting upon it. Using the principle of virtual velocities in this case of equilibrium of balancing forces, Newton expresses the rule for exploring all the mechanical efficacies (superadded to the immutable ones) acting in the system by concluding that the sum of all the similar "works of *compulsion*," or of all the "actions" in a short time corresponding to a small motion of the system, when the reversed ones have been introduced, will be a constant quantity. Thus both Newton and D'Alembert agree in this, that they recognise in forces *causes which differ from* the effects which they produce. By what similar laws of work found to hold true in a proper conservative system the modern science of energy (which deals with the phenomena of causation in a wider and more diversified form) seeks to extend the method of cancelling the counteracting causes, or the principle of energy conservation here laid down by Newton for a mechanical one, to the far larger, but less thoroughly explored and exhausted field of all the onward flowing streams of physical agencies which we perceive following their natural bents or inclinations around us, I will presently endeavour to explain. It should be noticed in connection with this general extension of the principle, that the "work" of a force *in a short time*, however fixed its efficacy, or its rate of doing work *in a short space* (or of producing *momentum* in the short time) may be, is incidental, and not a fixed quality of the agent force like its faculty of tension, since a force as often diminishes as increases actual energy by a momentary action, and thus no fixed rule is drawn from the natural tendency of force to impart momentum, that potential energy necessarily becomes, or even necessarily tends to become, actual energy in every mechanical energy-transformation. The mechanical stress of friction is an example of the opposite tendency ; and it also furnishes us with an example of a force whose *working power* only, and not its motive tendency, is a mechanical "*agent*" which we can summon up at will ; but

[1] Continued from p. 303.

of which we still regard the motive tendency as similar to that of other mechanical forces, because it can maintain equilibrium with them.

If the natural office of force as a bond between space and matter is to ward off contact between material points, and to endow them with impenetrability (for this marked feature as far as it has been explored appears to be inherent in all matter) by absorbing at the proper places the energy of motion, and by curbing and accelerating it elsewhere according to these dictates, it must be implanted in material points in such permanent pairs as have just now been described; for the third law in Newton's master-summary is often held (with how much correctness, perhaps, may be questioned), to assert that all the forces of nature consist of an action and a reaction in equal and opposite pairs, and be omnipresent with a particle to protect it, its permanent impulse, or rate of doing work, being at the same time referrible to the space or distance between the mutually impinging, or colliding pair of atoms. A mechanical system so constituted, as we have seen, if not disturbed by the forces of foreign bodies outside of it will have the sum of its actual and potential energies constant. If we include those foreign bodies (endowing their forces at the same time with persistency), and if we find that the whole material universe as far as we can explore it admits of being comprehended in one system of this kind, a mechanical explanation of all known physical agencies might thus, apparently, be arrived at. In every particular of the motion of any group of bodies in it, however, except the single one of its total energy, we would be obliged to abandon (as Newton does) the local centres of reckoning of the several force pairs, and betake ourselves to the mass-centre of the group as our origin of reference for noting all the forces, and tracing all the motions of a body completely in its wanderings through the system. If this obligation, and the end to which it leads us of referring force at last to perfectly abstract realms of space and matter terrifies and affronts the scientific sense, it is sufficient consolation to observe that if force were not ultimately so referred, and if its impulse was exerted in those local spaces only in which we find it acting apparently as a simple action and reaction, there would be as many distinct kinds of energy of motion and of configuration (which we would still retain as expressing the local laws of force) as there are local spaces, with countless complex rules for mutual equivalence of these several energies connected with the common path of the body in several of them together, which would effectually defy even a modern physicist to disentangle and employ! We may rest thankfully contented with the laws that Newton has traced out. But does the simple mechanical system which we have just imagined really represent that of nature? Are nature's force pairs really all permanent? (We will suppose that they are all dual and reciprocal, for, as will presently be noticed, a special and peculiar explanation only can be given of forces which are absolutely external or solitary without any physical qualification); and is the sum of their mechanical energies a constant quantity, as we know that it becomes when all the other kinds of energy in material nature are added to it? The answer is very obvious, but it betrays at the same time a complete ignorance of the extent and depth of the question that we put. The readily-preferred response is "No; the sum of the mechanical energies, as far as they can be recounted, is not constant, for numberless forces, and mechanical energies proceeding from them are being constantly produced by heat, radiation, chemical action, and other physical agencies, or are being employed to renovate those agents with energy in equivalent stores, but whose special kinds *are not* mechanical." At the same time the progress of physical inquiry reveals to us in the operations of these agents microscopical, or rather hyper-microscopical, actions of force, and invisible charges of actual energy, with which our earlier knowledge of these agencies was entirely unacquainted, and a little step of inductive generalisation only is needed (I believe that this view was unfolded by Helmholtz many years ago, but I have not been able to rediscover his remarks and demonstrations[1]) to suppose that all physically-generated forces form but visible and changing resultant-links in an invisible chain of persistent forces, of which we hold some of the most prominent loops in our hands, and thoughtfully wonder what they are.

In his Glasgow lecture on "Force" (in NATURE, vol. xiv. p. 463), an important hint was offered by Prof. Tait regarding an apparent character of potential energy, founded on the mathematical condition that quantities whose units of measurement are of the same dimensions in space, mass, and time, are of the same kind. It follows from this that the potential energy of a force, or the energy received from and transferred to it is of the same kind as actual energy of material motion, because they are both measured by the same combination of the units of space, time, and mass.

Let us first observe that it cannot be *matter* in motion which constitutes the potential energy, unless this matter can traverse itself and other matter freely (because, itself the cause of force, it cannot itself experience any), and therefore that it is something not matter, but both resident in and proportional to matter, and also free; and that its energy of motion as an occupant of matter is actual, and in a state of freedom is potential energy. Imagine a perfect reflection of the material universe to be formed by a plane in space; then the changes of energy of motion of the reflected image of any mass particle taken negatively are equal and opposite to the similar changes of energy of the particle itself, and would measure in a kinetic form the amount of the work of force upon it; but the *tendency* of energy of this form and of the actual form to pass into each other would not be accounted for. In our present knowledge of its transformations it scarcely seems possible that a simpler picture of potential energy as a form of energy of motion than this downright imitation of the actually-existing motions could be reasonably offered. The proposition that force must be a process of transformation of *a new energy of motion*, so astonishingly complex, I confess staggered me, and even led me to doubt if the simple laws of force and motion laid down in the "Principia" could be really so perfect and complete as they appear, amid the pell-mell of motions which the thought suggests! I began this letter shortly before going to Plymouth, intending to recommend much more careful experiments than even Mr. Crookes has carried out with the radiometer, and with his recent, most effective form of the instrument, the otheoscope, in order to test and examine the question of the laws of force (especially with the idea of possibly isolating a single force) seriously; and though much induced to do so by the warm and timely words of commendation passed on Mr. Crookes' labours in his opening address to Section A at Plymouth (NATURE, vol. xvi. p. 314) by Prof. G. C. Foster, I have been unable from other occupations to finish it until now. But I have entirely abandoned my original intention, in great part, by reason of a new light on the exceedingly abstruse and puzzling question which the able remarks by "X" in NATURE (vol. xvi. pp. 438, 457) have afforded me about the real characters of force and of potential energy.

Newcastle-on-Tyne A. S. HERSCHEL

(*To be continued.*)

[1] They are, I find, contained in a paper of some length in vol. vi. of "Taylor's Scientific Memoirs," 1853, pp. 114—162.

Cumulative Temperatures

UNDER the above heading, among the "Meteorological Notes" in your issue of last week, I notice the announcement that "To simplify the difficulty of obtaining sums of temperature . . . M. von Sterneck has recently proposed to obtain these indirectly by observation of the sums of actions produced by the temperature." And that M. von Sterneck's proposal is to employ for this purpose a pendulum clock in which the variation of rate due to the raising or lowering of the centre of gravity of the pendulum under variations of temperature is, through its influence upon the daily error of time shown on the dial, employed for determining the mean temperature of the air throughout the twenty-four hours. And the notice concludes by saying that M. von Sterneck has also proposed to apply the same principle to determine the variations in atmospheric pressure and in the intensity of magnetism.

In reference to this subject it is only right to point out to the readers of NATURE (a term synonymous with the general body of lovers of science all over the world) that the merit of these suggestions is, by priority, due to one of our own countrymen, Mr. W. F. Stanley, who, at the *soirée* given by the President of the Royal Society as far back as April, 1876, exhibited two instruments in which the chronometrical method of determining thermometric and barometric averages was carried out with very marked success.

One of these to which the name chrono-thermometer was applied, consisted of a clock, the pendulum of which was a mercurial thermometer, its centre of gravity being raised or lowered by the expansion or contraction of a column of mercury under variations of temperature. In the other instrument, or chrono-barometer, the pendulum consisted of a glass tube con-

taining a column of mercury, the rising and falling of which under influences of barometric pressure raised or lowered the centre of gravity of the pendulum and varied the rate of the clock accordingly. The clock-train in both instruments was so arranged that the dial-readings could, by an exceedingly simple calculation, be interpreted in terms of mean daily thermometrical or barometrical variations. As the alteration in the lengths of the pendulums takes place second by second throughout the day, it follows that the daily error of time shown on the dial must be proportional to the mean of the variations of the thermometer or barometer during the same period.

The method of estimating, by observation of the rate of an uncompensated time-keeper, the mean amount of heat received during any given period, without the necessity of recording the actual temperature at any particular time, is not by any means new, for the chronometrical thermometer, an instrument which has for many years been employed at the Royal Observatory for testing the rates of chronometers under variations of temperature, is founded upon the same principle. This instrument consists of a chronometer in which the usual compensation for temperature is reversed; that is to say, in the balance the positions of the brass and the steel are interchanged, the latter being outside, so that variations of temperature produce an exaggerated effect upon the rate of the instrument.

M. von Sterneck is probably the first to suggest the employment of the chronometrical method to the determination of mean variations in the vertical intensity of terrestrial magnetism, but he has, I think, been anticipated in its application to the computing of thermometric and barometric averages.

Scientific Club, February 18 CONRAD W. COOKE

BACTERIA IN WATER

IT is well known that water—whether tap or ordinary distilled—possesses the property of contaminating, with a growth of bacteria, any "cultivation" liquid inoculated with it, but the morphological condition in which these organisms occur in it is open to question; it may be supposed on the one hand, that they exist as developed bacteria, and are not apparent under the microscope merely in consequence of their scarcity, as shown by Mr. Lister in the account of his recent admirable investigation of the lactic fermentation, to be the case with some specific ferments, or, on the other hand, that they are present as "germs" of the bacteria, bodies yet far more minute than the mature forms, and on that account invisible—ultra-microscopic. Which of these alternatives is true I have endeavoured to determine by experiment, the details of which will shortly be published, and the general result is here briefly communicated.

M. Pasteur has recently stated in the *Comptes Rendus*, that if a cylinder of water be allowed to stand for several weeks at a constant temperature, the organisms in it sink to the bottom, leaving the upper portion free, and incapable of contaminating. Following this method and placing a vessel of ordinary distilled water in an apparatus constructed for the purpose of maintaining an invariable temperature, after seven or eight days a specimen of the water was taken from the bottom of the vessel by a pipette closed with the finger and dipped into it. The water so taken was in appearance perfectly bright and pellucid, but under the microscope it was found to contain amorphous particles, some spores of filamentous fungi, micrococci in great numbers, bacteria of the common form (*B. Termo*) and bacilli (long and extremely slender filaments). All of these forms were motionless, or exhibited only Brownian movement. No such forms could be detected in the upper layers of the water, nor in a specimen taken from the bulk of that from which the experiments were made. As regards limpidity, there was no difference between the top and bottom portions.

I have made four experiments with specimens of water obtained from two different sources, and in all I have been able by this method of subsidence to prove the presence of organisms in great numbers in the sediment. It may be mentioned that they stain with facility by Hæmatoxylon, and are thereby rendered more readily apparent.

These observations show that bacteria occur in water under their usual forms, and that they are not generally distinguished on account of their small number, in any one portion of the water, when disseminated through its mass. The observed contaminating property of distilled water is thus accounted for without necessitating the assumption of "germs" of any sort, an hypothesis which is unsupported by observation.

The examination, by cultivation, of the difference in contaminating properties of the upper and lower layers, stated by M. Pasteur to exist, has as yet been inconclusive.

G. F. DOWDESWELL

OUR ASTRONOMICAL COLUMN

THE URANIAN SATELLITES, ARIEL AND UMBRIEL.—The following positions of the two interior satellites of Uranus for the ensuing fortnight are derived from the data furnished by Mr. Marth in the January number of the *Monthly Notices* of the Royal Astronomical Society, and are for 11h. G.M.T., or about the time of the planet's meridian transit. There must be many telescopes in this country which will command the two exterior satellites, Titania and Oberon, but, so far as we know, neither Ariel nor Umbriel have yet been certainly observed here with any but Mr. Lassell's reflectors. Prof. Newcomb states that Ariel is intrinsically brighter than Umbriel; he thinks that Ariel at least belongs to that class of satellites of which the brilliancy is variable, and dependent on its position in its orbit, and he adds that the evidence of variability of some kind seems indisputable, since he has repeatedly failed to see it with the Washington refractor when the circumstances—distance from the primary not excepted—were favourable, and when the next satellite, Umbriel, though less favourably situated, was visible. "On the other hand," he remarks, "there were two occasions, 1874, January 28, and 1875, March 25, when it was surprisingly conspicuous," and at these times the angle of position was about 350°. Prof. Newcomb further expresses the opinion that where any difficulty is experienced in seeing the outer satellites, he would not hesitate to pronounce it impossible to see the inner ones.

Nevertheless, the success which has attended the search for one, at least, of the satellites of Mars by English observers who are provided with large instruments, may perhaps induce them to look for the close satellites of Uranus at the present opposition.

	Ariel.		Umbriel.	
Feb. 22	Pos. 179°	Dist. 13″·6	Pos. 29°	Dist. 14″·6
,, 23	,, 11	,, 15·1	,, 355	,, 17·4
,, 24	,, 207	,, 11·2	,, 214	,, 13·1
,, 25	,, 77	,, 5·2	,, 178	,, 18·6
,, 26	,, 340	,, 8·7	,, 40	,, 11·5
,, 27	,, 181	,, 14·2	,, 0	,, 19·5
,, 28	,, 13	,, 14·8	,, 228	,, 10·0
March 1	,, 211	,, 10·2	,, 183	,, 20·3
,, 2	,, 95	,, 4·9	,, 59	,, 8·6
,, 3	,, 344	,, 9·8	,, 5	,, 20·8
,, 4	,, 183	,, 14·6	,, 253	,, 7·5
,, 5	,, 15	,, 14·4	,, 187	,, 21·1
,, 6	,, 216	,, 9·2	,, 91	,, 6·9
,, 7	,, 112	,, 5·1	,, 9	,, 21·2

PIGOTT'S OBSERVATIONS OF VARIABLE STARS.—Some years since it was suggested, we believe, by Prof. Argelander, that the Royal Society might have in its possession manuscripts of Edward Pigott of York, amongst which might be found observations of variable stars that would prove of importance in the history of their fluctuations. It would appear, however, that none of Pigott's papers are preserved in the Society's *Archives*, an ineffectual search having been lately made for them.

THE TEMPLE OBSERVATORY, RUGBY.—Mr. G. M. Seabroke, as Curator of the Temple Observatory, has issued a Report upon proceedings during the year 1877. The whole of the measures of double-stars, 398 in number, during the last three years up to the time of dismantling the old Observatory, have appeared in vol. xliii. of the *Memoirs* of the Royal Astronomical Society. More recently investigations into the motions of stars in the line of sight by the spectroscopic method have occupied Mr. Seabroke's attention, but the chief work in the year has been the rebuilding of the Observatory. The Report contains an outline of its history and a description of the instruments to which reference may be made in future years. The equatorial of 8¼ inches aperture, by Alvan Clarke, was formerly in the possession of the Rev. W. R. Dawes, and an interesting letter from him upon its capabilities is appended to the Report. Not the least notable of its performances is its having shown the close satellite of Saturn, Mimas, on many occasions, and we know that its former possessor was not likely to have mistaken faint stars for the satellite.

The observatory is open to the members of the school at certain hours on fine evenings when opportunities for observing with the equatorial and transit are afforded them. It should be mentioned that in addition to the Alvan Clark refractor the observatory possesses a twelve-inch With-reflector which is chiefly used with the spectroscope.

The cost of the new observatory and house for the curator, upwards of 1,230*l*., has been defrayed by subscriptions from the masters, old Rugbeians, and others in the school, upon the occasion of its tercentenary.

GEOGRAPHICAL NOTES

AFRICAN EXPLORATION.—The two African Societies of Berlin, which are now combined, have resolved to turn their attention to practical (*i.e.*, commercial) objects as well as scientific ones with regard to the great continent in which the travels of Cameron and Stanley have revealed vast stores of the most varied products. The twin societies therefore invite all German merchants, manufacturers, &c., to participate in their efforts to open up a great African commerce, and announce that the German Government is ready to grant a preliminary sum of 100,000 marks (5,000*l*.) to further the object in question. The Germans seem determined that no single nation, more especially England or Portugal, shall have the supremacy on the Congo. In Switzerland a new geographical society has been formed for the same object as the above.—An official telegram from Zanzibar to Brussels announces the death at Zanzibar of Dr. Maes and Capt. Crespel, who were sent out by the International African Association as leaders of an exploring colony in Central Africa. With them were M. Cambier and Ernest Marno, and they were to establish a station somewhere in the Tanganyika region, which would form a centre of further exploration. The death is also announced of Capt. Elton, who, with Mr. Cotterill, was surveying the route between the north end of Lake Nyassa and the east coast.

ARCTIC EXPLORATION.—The Committee on Naval Affairs of the U.S. Congress have adopted a report from Mr. Benjamin A. Willis recommending the equipment of an Arctic expedition as proposed by Capt. Howgate. At a public meeting held by the New York Geographical Society to discuss the subject of polar exploration a paper by Capt. Howgate was read on his intended colony. Lord Dufferin, governor-general of Canada, was elected an honorary member of the Society, and returned thanks in his usual style. He referred to himself as "a potentate whose sceptre touches the pole, and who reigns over a larger area of snow than any monarch of the earth."

THE PAMIR.—We learn from the *Turkestanskiya Védomosi*, that two members of the Pamir expedition, MM. Skassi and Schwarz, have returned to Tashkend, M. Severtsoff remaining for some time at Osh. The expedition, which started in September last, has met with great difficulties from deep snow and the cold weather, the thermometer falling as low as 31° Celsius below zero. Owing to the absolute want of forests, wood was brought by yaks. No inhabitants were found in the Pamir, nor even in the valley of Alay, the Kirghiz having already left the valley for warmer regions. The rarity of air on those great heights, which exceed 15,000 feet, occasioned much suffering to the members of the expedition. The Valley of Alay was reached from that of Fergana, by way of the Shart Pass, and from the Alay the expedition followed the path which was followed by Gen. Skobeleff in 1876. M. Severtsoff reached as far as the Lok-sai River, which the natives said flows into the Lob-nor, under the name of the Tarim-gol. Thence he was compelled by the deep snow to return, without reaching the problematic meridional ridge which was the aim of the expedition. Prof. Schwarz has determined the latitudes and longitudes of six places, and has made numerous magnetical observations; a complete survey of the route was made by the topographers, the heights of a hundred points were determined, partly barometrically and partly geodetically. M. Severtsoff has brought in a large ornithological collection.

EDUCATIONAL TRAVEL.—We learn that a society is in course of formation at St. Petersburg for the organisation of travels for children and for young men. The travels of the children are intended for the general development of the intellectual faculties and of the power of observation, and those of the young men will be arranged so as to give them a practical knowledge of some branch of science, together with an acquaintance with their own country. The travellers will be divided into several groups—natural sciences, history, ethnography, &c., and each section will be placed under the leadership of some well-known specialist. The success of the botanical and geological excursions, which were organised during several summers by the members of St. Petersburg and Moscow Societies of Natural Sciences, lead us to expect that the new enterprise will be successful.

PRSHEVALSKY AND MACLAY.—The Russian Geographical Society has received telegrams from Col. Prshevalsky, dated Fort Alexandrovsky, announcing that the traveller is now recovering from an illness, and will continue his journey to Tibet; and from Dr. Mikluho-Maclay, announcing his return from New Guinea to Singapore.

SEA TRADE WITH SIBERIA.—We learn that several Bremen and Moscow merchants have formed a company for sea trade with Siberia. A large steamer, with two barges and a small river steamer on board, will start from Bremen in July next for the mouth of the Ob or of the Yenisei. Leaving the river steamer and the barges for river communication, the large steamer will return with Siberian wares.

GEOGRAPHICAL BIBLIOGRAPHY.—In the last part for 1877 of the *Zeitschrift* of the Berlin Geographical Society will be found a copious list, covering 100 pages, of the principal geographical works published between November, 1876, and November, 1877.

RUSSIAN GEOGRAPHICAL SOCIETY.—The Great Constantine gold medal of the Russian Royal Geographical Society was awarded this year to M. Zakharoff for his remarkable Manchurian dictionary, the result of many years' study of the language and life of the Manchurians, during his residence as Consul at Kuldja. The gold medal of Count Lütke was awarded to Capt. Rykacheff, of the Physical Observatory of St. Petersburg, for his researches into the distribution of atmospherical pressure throughout Russia. Small gold medals were awarded to M. Marx for ten years' meteorological observations at Yeniseisk, and to Col. Tillo for his levelling between the Aral and Caspian.

A NEW UNDERGROUND MONSTER

A RECENT communication from Fritz Müller, of Itajahy, in Southern Brazil, to the *Zoologische Garten* contains a wonderful account of the supposed existence of a gigantic earthworm in the highlands of the southern provinces of Brazil, where it is known as the "Minhocao." The stories told of this supposed animal, says Fritz Müller, sound for the most part so incredible, that one is tempted to consider them as fabulous. Who could repress a smile at hearing men speak of a worm some fifty yards in length, and five in breadth, covered with bones as with a coat of armour, uprooting mighty pine trees as if they were blades of grass, diverting the courses of streams into fresh channels, and turning dry land into a bottomless morass? And yet after carefully considering the different accounts given of the "Minhocao," one can hardly refuse to believe that some such animal does really exist, although not quite so large as the country folk would have us to believe.

About eight years ago a "Minhocao" appeared in the neighbourhood of Lages. Francisco de Amaral Varella, when about ten kilometres distant from that town, saw lying on the bank of the Rio das Caveiras a strange animal of gigantic size, nearly one metre in thickness, not very long, and with a snout like a pig, but whether it had legs or not he could not tell. He did not dare to seize it alone, and whilst calling his neighbours to his assistance, it vanished, not without leaving palpable marks behind it in the shape of a trench as it disappeared under the earth. A week later a similar trench, perhaps constructed by the same animal, was seen on the opposite side of Lages, about six kilometres distant from the former, and the traces were followed, which led ultimately under the roots of a large pine tree, and were lost in the marshy land. Herr F. Kelling, from whom this information was obtained, was at that time living as a merchant in Lages, and saw himself the trenches made by the "Minhocao." Herr E. Odebrecht, while surveying a line of road from Itajahy into the highlands of the province of Santa Caterina, several years ago, crossed a broad marshy plain traversed by an arm of the river Marombas. His progress here was much impeded by devious winding trenches which followed the course of the stream, and occasionally lost themselves in it. At the time Herr Odebrecht could not understand the origin of these peculiar trenches, but is now inclined to believe that they were the work of the "Minhocao."

About fourteen years ago, in the month of January, Antonio José Branco, having been absent with his whole family eight days from his house, which was situated on one of the tributaries of the Rio dos Cachorros, ten kilometres from Curitibanos, on returning home found the road undermined, heaps of earth being thrown up, and large trenches made. These trenches commenced at the source of a brook, and followed its windings; terminating ultimately in a morass after a course of from 700 to 1,000 metres. The breadth of the trenches was said to be about three metres. Since that period the brook has flowed in the trench made by the "Minhocao." The path of the animal lay generally beneath the surface of the earth under the bed of the stream; several pine trees had been rooted up by its passage. One of the trees from which the Minhocao in passing had torn off the bark and part of the wood, was said to be still standing and visible last year. Hundreds of people from Curitibanos and other places had come to see the devastation caused by the Minhocao, and supposed the animal to be still living in the marshy pool, the waters of which appeared at certain times to be suddenly and strangely troubled. Indeed on still nights a rumbling sound like distant thunder and a slight movement of the earth was sensible in the neighbouring dwellings. This story was told to Herr Müller by two eye-witnesses, José, son of old Branco, and a step-son, who formerly lived in the same house. Herr Müller remarks that the appearance of the Minhocao is always supposed to presage a period of rainy weather.

In the neighbourhood of the Rio dos Papagaios, in the province of Paranà, one evening in 1849 after a long course of rainy weather, a sound was heard in the house of a certain João de Deos, as if rain were again falling in a wood hard by, but on looking out, the heavens were seen to be bright with stars. On the following morning it was discovered that a large piece of land on the further side of a small hill had been entirely undermined, and was traversed by deep trenches which led towards a bare open plateau covered with stones, or what is called in this district a "legeado." At this spot large heaps of clay turned up out of the earth marked the onward course of the animal from the legeado into the bed of a stream running into the Papagaios. Three years after this place was visited by Senhor Lebino José dos Santos, a wealthy proprietor, now resident near Curitibanos. He saw the ground still upturned, the mounds of clay on the rocky plateau, and the remains of the moved earth in the rocky bed of the brook quite plainly, and came to the conclusion that it must have been the work of two animals, the size of which must have been from two to three metres in breadth.

In the same neighbourhood, according to Senhor Lebino, a Minhocao had been seen several times before. A black woman going to draw water from a pool near a house one morning, according to her usual practice, found the whole pool destroyed, and saw a short distance off an animal which she described as being as big as a house moving off along the ground. The people whom she summoned to see the monster were too late, and found only traces of the animal, which had apparently plunged over a neighbouring cliff into deep water. In the same district a young man saw a huge pine suddenly overturned, when there was no wind and no one to cut it. On hastening up to discover the cause, he found the surrounding earth in movement, and an enormous worm-like black animal in the middle of it, about twenty-five metres long, and with two horns on its head.

In the province of São Paulo, as Senhor Lebino also states, not far from Ypanema, is a spot that is still called Charquinho, that is, Little Marsh, as it formerly was, but some years ago a Minhocao made a trench through the marsh into the Ypanema River, and so converted it into the bed of a stream.

In the year 1849, Senhor Lebino was on a journey near Arapehy, in the State of Uruguay. There he was told that there was a dead Minhocao to be seen a few miles off, which had got wedged into a narrow cleft of a rock, and so perished. Its skin was said to be as thick as the bark of a pine-tree, and formed of hard scales like those of an armadillo.

From all these stories it would appear conclusive that in the high district where the Uruguay and the Paranà have their sources, excavations, and long trenches are met with, which are undoubtedly the work of some living animal. Generally, if not always, they appear after continued rainy weather, and seem to start from marshes or river-beds, and to enter them again. The accounts as to the size and appearance of the creature are very uncertain. It might be suspected to be a gigantic fish allied to *Lepidosiren* and *Ceratodus*; the "swine's snout," would show some resemblance to *Ceratodus*, while the horns on the body rather point to the front limbs of *Lepidosiren*, if these particulars can be at all depended upon. In any case, concludes Herr Müller, it would be worth while to make further investigations about the Minhocao, and, if possible, to capture it for a zoological garden!

To conclude this remarkable story, we may venture to suggest whether, if any such animal really exist, which, upon the testimony produced by Fritz Müller, appears very probable, it may not rather be a relic of the race of

gigantic armadilloes which in past geological epochs were so abundant in Southern Brazil. The little *Chlamydophorus truncatus* is, we believe, mainly, if not entirely, subterranean in its habits. May there not still exist a larger representative of the same or nearly allied genus, or, if the suggestion be not too bold, even a last descendant of the Glyptodonts?

SUN-SPOTS AND DECLINATION RANGES

THE excellent article by Mr. Broun in a recent number of NATURE puts before us in a very clear manner the strong grounds that we have for believing in a true connection between sun-spots and terrestrial magnetism. If the argument were not already sufficiently powerful it might be yet further strengthened by bearing in mind that not merely do the most prominent inequalities march together in these two phenomena but the correspondence extends likewise to those waves of shorter period that ride as it were on the back of the longer ones. In a paper which is now before the Royal Society I have shown this intimacy of correspondence by comparing together the sun-spot and declination range records for the cycle extending from the minimum of 1855 to that of 1867. All the prominent sun-spot waves are reproduced by magnetic declination waves, the latter, however, invariably lagging behind the former.

Then with regard to the long period cycle under discussion I make it to begin for sun-spots with September 15, 1855, which was a minimum point, and to end with March 15, 1867, which was another minimum point. On the other hand the corresponding cycle for declination range begins with February 15, 1856, and ends with August 15, 1867. Thus the length of period is the same in both; the magnetic cycle lagging, however, five months behind that for sun-spots.

I may also mention that I am at present comparing together the Prague declination ranges with the sun-spot curve determined from Hofrath Schwabe's observations, and although the comparison is not finished, I believe that this lagging behind will form a prominent feature of the results. Further back than Schwabe we cannot go, as the sun-spot records are not sufficiently accurate for this kind of work.

I am not sure, however, that I quite agree with Mr. Broun when he says "no doubt the admission of the existence of a causal connection between the two phenomena is opposed to the hypothesis, which many other facts render wholly untenable, that the magnetic variations are due to the heating action of the sun."

As far as sun-spots and declination ranges are concerned, what are the facts regarding the connection between them? These are two in number. In the first place, all the considerable oscillations of the sun-spots are reproduced in the declination-ranges. Secondly, the reproductions in the declination-ranges lag, it appears, behind the corresponding sun-spot waves. This latter fact strikes me as being rather in favour of the view which regards declination-ranges to be (like temperature-ranges) in some way the result of an influence from the sun which is of the nature of an emanation or radiation. But I will not press the point except to remark that this and a host of other questions, some of them of great importance, must wait for their solution until we shall have obtained a sufficiently complete and continuous record of solar activity, and along with it an equally complete and continuous record of the radiant power of the sun.

From the observatories already established, we have a reasonable prospect of receiving good magnetical information, and there is abundance of meteorological activity, but it is nearly, if not absolutely, impossible, from the observations already made, to tell whether the sun be hotter or colder as a whole, when there are most spots on his surface. The sooner we get to know this the better for our problem.

BALFOUR STEWART

THE ISLANDS OF ST. PAUL AND AMSTERDAM

AS is now well known, a French expedition visited these islands towards the close of 1874 for the purpose of watching the transit of Venus across the sun on December 9 of that year. M. G. de l'Isle accompanied the expedition as botanist; Dr. Rochefort, with M. Vélain to assist him, were to look after the zoological and geological departments. M. Vélain, who was a pupil of Prof. Lacaze-Duthiers has just published, in the *Archives de Zoologie Expérimentale et Générale* (tome 6, 1877), a most interesting account of these islands and their fauna, with, in addition, a very detailed account of the collections of shells made. We are indebted to the extreme kindness of M. Vélain for the excellent illustrations which accompany this notice, which are taken from the original memoir.

FIG. 1.—Ninepin Rock.

If the reader wishes to fix the exact position of these curious islands he has only to trace along the line of lat. 40° S., and about mid-way in the Southern Ocean between the Cape of Good Hope and Melbourne, near long. 80° E., he will find them.

Their discovery has been claimed by the Dutch and the Portuguese. Placed just in the grand ocean route for all vessels leaving the Cape for Australia or China, they were doubtless, despite their isolation, long known. The history of our knowledge of them from 1522 to the present day is well, though briefly, written by M. Vélain.

The *Novara* called at St. Paul in 1857, and stayed for fourteen days, and we are indebted to Hochstetter for an excellent account of the geology of the island, though

the weather was so bad at the time that the collections made were not numerous.

In June, 1871, the English frigate *Megæra* was wrecked on this island and most of the 400 souls that were aboard her had to reside on it for over three months.

On September 30, 1874, the members of the Transit of Venus expedition landed on St. Paul and spent thereon over three months.

Both the islands are essentially volcanic. In 1696 when van Vlaming visited St. Paul, the vast crater occupied its central part, and was above and quite isolated from the sea, and it seems to have been even thus in 1754, but at present the sea flows freely into it, and at the place of communication there is a depth of upwards of six feet. It attains a height of about 250 metres and its contour line is not much more than five nautical miles. A little to the north of the entrance to the crater where the sea has broken in there is a wonderful pinnacle of basaltic lava, which receives the name of the Ninepin rock (Fig. 1). The rocks composing it are trachitic, of a compact texture, but more or less zoned. These rocks, full of silex, and poured forth in great measure under the sea, exhibit still the traces of the energetic alterations which they underwent, not only at the moment of their emission, but also after their complete solidification, for they have been traversed since their formation in every way; not only numerous fissures forced up by the impetuous escape of gaseous emanations but by the force of geysers, which latter considerably increased the amount of silex on the rock, and this so much so that the walls of such fissures through the trachytic rock are formed of a very able solid enamel of silex which is rarely hollow, and all the alkalies have totally disappeared. A microscopical examination shows, amid a highly developed amorphous paste, crystals of felspath, and pyroxene, with notable quantities of silex, amorphous (opal) or crystalline (tridymite); but the lavas of different periods of eruption seem to differ in their compositions.

A wonderful core of basalt columns is to be seen at the little North Island (Fig. 2) which consists of little else than columns, though many of them are now thrown down. Some of the more compact of the lavas present a more or less picturesque outline, as can be seen at Hutchinson Point (Fig. 3), towards the south-east of the island. Their endurable and adhesive glissades could alone furnish such needle-shaped projections as would be capable of resisting the extreme and never-ceasing violence of the seas that beat on them. Along with the basaltic lavas, there will be found here and there on their upper surfaces little cones of scoriæ thrown up from little supplementary volcanoes; sometimes these will be found here and there quite isolated, at other times they will be found forming a ring as it were around the principal crater. They form a record of the fact that long after the great original outburst that formed this island there were numerous smaller eruptions, and that the source of volcanic power endured for a considerable time.

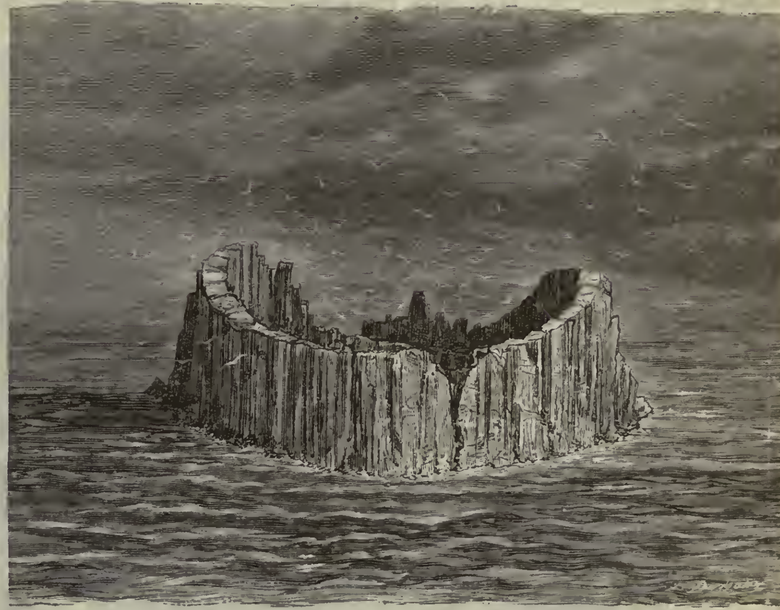

FIG. 2.—North Island.

Although at the period of Lord Macartney's visit (1793), Dr. Gillian remarks that there were spots on the island too warm to walk on, yet there is not a trace of recent volcanic action to be now felt or seen, except in the interior of the crater. M. Vélain informs us that the botanical collections made will be fully described by Dr. de l'Isle (from fifty to fifty-five species, not counting algæ, were found), and that the rich and large collections of marine animals, including fish, Crustacea, Echinoderms, Ascidians, Hydro-

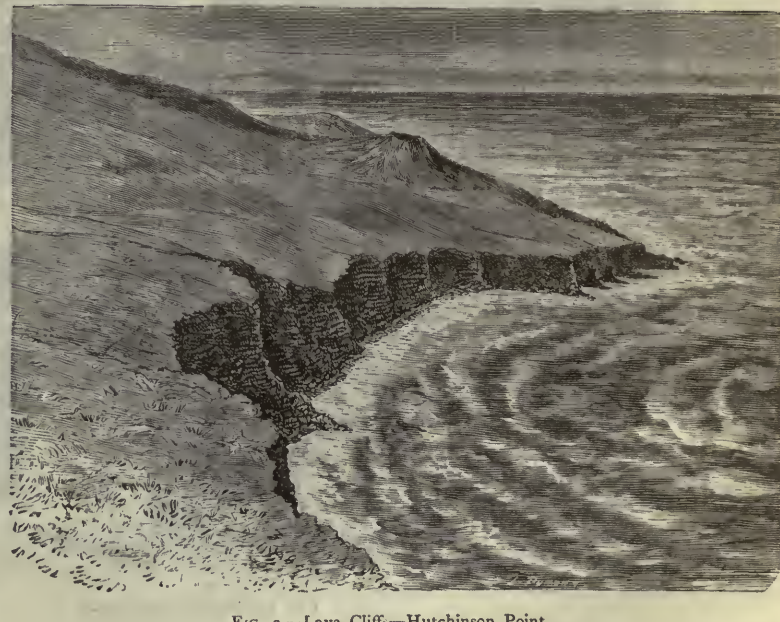

FIG. 3.—Lava Cliffs—Hutchinson Point.

zoa, and Alcyonaria, will be described under the superintendence of Prof. Lacaze Duthiers.

In this present memoir M. Vélain himself gives some most graphic descriptions of the birds that were met with. The little swift seen by Dr. Scherzer, of the *Novara*, did not turn up, nor were any land birds met with, but the aquatic birds abounded in immense numbers. Among these were the following :—*Diomedea exulans*, *D. fuliginosa*, *D. melanophrys*, *D. chlororhyncha*, *Ossifraga gigantea*, *Procellaria capensis*, *P. cinerea*, *P. hæsitata*, *Puffinus æquinoctialis*, *Stercorarius antarcticus*, *Prion vittatus*, *Sterna melanoptera*, and last, but by no means

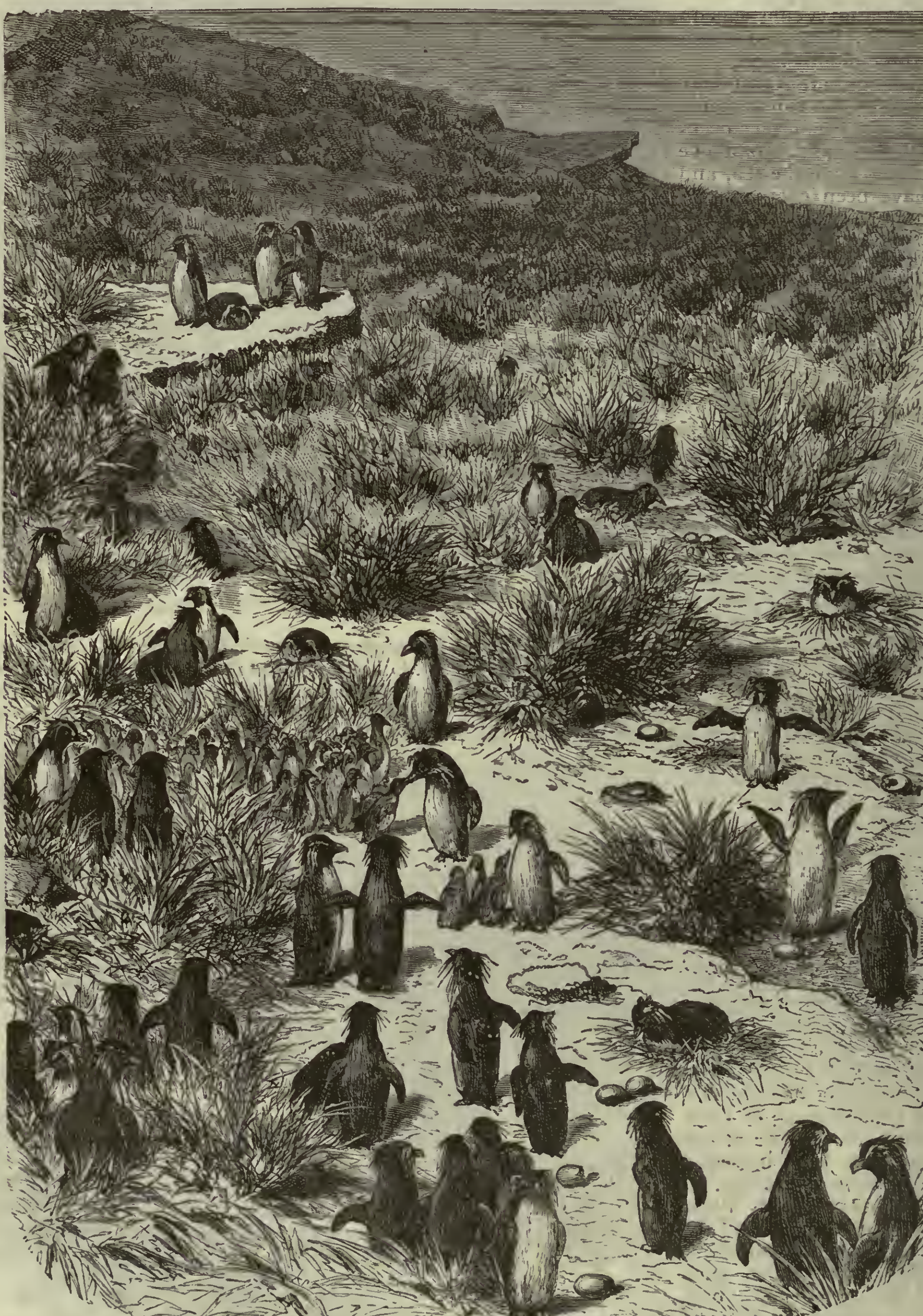

FIG. 4.—Penguins and Young.

the less important, *Eudyptes chrysolopha*. The history of these last birds, though often told, is ever strange, and seems always new. In the month of September these penguins began to lay ; there were two colonies of them, the larger of which contained millions of the birds ; the ground seemed alive with them. But it would be impossible, in a few words, to tell the reader all that M. Vélain has here written about their village life and their infant-schools ; about their wonderful powers of diving ; nor do we wonder that he looks back with no regret to the pleasant

hours he spent in their midst, and we quite agree with him that such intelligent birds can't merit that nasty English word "stupid."

The accompanying illustration (Fig. 4) will give some slight notion of a nesting station of these interesting birds. At the time of the arrival of the expedition (October) the birds were preparing to hatch; each pair kept entirely to themselves; each nest had two eggs, large, nearly round, of a dirty white colour, but marked here and there with a few russet spots. Both birds partook of the cares attendant on the incubation, and took turn about on the nest. The bird off duty would at once make for the sea, faithfully returning at the appointed time, and never failing to waddle direct to its own nest, though no human being could see a difference between the thousands that were strewn about. Sometimes the whole camp of birds would have to be traversed ere the nest sought for would be gained, and a bird trying to make a short cut would be sure to be attacked by those whom it disturbed, for they are not at all tolerant of one another, and in this they also prove that they are not stupid, for surely neither stupid people nor stupid birds ever quarrel. On M. Vélain arriving in their midst, they would one and all set up an immense, and beyond measure stunning cry, but soon they would calm down, and seem not to mind his presence. The incubation lasted for five weeks. The little ones made their appearance covered all over with a fine close down, and looked like balls of fine grey-coloured wool. They soon got tired of the comforts of their nests, and began to assemble together with their little brothers and sisters of the same colony in large infant schools, which are presided over by some of the sedate old birds. Many times a day, at stated intervals, they are fed, the other portions they spend in sleeping and talking, and a little fighting. Space will not permit us to refer to many curious details about their swimming lessons.

M. Vélain's description of the molluscs of Saint Paul is an important contribution to science; the new species are well illustrated on four plates. As was to be expected, there are forty species of Gasteropods to but nine of Acephala, and there is but a single Brachiopod; no land-shells seem to have been found. The cuttle-fish taken are not enumerated, but one gigantic ten-armed species was often alluded to by the fishermen, and at last, as if to prove their assertions true, one morning, after a great storm, a specimen thereof was thrown ashore, and fortunately was at once photographed; unfortunately only its head, arms, and pen could be preserved. The generic name of Mouchezis (after the commander of the expedition) has been proposed for it. Probably it comes near to Steenstrup's Architeuthis, which it resembles in size, by its having circular-shaped suckers, which were ornamented by a row of fine horny denticulations and by their arrangement on the arms, but from which it differs by the singularly shortened form of the short arms, which presented quite the appearance of having been abruptly truncated instead of running out to a more or less tapering point as in most cephalopods; and then the inferior termination of the dorsal ossicle is quite unlike that described by Steenstrup in his genus. *Mouchesia Sancti-Pauli* measured from the tip of its longest arms to the end of the body, upwards of twenty-two feet. A species of Ommastrephes swarmed in the adjacent sea and seemed to be the chief food of the penguins.

E. PERCEVAL WRIGHT

NOTES

SYSTEMATIC botany has lost one of its greatest living names in the death of Elias Magnus Fries, Emeritus Professor of Botany in the University of Upsala. He was born August 15, 1794, and died on February 8 inst. His very numerous works, especially on fungi and lichens, give him a position as regards those groups of plants only comparable to that of Linnæus. His services to science were recognised by the Royal Society in his election as a foreign member in 1875.

THE funeral service of M. Claude Bernard took place at Paris, at the public expense, on Saturday, February 16, at St. Sulpice, in presence of an immense assembly. The interment took place at Père-la-Chaise. The chief mourners were MM. Bardoux, the Minister of Public Instruction, Dumas and Bertrand, Perpetual Secretaries of the Academy of Sciences, Fizeau, President, Mezières, Chancellor of the Academy of Sciences, M. Paul Bert, who is filling the chair of Claude Bernard at the Jardin des Plantes, and Laboulaye. These gentlemen delivered addresses at the grave, which will be published in the *Comptes Rendus* and official papers.

FROM the last report of Dr. Dohrn, the director, we notice that the zoological station at Naples has developed a most remarkable degree of activity, and is becoming a valuable centre of biological research. By the generosity of the Prussian Government it has been provided with a small steamer, and the uninterrupted expeditions in this vessel have secured to the laboratories an enormous and most varied stock of material for research. Dr. Dohrn has carefully organised a plan for the systematic examination of the entire fauna of this part of the sea, to be accompanied by exhaustive description. The literary portion of the work will consist of elaborate monographs on all the families and species represented in the Gulf of Naples. They will not be prepared by the members of the station only, but it is hoped to procure the assistance of all familiar with this special department, and the contributions can be in English, French, German, or Italian. Two monographs on the Elenophoræ and Balanoglossi will appear during the present year, and arrangements have been made for the speedy preparation of eleven others. These will all be based, in regard to nomenclature and classification, on a work shortly to appear under the title, "Prodromus Faunæ Mediterraneæ," which will contain a complete abstract of the literature on this subject up to the present time. The details of anatomical and embryological investigation will form the leading feature of the whole work.

THE Radicals in the French Chamber cannot be accused of opposition to the claims of science. We notice that in a late session a member of the extreme left proposed an amendment to the budget of instruction, which provided for the appropriation of 30,000 francs for an expedition to California to observe the next transit of Mercury, 40,000 for the continuation of the explorations in Northern Africa, where it has been proposed to admit water from the Mediterranean, and 100,000 to enable the Abbé Debès to make a journey across Africa from Zanzibar to the Congo. As the appropriation was granted, we may hope soon to see the latter portion of it cause the appearance of a new rival of Stanley, for the Abbé has had, like Livingstone, invaluable experiences as a missionary, which will enable him to enter upon the undertaking with great promises of success.

THE Astronomical Section of the French Academy has been summoned by the Minister of Instruction to nominate two candidates for the vacant position of the late M. Leverrier.

IN Parisian scientific circles Prof. Charles Friedel is mentioned as the probable successor to the place in the Chemical Section of the Academy rendered vacant by the death of Victor Regnault.

A NEW Archæological Institution at St. Petersburg was opened on January 27 last. The director and founder of the Institution, M. N. W. Katcholoff, delivered the inaugural address, in which he pointed out the importance of the archæological investigation of the great Russian empire, and the great

support the Institution will offer to students of Russian archæology. He also announced that the Russian Government had permitted the publication of a special organ of the Institution the first part of which would shortly appear, and would contain valuable details dating from the time of Alexander I.

A NEW Society of Ethnography, Archæology, and History is to be founded at the University of Kazan.

THE Annual Archæological Congress of France will take place this year at Mans and Laval, beginning at the former place on May 20 and closing at the latter on May 28.

AN interesting course of lectures has been inaugurated in connection with the new museum of ethnography at Paris, which is well adapted to heighten the value of these extensive collections. Nearly every afternoon is appropriated to a discourse by some well-known *savan* on topics illustrated in the museum. Among the subjects for the remainder of the month we notice "The Industrial Products of Central Asia," by M. de Ujfalvy; "The Ancient Mexicans," by Dr. Hamy; "The Lambaquis of Brazil," by M. Wiener; "Feathers, and their Employment among Savage Tribes," by M. Milne-Edwards; "Peruvian Ceramic," by M. Wiener; "Exploration of the Sahara," by Commander Roudaire; "The Useful Plants of Equatorial America," by M. André, &c. Like most of the lectures in Paris, these are free to the public.

THE works for establishing the monster captive balloon at the Tuileries have begun in the court of the old palace. The Municipal Council of Paris voted the demolition of the ruins at its last sitting. It is proposed by the Corporation that the demolition be completed for the opening of the Paris Exhibition.

NEWS from Berlin states that Signor Martinelli has started from Athens for Olympia in order to take the casts of the sculptures recently excavated, particularly of the Apollo of the western front of the building and of the Hermes of Praxiteles. The exhibition of the Olympian casts at Berlin will be deferred until Signor Martinelli has finished his work. All the other casts are now complete at the Campo Santo, near the Berlin Dome. The second volume of the "Ausgrabungen von Olympia," with thirty-five photographic plates, is in course of publication.

SIR JOHN LUBBOCK's Ancient Monuments' Bill passed the second reading on Tuesday. We hope that it will this session pass successfully through the final stage.

MR. W. ACKROYD writes to us with reference to the mechanism of the ear and the bearing it may have on the structure and use of the telephone. In man the drum is inclined to the axis of the external ear passage at about an angle of 46°, and may be less or more in other animals. Mr. Ackroyd thinks that here we are taught that the best disposition of a membrane designed to receive aërial impulses is that of a less or greater angle to the resonating cavity in which it is placed, the value of this angle probably depending upon the depth and form, &c., of such cavity, points only to be ascertained by experiment. In communicating these ideas the other day to Mr. Wilson, of the Physical Laboratory, South Kensington, he stated that Mr. Newth, of the Chemical Laboratory, had found that his telephone worked best when he spoke into it in a slanting direction. Mr. Ackroyd thinks that telephonists will receive many valuable ideas from the study of the comparative morphology of the external auditory apparatus as Bell did by studying the action of the human tympanic membrane.

WE learn from the Annual Report of the Russian Hydrographical Department, just appeared, that during the year 1876 the officers of the department took soundings in the Baltic Sea and along the Finnish shores for 1,100 miles, in the Gulf of Bothnia for 2,130 miles, in Lake Onega for 870 miles, and in the Black Sea for 2,170 miles.

THE Central Physical Observatory at St. Petersburg has issued its report for 1876, containing meteorological observations made during that year at ninety-eight stations, according to the international regulations. An appendix gives the results of the hourly observations made at Moscow during the last fourteen years.

WE are glad to announce the opening at St. Petersburg of a new hygienic society. It is divided into five sections: Biology; Statistics and Epidemiology; Hygiene of towns, manufactures, and public buildings; Hygiene of schools; and Hygiene of food. Prof. Zdekauer is president of the Society, and among the members are some of the most prominent names in the St. Petersburg University and Academy of Sciences.

SINCE January 5 a new *Allgemeine Technikerzeitung* has been appearing at Leipzig (Schäfer) every week. It is a well-written serial and contains frequent reports of the latest progress of the natural sciences from a practical point of view.

THE German Emperor has presented a most valuable collection of arms and weapons to the Ethnographical Department of the Royal Museum of Berlin. The collection was made by Herr Erdmann, the German Consul at Samarang (Java), and consists of weapons from Java, Sumatra, Borneo, Celebes, Flores, Amboina, and other islands of the great Archipelago.

EARTHQUAKES are reported from the Lower Danube on January 31 at 4.30 A.M. It is also announced that the cities of Lima and Guayaquil, in South America, have suffered terribly from recent shocks.

FOR the first time since 1840 Lisbon has been visited by snow. Besides 1840 the years 1837 and 1839 were characterised by this phenomenon.

IN studying the vibrations of solid bodies, M. Dubois has recently got some interesting effects by use of water mixed with vermilion. If this be put on the branches of a tuning-fork which is vibrated, striæ are produced, the vermilion settling in the grooves of the liquid, and giving a figure. Operating first with tuning-forks, then with sounding-tubes and vibrating-plates, M. Dubois arrived at these two laws:—1. Two sounds produced by different instruments give the same separation of striæ, if these sounds are of the same pitch. 2. Two sounds of different pitch give striæ inversely proportional to the numbers of vibrations of the sounds. In the case of the pipes (which were open), a small band of paper carrying the liquid charged with vermilion was fixed with wax at the open part. The vibration of the air immediately produced striæ. The blast being adapted to give a grave fundamental sound, a certain set of equidistant divisions was produced; then on blowing to sound the octave, these divisions remained, but a second set of intermediate lines appeared.

AT p. 113, vol. xvi. of NATURE we drew attention to the gratuitous distribution of a little pamphlet entitled "Notes for Observations of Injurious Insects." This was issued under the auspices of a few well-known entomologists with a view of obtaining any information, however varied, on the habits of the insects and the conditions of the crops most conducive to their increase. It will be remembered that the late Mr. Andrew Murray took a lively interest in the question of the destruction of the crops by insect pests, and read a paper on the subject before the Society of Arts, so that the returns which have been received in answer to the above-mentioned pamphlet and which are now embodied in the form of a report will be specially interesting to entomologists and valuable to cultivators. It is satisfactory to find that some well-known pests were not so abundant in some districts last year as they were in the preceding year; thus we are told that near Isleworth but little injury was noticed amongst the onions from the fly, *Anthomyia ceparum*, though in 1876 it was very destructive, which indeed was the case generally

in the western suburbs of London, and perhaps also in other parts. Two remedies are recommended for warding off the insects; one by scattering amongst the plants some pulverised gas-lime, and the other by watering with the liquid from pig-sties. The clouded yellow butterfly (*Colias edusa*) was, it seems, "the great appearance of the year," and was first seen near Dumfries early in June, and across the south of England it was generally observable from June till October. The frequent death of the larvæ when feeding on various clovers and trefoils is mentioned as a point of interest relatively to its permanent settlement, as also the great difference in the quantity of the sexes noticed at various stations which may be followed by coincident variety of appearance next year. The report is published by Mr. T. P. Newman, Botolph Lane, Eastcheap, from whom we believe copies may be obtained. Every information on the subject will also be supplied on application to the Rev. T. A. Preston, The Green, Marlborough, Wilts, E. A. Fitch, Esq., Maldon, Essex, or Miss E. A. Ormerod, Dunster Lodge, Spring Grove, Isleworth.

THE St. Petersburg University has addressed a note to the Ministry of Public Instruction requesting that the necessary steps be taken for the preservation of any valuable manuscripts which may be found in the Turkish towns occupied by Russian troops. Valuable manuscripts were preserved in this way from destruction in the War of 1829, and important manuscripts have already been discovered in the mosques of Tirnova.

A SMALL Japanese "blue" book comes to us in the shape of a report by the department of Public Hygiene on some of the mineral waters of the country and the uses to which they may be put. Japan seems to contain a great variety of such waters.

AT the meeting of the Musical Association on February 4 a paper was read by Mr. D. J. Blaikley, "respecting a Point in the Theory of Brass Instruments." The necessary difference in form between such instruments and conical tubes was pointed out, and a new experimental method for determining the positions of the nodal points in tubes, especially applicable to such as are of varying section, was shown. As an instance may be given a conical tube open at both ends and of the pitch C 512 vib. The node is nearer the small than the large end of the tube, and by sinking one end in water and holding a fork of the pitch of the tube over the other, the exact position of the node is shown by the level of the water when the tube is giving its maximum resonance.

THE additions to the Zoological Society's Gardens during the past week include two Macaque Monkeys (*Macacus cynomolgus*) from India, presented by Lieut-Col. Fielden; a Grivet Monkey (*Cercopithecus griseo-viridis*) from North-east Africa, presented by Mr. E. H. Lockley; a Garden's Night Heron (*Nycticorax gardeni*) from South America, presented by Mr. Henry Bottrell; three Chimpanzees (*Troglodytes niger*) from West Africa, deposited; a Black-faced Spider Monkey (*Ateles ater*) from East Peru, a Collared Peccary (*Dicotyles tajaçu*) from South America, a Globose Curassow (*Crax globicera*) from Central America, a Black-footed Penguin (*Spheniscus demersus*) from West Africa, a Hey's Partridge (*Caccabis heyi*) from Arabia, purchased.

ON COMPASS ADJUSTMENT IN IRON SHIPS [1]

I.—*New Form of Marine Azimuth and Steering Compass with Adjuncts for the complete Application of the Astronomer-Royal's Principles of Correction for Iron Ships.*

THIRTY-EIGHT years ago the Astronomer-Royal showed how the errors of the compass, depending on the influence experienced from the iron of the ship, may be perfectly corrected by magnets and soft iron placed in the neighbourhood of the binnacle. Partial applications of his method came into immediate use in merchant steamers, and within the last ten years have become universal not only in the merchant service, but in the navies of this and other countries. The compass and the binnacles before you are designed to thoroughly carry out in practical navigation the Astronomer-Royal's principles. The general drawback to the complete and accurate realisation of plans for carrying out these principles heretofore, has been the great size of the needles in the ordinary compass which renders one important part of the correction, the correction of the quadrantal error for all latitudes by masses of soft iron placed on the two sides of the binnacle, practically unattainable; and which limits, and sometimes partially vitiates, the other chief part of the correction, or that which is performed by means of magnets placed in the neighbourhood of the compass. Five years ago my attention was forced to this subject through my having been called upon by the Royal Society to write a biographical sketch of the late Archibald Smith, with an account of his scientific work on the mariner's compass and ships' magnetism, and I therefore commenced to make trial compasses with much smaller needles than any previously in use; but it was only after three years of very varied trials, in the laboratory and workshop, and at sea, that I succeeded in producing a mariner's compass with the qualities necessary for thoroughly satisfactory working in all weathers and all seas, and in every class of ship, and yet with small enough needles for the perfect application of the Astronomer-Royal's method of correction for iron ships. One result at which I arrived, partly by lengthened trials at sea in my own yacht, and partly by dynamical theory analogous to that of Froude with reference to the rolling of ships, was that steadiness of the compass at sea was to be obtained not by heaviness of needles or of compass-card, or of added weights, but by longness of vibrational period [1] of the compass, however this longness is obtained. Thus, if the addition of weight to the compass-card improves it in respect to steadiness at sea, it is not because of the additional friction on the bearing-point that this improvement is obtained; on the contrary, dulness of the bearing-point, or too much weight upon it, renders the compass less steady at sea, and, at the same time, less decided in showing changes of the ship's head, than it would be were the point perfectly fine and frictionless, supposing for the moment this to be possible. It is by increasing the vibrational period that the addition of weight gives steadiness to the compass; while, on the other hand, the increase of friction on the bearing-point is both injurious in respect to steadiness, and detrimental in blunting it or breaking it down, and boring into the cap, and so producing sluggishness, after a short time of use, at sea. If weight were to be added to produce steadiness, the place to add it would be at the very circumference of the card. My conclusion was that no weight is in any case to be added, beyond that which is necessary for supporting the card; and that, with small enough needles to admit of the complete application of the Astronomer-Royal's principles of correction, the length of period required for steadiness at sea is to be obtained, without sacrificing freedom from frictional error, by giving a large diameter to the compass-card, and by throwing to its outer edge as nearly as possible the whole mass of rigid material which it must have to support it.

In the compass before you (Fig. 1), these qualities are given by supporting the outer edge of a card on a thin rim of aluminium, and its inner parts on thirty-two silk threads or fine copper wires stretched from the rim to a small central boss of aluminium, thirty-two spokes, as it were, of the wheel. The card itself is of thin strong paper, and all the central parts of it are cut away, leaving only enough of it to show conveniently the points and degree-divisions of the compass. The central boss consists of a thin disc of aluminium, with a hole in its centre, which rests on the projecting lip of a small aluminium inverted cup mounted with a sapphire cap, which rests on a fixed iridium point (Figs. 2 and 3).

Eight small needles from $3\frac{1}{4}$ inches to 2 inches long, made of thin steel wire, and weighing in all fifty-four grains, are fixed like the steps of a rope ladder on two parallel silk threads, and slung from the aluminium rim by four silk threads or fine copper wires through eyes in the four ends of the outer pair of needles.

The weight of the central boss, aluminium cup, and sapphire

[1] Report of paper read to the Royal United Service Institution, February 4, by Sir Wm. Thomson, LL.D., F.R.S., P.R.S.E., Professor of Natural Philosophy in the University of Glasgow, and Fellow of St. Peter's College, Cambridge. Revised by the Author. [The Council of the U.S.I. have kindly permitted us to publish Sir W. Thomson's paper in advance, and have granted us the use of the illustrations.—ED.]

[1] The vibrational period, or the period (as it may be called for brevity) of a compass, is the time it takes to perform a complete vibration, to and fro, when deflected horizontally through any angle not exceeding 30° or 40°, and left to itself to vibrate freely.

cap, amounts in all to about five grains. It need not be more for a 24-inch than for a 10-inch compass. For the 10-inch compass the whole weight on the iridium point, including rim, card, silk threads, central boss, and needles, is about 180 grains. The limit to the diameter of the card depends upon the quantity of soft iron that can be introduced without inconvenient cumbrousness on the two sides of the binnacle to correct the quadrantal error. If, as sometimes may be advisable in the case of a pole or masthead compass, it be determined to leave the quadrantal error uncorrected, the diameter of the compass-card may be anything from 12 to 24 inches, according to circumstances. A 24-inch card on the new plan will undoubtedly have less frictional error or "sluggishness" for the same degree of steadiness than any smaller size; but a 12-inch card works well even in very unfavourable circumstances, and it will rarely, if ever, be necessary to choose a larger size unless for convenience to the steersman for seeing the divisions, whether points or degrees. You see hanging over the table, from the roof, one of my 12-inch pole-compasses. Specimens of 15-inch and 24-inch pole-compasses have also been made. The last-mentioned may be looked at with some curiosity, as being probably the largest compass in the world. It will no doubt be properly condemned as too cumbrous for use at sea, even in the largest ship, but there can be no doubt it would work well in a position in which a smaller compass would be caused to oscillate very wildly by the motion of the ship. The period of the new 10-inch compass is in this part of the world about forty seconds, which is more than double the period of the A card of the Admiralty standard compass, and is considerably longer than that of the ordinary 10-inch compass, so much in use in merchant steamers. The new compass ought, therefore, according to theory, to be considerably steadier in a heavy sea than either the Admiralty compass or the ordinary 10-inch compass, and actual experience at sea has thoroughly fulfilled this promise. It has also proved very satisfactory in respect to frictional error; so much so that variations of a steamer's course of less than half a degree are shown instantly and surely, even if the engine be stopped, and the water perfectly smooth.

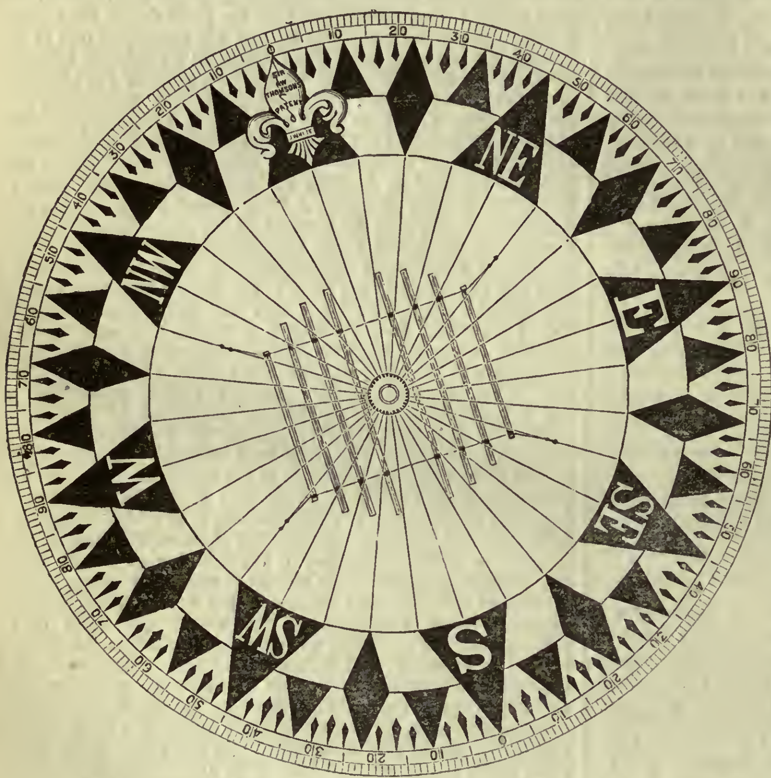

Fig. 1.


Fig. 2.

With the small needles of the new compass, the complete practical application of the Astronomer-Royal's principles of correction is easy and sure: that is to say, correctors can be applied so that the compass shall point correctly on all points, and these correctors can be easily and surely adjusted at sea, from time to time, so as to correct the smallest discoverable error growing up, whether through change of the ship's magnetism, or of the magnetism induced by the earth, according to the changing position of the ship. To correct the quadrantal error I use a pair of solid or hollow iron globes placed on proper supports, attached to the binnacle on two sides of the compass. This mode is preferable to the usual chain boxes, because a continuous globe or spherical shell of iron is more regular in its effect than a heap of chain, and because a considerably less bulk of the continuous iron suffices to correct the same error. When in a first adjustment in a new ship, or in a new position of a compass in an old ship, the quadrantal error has been found from observation, by the ordinary practical methods, it is to be corrected by placing a pair of globes in proper positions according to the following table:—

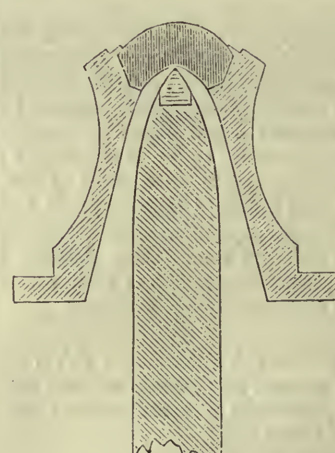
Fig. 3.

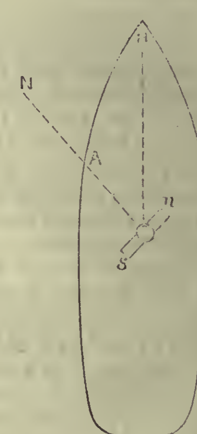

Fig 4.

Table for Correction of Quadrantal Error.

Error to be Corrected.	Distances of the Nearest Points of Globes from Centre of Compass.									
	9-inch globes.	8½-inch globes.	8-inch globes.	7½-inch globes.	7-inch globes.	6½-inch globes.	6-inch globes.	5½-inch globes.	5-inch globes.	4½-inch globes.
°	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.
1	20.52	19.38	18.24	17.10	15.96	14.82	13.68	12.54	11.40	10.26
1½	17.36	16.39	15.42	14.46	13.50	12.54	11.57	10.61	9.65	8.68
2	15.36	14.51	13.66	12.81	11.95	11.10	10.24	9.39	8.53	7.68
2½	13.94	13.16	12.39	11.61	10.84	10.07	9.29	8.52	7.74	6.97
3	12.84	12.13	11.42	10.70	9.99	9.28	8.57	7.85	7.14	5.42
3½	11.98	11.32	10.65	9.99	9.32	8.65	7.99	7.32	6.66	5.99
4	11.26	10.63	10.01	9.39	8.76	8.13	7.51	6.88	6.26	5.63
4½	10.66	10.07	9.47	8.88	8.29	7.70	7.10	6.51	5.92	5.33
5	10.13	9.57	9.01	8.45	7.88	7.32	6.75	6.19	5.63	5.07
5½	9.67	9.13	8.59	8.06	7.52	6.99	6.45	5.91	5.38	4.84
6	9.27	8.75	8.24	7.72	7.21	6.70	6.18	5.66	5.15	4.53
6½	8.91	8.41	7.92	7.42	6.93	6.44	5.94	5.44	4.95	4.46
7	8.58	8.10	7.63	7.15	6.67	6.20	5.72	5.24	4.77	4.29
7½	8.28	7.82	7.36	6.90	6.44	5.98	5.52	5.06	4.60	4.14
8	8.01	7.57	7.12	6.68	6.23	5.79	5.34	4.90	4.45	4.01
8½	7.76	7.33	6.90	6.47	6.04	5.60	5.17	4.74	4.31	3.88
9	7.53	7.11	6.69	6.27	5.86	5.41	5.02	4.60	4.18	3.76
9½	7.32	6.91	6.50	6.09	5.69	5.28	4.87	4.47	4.06	3.66
10	7.11	6.72	6.32	5.93	5.53	5.14	4.74	4.35	3.95	3.55
10½	6.93	6.54	6.16	5.77	5.39	5.00	4.62	4.23	3.85	3.46
11	6.75	6.37	6.00	5.62	5.25	4.87	4.50	4.12	3.75	3.37
11½	6.58	6.22	5.85	5.49	5.12	4.76	4.39	4.02	3.66	3.29
12	6.43	6.07	5.71	5.36	5.00	4.64	4.29	3.93	3.57	3.22

When the quadrantal error has been thus once accurately corrected, the correction is perfect to whatever part of the world the ship may go, and requires no adjustment at any subsequent time, except in the case of some change in the ship's iron, or of iron cargo or ballast sufficiently near the compass to introduce a sensible change in the quadrantal error. The vast simplification of the deviations of the compass effected by a perfect correction of this part of the whole error has not, as yet, been practically appreciated, because, in point of fact, this correction had rarely, if ever, in practice, been successfully made for all latitudes. The pair of large needles of the compass ordinarily used in merchant ships does not, as has been shown by Capt. Evans and Archibald Smith, admit of the correction of the quadrantal error in the usual manner, without the introduction of a still more pernicious error, depending on the nearness of the ends of the needles to the masses of chain, or of soft iron of whatever kind, applied on the two sides of the compass to produce the correction. The Admiralty standard compass, with its four needles proportioned and placed according to Archibald Smith's rule, is comparatively free from this fault: but even with it, and still more with the stronger magnets of the larger compasses of merchant ships, there is another serious cause of failure depending on the magnetism induced in the iron correctors by the compass needles, in consequence of which, if the quadrantal error is accurately corrected in one latitude, it will be found over-corrected in high magnetic latitudes, and under-corrected in the neighbourhood of the magnetic equator. The new compass was specially designed to avoid both these causes of failure in the correction of the quadrantal error; and experiment has shown that with it the correction by such moderate masses of iron as those indicated in the preceding table, is practically perfect not only in the place of adjustment, but in all latitudes.

When once the quadrantal error has been accurately corrected, the complete application of the Astronomer-Royal's principles becomes easy and sure, if the binnacle is provided with proper appliances for readjusting the magnetic correctors from time to time, whether at sea or in port. But the system of nailing magnets to the deck, in almost universal use in the merchant service, is not satisfactory, and is often even dangerous. It always renders needlessly tedious and cumbrous the process of readjustment by the adjuster in port, and it leaves the captain of the ship practically no other method of readjustment at sea than removing the magnets altogether, or taking them out of their cases and replacing them in inverted positions. The Astronomer-Royal himself pointed out that his correcting magnets should be mounted in such a manner that their distances from the compass can be gradually changed, so as always to balance the ship's magnetic force as it alters, whether by gradual loss of her original magnetism through lapse of time, or by the inductive influence of the earth's vertical magnetic force coming to zero, and then becoming reversed in direction when a ship makes a voyage from the northern to the southern hemisphere. The not carrying out of this essential part of his plan, whether through no method or no sufficiently convenient method of adjustment having been hitherto provided, has undoubtedly taken away much of the credit among many practical men to which the Astronomer-Royal's method is justly entitled. I have, therefore, introduced into the binnacles provided for my compass, when it is to be used in iron ships, a complete system of adjustable correctors for perfectly correcting the error of the compass for every position of the ship's head when she is on even keel, and a vertical adjustable magnet below the compass, for correcting the heeling error (more properly speaking, a magnet, which is vertical when the ship is on even keel, and which shares the inclination of the ship when she heels over to either side).

An objection which has often been made to the use of correctors at all, and particularly to the use of correctors for a standard compass, is that they conceal the actual state of the ship's magnetism, and that readjustment of the correctors at sea leaves the navigator without means of judging, when he returns from a foreign voyage, how much of the changed error found on readjustment in port depends on changes which have been made in the correcting magnets, and how much on changes of the ship's own magnetism. This objection I meet by providing that at any moment my correctors can be removed or set to any degrees of power to which they may have been set at any time in the course of the voyage, and again reset to their last position with perfect accuracy. The appliances for changing the adjustment are under lock and key, so that they can never be altered, except by the captain or some properly authorised officer. Farther, to facilitate the use of the correctors, I graduate the scales accurately to correspond to definite variations of the force which they produce on the compass. Thus, as soon as the error has been determined by the known method of observation at sea, the corrector may at once be altered to the proper degree to correct it. Of course the officer performing the adjustment will satisfy himself of its correctness by a second observation. The objection of "delicacy of manipulation," and the difficulty of carrying it out, except by a professional adjuster, of which so much has been said, is entirely done away with when adjustable correctors, with scales thus accurately graduated, are provided with the binnacle.

The binnacles before you are of two kinds adapted to the two

different methods given by the Astronomer-Royal for correcting the semi-circular part of the error; one, the square one, for correcting, by two sets of magnets, fore-and-aft and thwart-ship respectively; the other, the round one, for correcting by a single magnet, or group of bars equivalent to a single magnet, placed under the centre of the compass with its magnetic axis in the proper direction to balance the whole disturbing force on the compass due to that part of the ship's magnetism which is unchanged when she is put on different courses in the same magnetic latitude. The two sets of instructions, in the two printed pamphlets before you, explain sufficiently, for the two binnacles, the arrangements of the magnetic correctors in the two cases, and how to use them in practice.

The principle in each case is easily understood. In the system employed in the square binnacle the whole constant force, due to the part of the ship's magnetism which remains constant when the ship is put on different courses, is regarded as being replaced by three constant "component" forces in the direction of three lines, at right angles to one another—one fore-and-aft, one thwart-ship, and the third perpendicular to the deck. The fore-and-aft component is balanced by the fore-and-aft correcting magnets, the thwart-ship component by the thwart-ship magnets, and the component perpendicular to the deck by the heeling corrector, which is a bar-magnet, adjustable to the proper height, in a line perpendicular to the deck, through the centre of the compass and of the binnacle.

In the round binnacle the component perpendicular to the deck is balanced by a heeling corrector, just as in the square one; but, instead of considering separately two components parallel to the deck, their resultant or the single component parallel to the deck, which, with the component perpendicular to the deck, constitutes the whole force, is balanced by a single magnetic force parallel to the deck. This force is obtained by turning the revolving corrector round the central axis of the binnacle, and raising it or lowering it until the proper direction and proper magnitude of force are produced.

One novel feature in the last binnacle is the way in which, by aid of the guide-ring graduated to logarithmic cosecants, and the vertical scale graduated to equal proportionate differences of force, the adjustment to correct the compass on one course may be performed without disturbing its accuracy on another course on which it has been previously adjusted. The principle of this arrangement is most easily explained by aid of the mathematical notation of trigonometry, in connection with the annexed diagram (Fig. 4), in which O represents the compass-card, A, a point of the ship which was in the direction of the correct magnetic north, N, at the time of the first supposed adjustment, ns, the position of the axis of the revolving corrector set to correct the compass on that course, H the ship's head. We have (according to the notation of the instructions)—

$$\text{HOA} = \text{H},$$
$$n\text{OH} = \text{C};$$
$$\text{therefore,} \quad n\text{OA} = \text{H} + \text{C}.$$

Now the correction on the first supposed course, if it did not annul the force due to the magnetism of the ship and correctors, reduced it to a force in the line OA. Hence the component perpendicular to OA due to the corrector must be kept unchanged in subsequent correction, so as not to disturb the adjustment for that first course. Let F be the magnitude of the force due to the revolving corrector. Its direction being On, its component perpendicular to OA is equal to F sin nOA. Hence, if F be increased by raising, or diminished by lowering, the corrector, the angle nOA must be altered so that sin nOA shall vary inversely as F, or cosec nOA directly as F. In other words, $\frac{\text{F}}{\text{cosec } n\text{OA}}$ must be kept constant, and, therefore, the difference between log F and log cosec nOA must be kept constant. When the guide-ring is placed according to Rule 2, Section 4, of the Instructions, the reading upon it is the value of log-cosec (H + C). The reading on the vertical scale is always proportional to the logarithm of F. Hence Rule 3 secures that the change of magnitude and direction of the correcting force does not vitiate the correction on the course H.

(*To be continued.*)

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—An examination for the Burdett-Coutts Scholarship will be held in the University Museum, on Monday, March 11, and three following days, at 10 A.M., for the purpose of electing a scholar on that foundation. Candidates are requested to call on the Professor of Geology at 34, Broad Street, with certificates of their standing, and the consent of the head or vicegerent of their College or Hall, on Friday, March 8, between 4 and 5 P.M.

CAMBRIDGE.—The exhibition offered by the Clothworkers' Company, to non-collegiate students of the University, for proficiency in physical science, has been awarded to J. G. M'Cubbin, who was educated at the Manchester Grammar School. The exhibition is of the annual value of 50*l.*, and is tenable for three years. The next examination for a similar exhibition open to non-collegiate students who have not resided more than one term, or who have not commenced residence, will be held on July 15 and 17, in connection with the examination conducted by the Oxford and Cambridge Schools Examination Board. Intending candidates can obtain full information on application to the Rev. R. B. Somerset, censor of non-collegiate students, Cambridge.

GILCHRIST EDUCATIONAL TRUST.—A course of six Gilchrist Science Lectures for the People, will be delivered in the Bristol Athenæum, by members of the Council and Staff of University College, Bristol, as follows:—February 22, The Action of Heat, by S. P. Thompson, B.Sc., B.A.; March 5, Heat and the Steam Engine, by J. F. Main, B.A. Camb., D.Sc. Lond; March 12, The Ocean a Carrier of Heat, by W. L. Carpenter, B.A., B.Sc.; March 19, Heat within the Safety Lamp, by S. P. Thompson, B.Sc., B.A.; March 26, the Sun's Heat, by J. F. Main, B.A. Camb., D.Sc. Lond.; April 2, the Chemistry of Burning, by W. W. J. Nicol, M.A. The same course is to be given at Bath, Bridgwater, Trowbridge, and Newport (Monmouthshire).

THE BIRKBECK INSTITUTION.—The Lord Mayor has promised to preside at a meeting, to be held at the Mansion House on Wednesday afternoon, the 27th inst., at three o'clock, for the purpose of inaugurating a fund to provide the Birkbeck Institution with a building suitable to its large and important operations, and to enable it to take advantage of the many opportunities for further usefulness which are from time to time presented. As the Institution is doing such an important educational work amongst the young men and women of the metropolis, it is hoped that the friends of education will liberally assist the movement to accomplish so desirable an object. The number of students has been steadily increasing for some years past, and, notwithstanding alterations and extensions of the building, it is impossible any longer to accommodate those attending the Institution. Some indication of the work will be gained from the fact that 3,304 persons joined the Institution during the past term.

ST. PETERSBURG.—The professors of the High School of Medicine for Ladies at St. Petersburg, among whom are many names well known in science, have addressed a petition to the Minister of Public Instruction, in which they claim for ladies who have completed their studies at the high schools, the same degrees as for men. They support their request by pointing out that the five years' theoretical and practical study at the ladies' school are quite as extensive as those pursued by male students, and rather more extensive in the department of female diseases; that the monthly and yearly examinations have always proved that the ladies possess a very thorough knowledge of their subjects, and finally, that during their service with the army in Roumania and Bulgaria, the ladies have given numerous and sufficient proofs of their high capacity for acting as surgeons.

FREIBURG.—The university is attended at present by 334 students, including 41 in the theological faculty, 70 in the philosophical, 76 in the legal, and 147 in the medical. It possesses a library of 300,000 volumes, and well-equipped scientific laboratories and collections, but fails of late years to rank among the influential German universities, partly on account of the rivalry of its neighbours, Tübingen, Heidelberg, and Strasburg.

WÜRZBURG.—The corps of instructors numbers at present 40 ordinary professors, 5 extraordinary professors, and 17 privat docenten. The number of students, 947, shows a decrease of about 50 on the past half year. On January 2 the 296th anniversary of the foundation of the university was celebrated, and an address delivered by the rector, Prof. Risch, on the national importance of the German universities and their relations to the empire. In the course of the address the Imperial Government was sharply criticised for having, with the exception of the

ample provisions for Strasburg, utterly neglected the university system of the country, and failed to introduce the uniformity of management and many other reforms, the need of which is painfully felt since the formation of a united Germany. Prof. Sachs, the well-known botanist, has been raised into the nobility, possibly in recognition of his refusal of a flattering call to Berlin.

HANOVER AND AIX-LA-CHAPELLE.—The two large polytechnics in these cities show a striking diminution in point of attendance during the past year, a fact which would seem to show that the various technical branches in Germany are being overcrowded. Hanover is attended at present by 725 students and the instructors number 46. Aix-la-Chapelle has suffered a reduction of 200 students in comparison with 1876.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, January 17.—"New Determination of the Mechanical Equivalent of Heat," by J. P. Joule, LL.D., F.R.S.

An account is given by the author, of the experiments he has recently made, with a view to increase the accuracy of the results given in his former paper, published in the *Philosophical Transactions* for 1850. The result he has now arrived at, from the thermal effects of the friction of water, is, that taking the unit of heat as that which can raise a pound of water, weighed *in vacuo*, from 60° to 61° of the mercurial thermometer; its mechanical equivalent, reduced to the sea-level at the latitude of Greenwich, is 772·55 foot-pounds.

February 7.—"On the Comparison of the Standard Barometers of the Royal Observatory, Greenwich, and the Kew Observatory," by G. M. Whipple, B.Sc., F.R.A.S., Superintendent of the Kew Observatory.

Owing to certain statements having been circulated as to a large difference existing between the standard barometers of the above two chief meteorological establishments in this country, the Kew Committee decided to institute a direct comparison between them, by the conveyance of a number of instruments to and fro, several times between the two observatories.

The author accordingly did this, having made three extended experiments of this nature, details of which are given in the paper, the results being as follows:—

Mean difference from—

1st series of	128	comparisons—Greenwich-Kew	= + 0·0016 inch.
2nd „	144	„ „ „	= + 0·0007 „
3rd „	72	„ „ „	= + 0·0014 „
Final mean of	344	„ „ „	= + 0·0012 „

Certain experiments were also made to determine the necessary corrections to be applied to the Greenwich barometer on account of inequality of distribution of temperature around it. When these corrections are applied the difference between the two standards is reduced to 0·0001 inch, that is to say, the two instruments virtually agree.

In conclusion the author tenders his thanks to the Astronomer-Royal, for the facilities he afforded for the prosecution of the experiments, and to Messrs. Ellis and Nash for assistance they rendered.

Geological Society, January 23.—Prof. P. Martin Duncan, F.R.S., president, in the chair.—J. Eunson, C.E., R. C. Forster, Walter Mawer, Richard H. Solly, and the Rev. Arthur Watts, were elected Fellows of the Society. The following communication was read:—On the secondary rocks of Scotland.—Part III. The strata of the Western Coast and Islands, by John W. Judd, F.R.S., F.G.S., Professor of Geology in the Royal School of Mines. The existence of scattered patches of fossiliferous strata, lying between the old gneissic rocks and the masses of tertiary lava in the Hebrides, has been known to geologists for more than a century. By Dr. Macculloch, who did so much for the elucidation of the interesting district in which they occur, these strata were referred to the lias; but Sir Roderick Murchison showed that several members of the oolitic series were also represented among them. Later researches have added much to our knowledge of the more accessible of these isolated patches of Jurassic rocks in the Western Highlands. During the seven years in which he has been engaged in the study of these interesting deposits, the author of the present memoir has been able to prove that not only is the Jurassic system very completely represented in the Western Highlands, but that associated with it are other deposits representing the carboniferous, poikilitic (permian and trias), and cretaceous deposits, the existence of which in this area had not hitherto been suspected; and by piecing together all the fragments of evidence, he is enabled to show that they belong to a great series of formations, of which the total maximum thickness could have been little, if anything, short of a mile. The relations of the scattered patches of mesozoic strata to the older and newer formations respectively, are of the most interesting and often startling character. Sometimes the secondary rocks are found to have been let down by faults, which have placed them thousands of feet below their original situations in the midst of more ancient masses of much harder character. More usually they are found to be buried under many hundreds, or even thousands, of feet of tertiary lavas, or are seen to have been caught up and inclosed between great intrusive rock-masses belonging to the same period as the superincumbent volcanic rocks. Occasionally the only evidence which can be obtained concerning them is derived from fragments originally torn from the sides of tertiary volcanic vents, and now found buried in the ruined cinder-cones which mark the sites of those vents. In some cases the mineral characters of the strata have been greatly altered, while their fossils have been occasionally wholly obliterated by the action of these same igneous forces during tertiary times. In every case the survival to the present day of the patches of secondary rocks can be shown to be due to a combination of most remarkable accidents; and a study of the distribution of the fragments shows that the formations to which they belong originally covered an area having a length of 120 miles from north to south, and a breadth of fifty miles from east to west. But it is impossible to doubt the former continuity of these secondary deposits of the Hebrides with those of Sutherland to the north-east, with those of Antrim to the south, and with those of England to the south-east. From the present positions of the isolated fragments of the mesozoic rocks, and after a careful study of the causes to which they have owed their escape from total removal by denudation, the author concludes that the greater portion of the British Islands must have once been covered with thousands of feet of secondary deposits. Hence it appears that an enormous amount of denudation has gone on in the Highlands during tertiary times, and that the present features of the area must have been, speaking geologically, of comparatively recent production—most of them, indeed, appearing to be referable to the pliocene epoch. The alternation of estuarine with marine conditions, which had, on a former occasion, been proved to constitute so marked a feature in the Jurassic deposits of the Eastern Highlands is now shown to be almost equally striking in the Western area; and it is moreover pointed out that the same evidence of the proximity of an old shore-line is exhibited by the series of cretaceous strata in the west. The succession and relations to one another of the series of deposits, now described as occurring in the Western Highlands, is given in the following table:—

Miocene Volcanic and Intervolcanic Rocks.

UNCONFORMITY.

		Maximum thicknesses. feet.
Cretaceous.	1. Estuarine clays and sands with coal	20 +
	2. White chalk with flints (zone of *Belemnitella mucronata*)	10 +
	3. Estuarine sandstones with coal	100
	4. Upper greensand beds	60

UNCONFORMITY.

Jurassic.	5. Oxford clay	?
	6. Great estuarine series	1000
	7. Lower oolite	400
	8. Upper lias	100
	9. Middle lias	500
	10. Lower lias	400
	11. Infralias	200
	12. Poikilitic	1000 +

UNCONFORMITY?

Carboniferous strata (coal-measures).

UNCONFORMITY.

Old Gneiss Series and Torridon Sandstones.

Although no traces of the upper oolite or the neocomian formations have as yet been detected in the Western Highlands, yet it is argued that when we consider how enormous has been the amount of denudation, and how singular the accidents to which all the existing relics of the secondary period have owed their escape from total destruction, we cannot but regard it as a most rash and unwarrantable inference to conclude that no deposits belonging to those periods were ever accumulated within the district under consideration. The carboniferous strata of the Western Highlands have been detected at but a single locality; and even there, being exposed in a series of shore reefs that are only occasionally well displayed, can only be studied under favourable conditions of tide and wind. They consist of sandstones and shales with thin coaly seams, and their age is placed beyond question by the discovery in them of many well-known plants of the coal-measures, including species of *Lepidodendron*, *Calamites*, *Sigillaria*, and *Stigmaria*. The poikilitic strata consist of conglomerates and breccias at the base, graduating upwards into red marls and variegated sandstone, which contain concretionary limestones and occasional bands of gypsum. These strata have not as yet, like their equivalents in the Eastern Highlands (the reptiliferous sandstone of Elgin and the Stotfield rock) yielded any vertebrate remains. They were evidently deposited under similar conditions with the beds of the same age in England, and are not improbably of lacustrine origin. The Jurassic series presents many features of very great interest. The infralias is better developed than is perhaps the case in any part of the British Islands; and in the district of Applecross a series of estuarine beds, containing thin coal-seams, is found to be intercalated with the marine strata. The lower lias, in its southern exposures, presents the most striking agreement with the equivalent strata in England, but when traced northwards exhibits evidence of having been deposited under more littoral conditions; the lower division (lias α, Quenstedt) is represented by a great thickness of strata; while the upper (lias β) is absent or rudimentary. The middle lias is grandly developed, and consists of a lower argillaceous member and an upper arenaceous one, the united thickness of which is not less than 500 feet. The upper lias singularly resembles in the succession of its beds, and its palæontological characters, the same formation in England. The inferior oolite is formed by series of strata varying greatly in character within short distances, and betraying sufficient signs of having been accumulated under shallow-water conditions. Above the inferior oolite we find a grand series of estuarine strata, partly arenaceous and partly calcareo-argillaceous; and this is in turn covered conformably by an unknown thickness of blue clays with marine fossils of middle Oxfordian age. At the very lowest estimate, the Jurassic series of the Western Highlands could not have had a thickness of less than 3,000 feet! The cretaceous strata of the Western Highlands, though of no great thickness, are of surpassing interest. They consist of two marine series alternating with two others of estuarine origin. At the base we find marine deposits of upper greensand age, strikingly similar to those of Antrim, but in places passing into conglomerates along old shore lines. Above the upper greensand beds occur unfossiliferous sandstones, in which thin coal-seams have been detected, and these are in turn covered by strata of chalk, converted into a siliceous rock, but still retaining in its casts of fossils (*Belemnitella*, *Inoceramus*, *Spondylus*, &c.), and in its beautifully preserved microscopic organisms (*Foraminifera*, *Xanthidia*, &c.) unmistakable proofs of its age and the conditions of its deposition. Above this representative of the highest member of the English chalk there occur argillaceous strata with coal-seams and plant-remains which are perhaps the equivalent of younger members of the cretaceous series, not elsewhere found in our islands, or, it may be, they must be regarded as belonging to periods intermediate between the cretaceous and tertiary epochs. It is greatly to be regretted that these cretaceous deposits of the Western Highlands are so unfavourably displayed for our study as to present scarcely any facilities for the collection of their fossils; for these, if found, might be expected to throw a flood of light on some of the most obscure palæontological problems of the present day. Although the comparison and correlation of the secondary strata of the Highlands with those of other areas, and the discussion of the questions of ancient physical geography thereby suggested, are reserved for the fourth and concluding part of his memoir, the author takes the opportunity of making reference, in bringing the present section of his work to a close, to several problems on which the phenomena now described appear to throw important light. In opposition to a recent speculation which would bring into actual continuity the present bed of the Atlantic and the old chalk strata of our island, he points to the estuarine strata of the Hebrides as demonstrating the presence of land in that area during the cretaceous epoch. He also remarks on the singular agreement of the conditions of deposition of both the silurian and cretaceous strata of the Scottish Highlands and those of the North American continent. But he more especially insists on the proofs, which we now have, that the Highlands of Scotland, as well as the greater part of the remainder of the British Islands, were once covered by great deposits of secondary strata, and that the area has been subjected to enormous and oft-repeated denudation. He dwells on the evidence of the vast quantities of material which have been removed subsequently to the mesozoic and even to the miocene period, and he maintains the conclusion that many, if not all, of the great surface-features of the highlands must have been produced during the very latest division of the tertiary epoch, namely the pliocene.

Mathematical Society, February 14.—Lord Rayleigh, F.R.S., president, and subsequently Mr. C. W. Merrifield, F.R.S. vice-president, in the chair.—The following communications were made:—On a general method of solving partial differential equations, Prof. Lloyd Tanner.—On the conditions for steady motion of a fluid, Prof. Lamb (Adelaide), (particular cases of the conditions were given by Stokes in the Cambridge *Phil. Trans.* for 1842).—On a property of a four-piece linkage and on a curious locus in linkages, Mr. A. B. Kempe.—On Robert Flower's new mode of computing logarithms (1771), Mr. S. M. Drach.—On the Pluckerian characteristics of the modular equations, Prof. H. J. S. Smith, vice-president, F.R.S.—Mr. Drach also exhibited drawings of tricircloids made some thirty years since for Mr. Perigal.

Royal Microscopical Society, February 6.—Anniversary.—H. C. Sorby, president, in the chair.—The report of the Treasurer was submitted to the meeting.—The report of the Council stated that the library and instruments of the Society were in a satisfactory condition, and obituary notices of deceased Fellows, Dr. Bowerbank and Dr. Henry Lawson, were read by the Secretary. Messrs. Glaisher and Curties having been appointed scrutineers, a ballot for officers and council for the ensuing year took place with the following result:—President, H. J. Slack; Vice-presidents, Dr. L. S. Beale, Dr. C. T. Hudson, Sir John Lubbock, Bart., and Mr. H. C. Sorby; Treasurer, Mr. J. W. Stephenson; Secretaries, Mr. Chas. Stewart and Mr. Frank Crisp; Council, Mr. John Badcock, Mr. W. A. Bevington, Dr. R. Braithwaite, Mr. Chas. Brooke, Mr. C. J. Fox, Dr. W. J. Gray, Mr. E. W. Jones, Dr. Matthews, Mr. S. J. McIntire, Dr. John Millar, Mr. Thos. Palmer, and Mr. F. H. Ward; Assistant-secretary, Mr. Walter W. Reeves. The retiring president then delivered his annual address, which chiefly treated of the results of his investigations into a method of obtaining the refractive indices of minerals.

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THURSDAY, FEBRUARY 28, 1878

SNAKE POISON

EVERY now and again the British public is horrified by accounts of the famines which periodically carry off myriads of our fellow-subjects in India, but comparatively few have the least idea of the enormous destruction of human life which occurs there from the ravages of wild animals and venomous snakes. In a most interesting lecture recently delivered at a meeting of the Society of Arts by Sir Joseph Fayrer, the lecturer estimated the loss of life at no less than 20,000 human beings and 50,000 head of cattle annually. Wild animals destroy most of the cattle, but venomous snakes kill more human beings than all the wild animals put together. The bites of these reptiles caused the death of 17,000 persons, and over 3,000 cattle in the year 1875, and these figures very probably understate the facts, as the returns upon which they are based are incomplete. The desirability of obtaining an antidote to snake poison is thus evident, and many attempts have been already made to discover one. Another has been added to the already numerous investigations on this subject by Mr. Pedler, who has lately published the results of his research in a paper read before the Royal Society. Before proceeding to seek for the antidote, he endeavoured to analyse the poison chemically, and thus discovered several facts of great interest. The venom of snakes seems to contain very much the same proportion of solids at all times, even under such different climatic conditions as during the wet and dry seasons. It may be kept for two or three months without alteration, but if preserved for a year or eighteen months, it becomes insoluble, and, to a great extent, loses its poisonous qualities. Its composition is very like that of albumen, and, indeed, the dried poison, which looks very like gum arabic, contains about sixty per cent. of albumen, and only forty per cent. at most of the poisonous principle. By the use of solvents, Mr. Pedler endeavoured to separate a crystalline principle, such as Lucien Bonaparte affirmed to be present in the poison of the rattlesnake. His attempts were unsuccessful, and he therefore tried to obtain it by dialysing the poison through parchment paper. Part of the poison dialysed, and part did not. On evaporating the fluid inside the dialyser, the residue formed a gummy mass, with a poisonous action. The water outside the dialyser also gave a similar result, but in it a few crystals could be detected. It was, if anything, rather more poisonous than the ordinary virus. He did not succeed, however, in obtaining any very definite crystalline substance. Ammonia, which has lately been highly recommended as an antidote in snake poisoning, he found, as did Fontana two hundred years ago, to be useless, and indeed its addition to the poison before injection seemed really to hasten death.

Some organic poisons may have their physiological action greatly altered by changing their chemical constitution. Thus strychnia has its action completely altered by combination with iodide of methyl, so that instead of producing convulsions, it causes complete paralysis, like curara. At the same time its deadly power is greatly diminished, and it occurred to Mr. Pedler that the poisonous properties of cobra virus might be diminished in a similar way. On testing this supposition, he found it to be correct, as the poison, after digesting with ethylic iodide, took five times as long to kill an animal as fresh cobra poison would have done. Hydrochloric acid also diminished the activity of the virus, and platinum chloride had a still more powerful action. This salt seems to combine with the poisonous principle of cobra virus, forming with it a yellow amorphous precipitate, which is very insoluble in water, and which has little or no poisonous action. This result of the action of platinum chloride on cobra virus out of the body is most satisfactory; but this apparent antidote has not the same power when the poison has once entered the system. When the poison is injected under the skin and the platinum chloride is injected shortly afterwards into the same spot, death appears to occur even more quickly than when no antidote whatever is used, the second injection seeming to drive the poison before it and to cause it to act more rapidly. When the platinum chloride, however, is injected at the same point, but somewhat more deeply than the virus, so that in passing inwards the poison might come in contact with the platinum, life is considerably prolonged. If a short time elapses between the injection of the poison and that of the platinum, death ensues, even though the interval be only one or two minutes. It would thus seem that when the platinum chloride is brought directly into contact with the poisonous principle of the cobra venom it renders it insoluble and prevents its poisonous action, but that it is not a physiological antidote, and will not counteract the deadly action of the virus after it has once entered the circulation. It may be useful as a local application, but cannot be regarded as an antidote. Every means hitherto tried of counteracting the effects of cobra venom has thus proved ineffectual. Artificial respiration, proposed by Sir Joseph Fayrer and Dr. Lauder Brunton, gave fair promise of success, and by its use the heart may be kept beating for many hours. Indeed in one case an animal apparently dead for many hours has been partially revived by it, yet on no occasion has a fatal issue ever been averted by its use. The experiment just mentioned was performed by a commission appointed by the Indian Government, at Sir J. Fayrer's suggestion, to examine into the modes of preventing death from snake bite. A dog was bitten one afternoon by a water snake, and apparently died about three o'clock. Artificial respiration was at once commenced, and the heart continued to beat, but the animal seemed to be perfectly dead, and the limbs no longer responded to electrical stimuli. Early next morning, however, an alteration took place. The limbs again answered to electricity, voluntary movements occurred, and the eyelids closed not only when the eye was touched with the finger, but when the hand was simply brought near it. This showed that the animal could see the approaching hand, and closed its eyes in order to protect them from the expected touch. The dog seemed to be in a fair way to recovery, but about noon it began to get worse, and finally died at three o'clock on the second day, twenty-four hours after its first apparent death. Whether a combination of artificial respiration with

other appliances may yet enable us to prevent death altogether, is a question which can only be determined by a continuance of those experiments which led to the use of artificial respiration alone. But however valuable such a method as this may occasionally be in saving the lives of English officers, government officials, or persons living within reach of skilled assistance, and who might otherwise be doomed to certain death from the bite of a cobra, it is obvious that it is too complicated to be of much service to the numerous natives who are bitten in localities where no other assistance can be had than that of their comrades, equally ignorant with themselves. If any great diminution is to be effected in the frightful mortality annually resulting from the bites of venomous snakes in India, the remedies must either be so simple and easy of application that they can be used by the most ignorant, or the snakes must be destroyed. The best instructions yet given for the treatment of persons bitten by poisonous snakes are contained in Sir Joseph Fayrer's magnificent work on "The Thanatophidia of India." He recommends that a tight ligature be applied to the limb above the bite, that the bitten part be cut out as quickly as possible, and that the wound thus left be cauterised with a hot coal or hot iron, or touched with nitric or carbolic acid, while brandy or ammonia should be administered internally. Even this treatment, simple though it be, requires knowledge, as well as instruments and skill, which the majority of the natives do not possess. Sir Joseph Fayrer therefore recommends that in every police station and public place plain directions should be printed and hung up, and that at all such places a supply of whipcord, a small knife, a cautery iron, and a bottle of carbolic or nitric acid should be kept, as well as a supply of liquor ammonia for internal administration. But, as Sir Joseph Fayrer says, although comparatively little is to be expected even from this rational mode of treatment, much may be anticipated from prevention, and it is to be effected by making known the nature and appearance of the venomous as distinct from the innocent snakes, and by offering rewards (to be judiciously distributed) for the destruction of the former. The differences between many of the non-venomous and the venomous snakes are not known to the natives, and it is important that a knowledge of such distinctions should be widely disseminated, not only that the venomous ones may be more easily recognised, and thus avoided or destroyed, but in order to prevent death or serious illness from sheer fright, which may frequently result from the bite of a non-venomous species. For this purpose it would be well if the pictures of the chief venomous snakes contained in Sir Joseph Fayrer's work, or cheaper but accurate lithographic copies of them, were displayed in every police station and public place throughout India. Rewards should be paid for the destruction of venomous snakes only, and if these pictures were exhibited in the way suggested there would be little or no excuse for any mistake, either on the part of the natives who killed the snakes, or the officers whose duty it would be to pay the reward. As to the amount of reward, and its mode of distribution, there should, he suggests, be a department, or branch of a department, with a responsible chief and subordinate agents, for whom certain rules should be laid down, to be observed steadily and without hindrance throughout the country, leaving much, as to detail, to the discretion of local authorities. If the destruction of venomous snakes and wild animals in India were intrusted to an officer such as controls the Thuggie and Dacoitee department, he considers that the result would in a few years be as good in the case of noxious animals as it has been in that of noxious men, Thugs and Dacoits.

THE BEETLES OF ST. HELENA

Coleoptera Sanctæ-Helenæ. By T. Vernon Wollaston, M.A., F.L.S. 8vo, pp. i.–xxv., 1–256, coloured plate. (London: Van Voorst, 1877.)

THIS, the last of its lamented author's valuable descriptive works on the geographical distribution of beetles (in personally collecting the material for which, it is to be feared that his physical exertions during a weak state of health induced the attack that ended recently in his death), must have been the most satisfactory to him, on account of the complete isolation of its subject, and his discovery of its most striking endemic fauna. The investigation of the *Coleoptera* of the Madeiras, Salvages, Canaries, and Cape-de-Verdes, with which his name will always be associated, had already resulted in a firm opinion that their peculiar beetle-types could not be satisfactorily referred to any geographical area now existing, but rather to some submerged Atlantic region, of which these groups are the modern representatives; and the results of his exhaustive work at St. Helena cannot have failed to materially strengthen this idea. Curiously enough, also, the most dominant type in this island is one to which Wollaston was always specially devoted, viz., the *Cossonidæ*, a little known family of weevils, whereof the inordinately numerous species here found, consisting of variations of some half-dozen forms occasionally developed to so marvellous an extent as to be almost ludicrous, amply justified his expression (*in litt.*) that he had "tumbled on his legs in this little oceanic preserve of the southern Atlantic."

To any one interested in the faunæ of islands, no better conditions could be afforded than those found in St. Helena. Its vast distance from the nearest continents (nearly 1,200 miles from Africa, and 1,800 from South America) and, indeed, from the nearest island (Ascension, 700 miles), added to its complete severance by a fathomless depth at a mile and a half from its present coast-line, are premises of themselves suggesting the probability of abnormal resident forms; and the peculiar and very dense original vegetation of ebony, redwood, boxwood, *Psiadia*, asters, gumwood, cabbage-palms, tree-ferns, &c., would reasonably be expected to foster a development of special wood-feeding types, to the partia or entire exclusion of other groups. This development, anticipated by Wollaston from the eccentric species received in former years, is wonderfully illustrated by an analysis of the present work. In it, 203 species are recorded, and may probably be taken as very nearly exhausting the fauna, since the author captured, mounted, and examined (with a delicacy, precision, and care peculiar to himself) no less than 10,000 specimens. Of the difficulty attending the collection of such a mass in six months, the author affords an indication by his remark

(*Entomologists' Monthly Magazine*, xii. p. 252) that the net may be used over miles of grassy mountain-slopes without finding a single flower-frequenter, or anything approaching to it. Under these conditions, it is not to be wondered at that Mr. Melliss's account of the island, not long ago reviewed in NATURE, should, as not representing the work of an expert, have failed adequately to represent its peculiar coleopterous features. Of the 203 species above mentioned, fifty-seven have undoubtedly been conveyed to the island through various external media, and have since established themselves—many of them, indeed, being the regular followers of civilisation. Seventeen of the remainder possess doubtful claims to be considered indigenous, or even to have been taken in St. Helena at all. Of the 129 species left, and which may be safely deemed endemic, the distribution is highly eccentric. Whole groups, hitherto regarded as well-nigh cosmopolitan, are either entirely absent or barely represented; and one section, the weevils, is most unduly exaggerated, especially in one of its families. The missing divisions are water-beetles (both *Hydradephaga* and *Philhydrida*—the aquatic *Carnivora* and *Herbivora*), and *Longicornia;* and their absence is the more noteworthy, as proper natural conditions exist for both of them; and, as to the latter, other wood-feeders have inordinately increased and multiplied. The *Necrophaga* (a wide term, covering many families of universal distribution, including bone-, skin-, and fungus-feeders, acting as natural scavengers, and whereof we have, even in Great Britain alone, over 450 species) and *Trichopterygia* have each but a single representative. The *Pseudotrimera* (*Coccinellidæ*, &c.) and *Lamellicornia* can each only supply two. As to the former of these groups, Prof. Westwood has well observed that the inference is a want of *Aphides* and other plant-lice, on which lady-birds are the natural parasites; and on this point it would be interesting to know if the usual Homopterous vegetable-feeders are really wanting. If not indigenous they might be readily introduced; and, enumerating even the avowedly introduced *Pseudotrimera* in Mr. Wollaston's list, we find only four species to keep them down, since the *Corylophidæ* and *Erotylidæ* included in the group by the author cannot be reckoned. As to the Lamellicorns, the want of indigenous mammals would readily account for the absence of such of them as feed on the excreta of those animals (*two* only, both introduced, can be found; here Baron von Harold would assuredly perish of inanition!); but the mighty tropical clan, revelling in rotten wood, should surely in such a latitude, with the decaying forests of centuries for pabulum, have reared more than the miserable tale of four, whereof but two are autochthones! Next in number come the *Priocerata* and *Phytophaga*, respectively counting but three. The *Elateridæ* and *Anobiidæ*, essentially wood-feeders, are the only families of the first of these that provide indigenous species: how they have failed to produce more is incomprehensible. The fact of plant-feeding beetles being of the greatest scarcity has been already quoted from the author himself, and is equally unintelligible. The *Staphylinidæ* and *Heteromera* each supply six indigenous forms, the paucity of the latter being perhaps accounted for by the lack of those sandy wastes peculiarly affected by so many of its members. Next in importance come the *Geodephaga*, or land carnivorous beetles, whereof as many as fourteen (in fact all but one, and of them no less than eleven here described as new) are recorded. Here, again, the peculiarity of the island is emphasised, as the eleven new species, all of the genus *Bembidium*, depart widely from the shingle-, mud-, and marsh-frequenting habits of that vast and widely distributed genus, occurring as they do in the high central mountain ridges, and living inside the fibrous stems of rotten tree-ferns, an unexpected habitat as strange as that recorded in the Horatian lines:—

> "Piscium et summa genus hæsit ulmo,
> Nota quæ sedes fuerat columbis."

These arboreal Bembids have necessitated the creation of three new sub-genera, distinguished by abnormally minute eyes, want of wings, rounded outline, fossorial legs, and moniliform antennæ; and would alone have been sufficient to have stamped the fauna as *sui generis.*

Last, and most important, come the *Rhynchophora* or weevils, with no less than ninety-one representatives, more than two-thirds of the whole number. These again are represented in unusual proportions, the *Cossonidæ* numbering fifty-four, two-fifths of the entire fauna (we have in England but nine, out of 3,000 species), and the *Anthribidæ* twenty-six. The conclusion derived by the author is, that, as these weevils unquestionably represent the dominant autochthonous family, and all (but one) are of lignivorous habits, St. Helena may be pictured in the remote past as a densely-wooded island, in which they performed their natural functions of tree-destroyers among tree-ferns and *Compositæ* on a gigantic scale, unaided by the usual timber-eaters. The well-nigh complete destruction of indigenous trees in modern times has no doubt been accompanied by the loss of many a link in the aboriginal chain of these peculiar forms. Those that still survive are of such eccentric structure and facies that the creation of eleven new genera and forty new species has been necessitated for their reception in the present work, which, had it been the sole production of its author, would have effectually prevented his name from passing into oblivion.

E. C. RYE

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Oxygen in the Sun

ATTENTION having recently been directed by Dr. Schuster and Mr. Meldola, in connection with my discovery of oxygen in the sun, to the location of the oxygen, it may be of interest to allude to some experiments to determine the question by direct observation of the image of the sun spectroscopically. For this purpose I used a spectroscope furnished with a very fine grating on silvered glass given to me by Mr. Rutherfurd. This grating of 17,280 lines to the inch can be arranged to give a dispersion equal to twenty heavy flint glass prisms. The spectroscope was attached to my 12-inch Clark refractor, and I employed the full aperture of this telescope to produce an image of the sun on the slit. It did not seem practicable to use the spectroscope on the 28-inch Cassegrain reflector in this research, because the tremulousness of the air was usually too great, the image of the

sun being magnified to five inches in diameter. Even with the 12-inch refractor the occasions suitable for a critical examination are rare.

In front of the slit I also brought the terminals of the secondary circuit of an induction coil, which were connected with a Leyden battery. The current through the primary wire of the Ruhmkorff was furnished by a Gramme machine. This arrangement permitted the production of a strong oxygen spectrum near the spectrum of the sun's limb.

The most obvious point to determine was whether the oxygen lines visible in the spectrum of the solar disc projected beyond the apparent limb of the sun as seen in the spectroscope; in other words, whether oxygen could be detected in the lower parts of the chromosphere. For this purpose I looked particularly at the bases of the prominences. I saw a large number of reversed lines, including some of the more delicate lines of Young's preliminary catalogue, but on no occasion could I be sure that the oxygen lines were seen outside of the limb. Of course, unless such an observation could be made in a perfectly tranquil atmosphere, certainty could not be attained. The experiments were terminated temporarily on account of getting my right arm caught in the engine, but they will probably be resumed next summer.

On examining Prof. Young's catalogue of chromosphere lines made at Sherman Station, in the Rocky Mountains, it appears that he does not note the great oxygen group near G, and as his observations were made with remarkable accuracy and care, this would tend to corroborate the view that the bright-line spectrum of oxygen as seen on the sun's disc must have its upper limit close to the apparent spectroscopic limb of the sun.

HENRY DRAPER

Observatory, Hastings-on-Hudson, New York, January 28

Brain of a Fossil Mammal

IN NATURE (vol. xvii. p. 222) is an account of some remarkable characters of the brain of *Coryphodon*, as determined by Prof. Cope, and recently published in the *Proceedings* of the American Philosophical Society, vol. xvi. It may interest some of the readers of NATURE to know that the subject had been previously investigated by the writer, who published a description and figures of the brain cast of *Coryphodon* in the *American Journal of Science*, vol. xi. p. 427, May, 1876, more than a year before the article above quoted appeared. Prof. Cope made no reference to my paper, although perfectly familiar with it. His figures moreover do not represent, even approximately, the brain of *Coryphodon*, owing to serious errors in his observations, which were based upon an imperfect specimen, as I have shown elsewhere (*American Journal of Science*, vol. xiv. p. 83). One of the most glaring of these errors is seen in the supposed olfactory lobes which, as figured, include no small part of the nasal cavities, and naturally add a very remarkable feature to this brain cast. The specimens from which my figures and description were taken are in excellent preservation, and are in the Yale College Museum, where they have been examined by Prof. Huxley and many other anatomists.

The attention called by NATURE to this paper of Prof. Cope's makes the present correction seem necessary for English readers, especially as the paper quoted is a typical one, illustrating the methods and work of its author. O. C. MARSH

Yale College, New Haven, Conn., February 7

Origin of Tracheæ in Arthropoda

IN NATURE (vol. xvii. p. 284) is a notice of a work by Dr. Palmen, of Helsingfors, on the morphology of the tracheal system. From the wording of the notice it appears as if the views of Dr. Palmen as to the origin of tracheæ from skin-glands, and as to the importance of Peripatus as an ancestral form of the Tracheata, were new to science. I was, to the best of my belief, the first to discover that Peripatus was provided with tracheæ; and in a paper on the structure and development of *Peripatus capensis*, published in the *Phil. Trans.* for 1874, I discussed the question of the origin of tracheæ, and put forward exactly similar views to those cited in your notice. These views have been adopted by Prof. Gegenbaur in his new edition of his "Grundriss der Vergleichenden Anatomie" (1878), in so far at least as that Peripatus is placed in a separate division of the Arthropoda, "the Protracheata." Haeckel, following Gegenbaur, supposed his Protracheata to have been provided with tracheal gills, but the diffuse arrangement of the tracheæ in Peripatus led me to conclude that the ancestral tracheata were terrestrial, and not aquatic, in habit, and that tracheal gills were comparatively late developments.

I am very glad to find that Dr. Palmen has arrived at similar results. Unfortunately, the place of publication of his treatise is omitted from your notice. It would be of value if you saw fit to append the reference as a note to the present letter.

Exeter College, Oxford H. N. MOSELEY

[Dr. Palmen's paper was published in Helsingfors.—ED.]

The "Phantom" Force[1]

III.

WHILE very clearly establishing that it is to the force urging a body that the potential energy which the body *has not*, but can have, must properly be assigned, and calling it very appropriately the "energy of tension,"[2] a very apposite remark (which I do not remember to have met with before) is added by "X" in his concluding paragraphs. The body could not command this "force-work" in any position unless it had *been put* into the proper position to command it; and the *actual energy* spent in putting it there *is the* "energy of tension" which, although forfeited to the force, *it can* reclaim. In this view it is not surprising that potential energy should have the same terms for its measurement as actual energy, since it is nothing but the actual energy which the body, or some agent operating upon it, has really lost; and if we pa s from permanent forces to those ephemeral ones which physical agents can produce on an already existing arrangement of bodies, then, according to the existing configuration of the bodies when the force is generated, and in proportion to the "potential," or to the available statical energy developed, so is the work of the agent used to bestow this energy. In these cases of temporary "potentials" the actions are not actions at a distance, but through an intermediate medium, it may be strung with motion, and with permanent forces, which have absorbed the work applied to put the intervening medium, as it were, on the stretch, and to develop the ephemeral energy of tension. But we recognise this very clearly (as for instance in charging well-insulated electrical conductors) only in the rare cases of reversible arrangements. The fatigue and exhaustion which we soon feel when holding out at arm's length a heavy weight (although we do no work upon the weight) arises, for example (like that of a galvanic battery exciting an electro-magnet and supporting a heavy armature), from two causes, the first of which, the excitation of the magnet and armature, and the tightening of the muscles, or producing the requisite statical energy for the occasion, absorb but a small portion of the work. The main expenditure is "frittered away" (a most expressive description of the process, which I owe to Prof. Tait) in aimless and random paths as heat, by the wasteful process of electrical or muscular currents afterwards kept up to maintain the excitation.

I have thus far sketched out a general view of physics (one which is perfectly adapted to satisfy its general requirements), in which self-balancing actions and reactions, only depending in intensity on the distance between their centres are supposed to be permanently implanted in pairs of material particles, a special case, or fresh assumption regarding the general system of forces contemplated in the Newtonian theory of mechanics, which either may, or may not be the complete theory of their action, but which assists the mind very greatly, by giving them a mechanical explanation, in forming true and correct preliminary notions of the two leading laws of the great modern science of energy. And here I may take the opportunity to mention that my own views of the relationship of modern physics in its various mutually dependent branches to that famous foundation of mechanics which Newton laid (or perhaps I should rather say, since the supremacy of mechanics is by no means yet conceded, of the Newtonian basis of mechanics to modern physics) have been mainly imparted and completed by a perusal of the excellent little manual on "Matter and Motion" by Prof. J. Clerk

[1] Continued from p. 322.

[2] The term "statical energy" introduced by Sir W. Thomson (see a note in Prof. Tait's "Sketch of Thermodynamics," p. 52), and now proposed (NATURE, vol. xvi. p. 521) by "W. P. O." to be substituted for the above, is of all the phrases yet used to denote it, the truest and simplest description of its real character. That it appertains to the force and not to the body is apparent both from this name and from the definition (which I have endeavoured to illustrate) that it is the "work" of the "agent," a property or possession of *that individual*, equal and opposite to, but *not the same* as its "net," or effected work.

Maxwell, reviewed by Prof. Tait in NATURE (vol. xvi. p. 119), a very moderate acquaintance with which has sufficed to remove from my mind all the doubts and perplexities which, without such assistance, must beset every cultivator of physics and mechanics attempting to take a comprehensive view of these two parallel sciences in their close relations to each other. The latter science especially, mutilated and deformed, and roughly scattered up and down in fragments, as we commonly find it represented, wears in general in our crude brains and in ordinary practice very much the same dismembered aspect which physics in its numerous subordinate branches presents to those who devote their attention especially only to some particular one of its departments.

But the new and comprehensive science of energy has, besides, its own special debatable region, in much the same way that mechanics has, although of an entirely different description; and however cheerfully we might consent, by basing all the propositions of mechanics (a perfectly possible proceeding, as has here been indicated) upon a system of permanent and reciprocal force-pairs, to include among the vicissitudes of force-action, besides its own clearly distinguishable phenomena, also (with countless impenetrably hidden fields of operation) all the known agencies of its more versatile and less easily definable kindred science of energetics, yet it can scarcely be regarded as immediately desirable, in the absence of sufficiently abundant proof, to make this assumption; nor is it perhaps expedient, on the new account just mentioned, to take it too readily for granted as a sound and simple basis of the leading laws of the new science, until the field of phenomena which the latter are framed to include is itself so clearly defined and circumscribed, as not to offer in its own relations and conditions objections to the course which may seem to contain in them anything which may prove to be insuperable, or which might very quickly lead to its abandonment.

To assert the principle of virtual velocities concerning the agent force, although we can voluntarily enlist the action of this agent in mechanical combinations, does not necessarily compromise our free will in any way, because the manner of enlisting this servant of our will cannot be definitely, and in a scientific point of view completely specified as the necessary form which the exercise of volition must take; and accordingly no natural law which completely binds and describes any force, can possibly describe and define also, as completely, the volition which produces it. But even if the volition concerned in producing a force were, as a cause, completely definable, and if we may assume that pure inductive science is capable immediately of so describing it in part, and of ultimately (in its indefinitely achieved development) reaching no partial or imperfect view of every process of volition, so as to be able with assigned actions of will to construct a perfectly unerring plan of all the operations of a Providence subjected to these conditions, and to trace without a single fault or discontinuity the whole current of consequent events belonging to them, yet it is evident that the result would lack an element of genuineness, of whose absence we should immediately be conscious as rendering it an inadequate and unauthentic represesentation of the operations of that perfect will and of that Divine Omnipotence, to whose purposes we owe the obedience and the entire subserviency of our wills in all our actions. This moral obligation of our actions springs from a side of our natures *truly* unseen, but to which we owe dictates of our actions as quick and spontaneous as those which come endorsed with reason to us from our natural senses. On the other hand, to suppose that reason will ever bridge the gap which divides inanimate from living agency, and will be able to register perfectly on her tablets (in the way just now supposed) every event of volition, is as visionary as to suppose her capable of apprehending and of taking a measurable account of the purposes of those actions which we hold to be inspired. But in the part which reason plays as a faculty given to us for learning wisdom and for seeking after and cultivating virtue from our cradles, in all the vicissitudes of life, there appears to be no break or interruption to its ownward progress, though its goals may be partly invisible and partly unattainable; and "new forces" in nature must evidently lie abundantly along its path. The "forces" of living beings, in particular, are inscrutable to it, and those of humanity at least must especially be so, for two reasons, a *moral*, as well as a vital or organic one, both differently descriptive of the ultimate constitution of our free will. If, therefore, there appears no ground (as I believe that Hirn's, and perhaps other experiments, have shown) for introducing an exception of living agents in the law of conservation of energy, perhaps the progress of physiology and of biological physics may also show that to make the same exception in the law of dissipation, or of the loss of availability of energy in every action, is equally incapable of substantiation could we see those forms of energy which we, and other living beings, make use of in apparently transgressing the generality of this law by partially restoring their availability to some very obvious forms of energy.

In this view of infinite progress of investigation, energy must keep its form of energy of motion, or of such energy converted into work of "agents;" and from what has been above described, it is not necessary that the work of these agents should be the energy thus abandoned in *a new kinetic* form. All the actions of an agent *can be imagined* to be consequences of special kinds of motion, but of what advantage it may be *to suppose it*, when in the midst of conceptions so distractingly profound and unapproachable as encircle the new science of energy, an agent as simple and intelligible as mechanical force is presented to our understanding as an example of what an agent of will and purpose may perhaps be like, it is very difficult to reflect upon and comprehend.

At the outset of this long-since-begun, and now quite differently-concluded letter from what I contemplated, I proposed, in connection with Mr. Crookes' famous series of investigations (especially those last crowning points of his discoveries in which vacua so perfect were produced as fairly to eliminate the principal cause of rotation of the arms of a radiometer, originally recognised in the action of residual gas), to point out some means by which, in vacua so complete, the mode of action of force might possibly be elucidated by experiments. A beam of rays, bent and reflected, for example, so as to fall at grazing incidence from the right or left on a flat end, instead of on a vane of one of the arms of a very perfectly-exhausted radiometer, might be found to move it sensibly, and perhaps more distinctly, as the exhaustion reached its limit, in opposite directions corresponding to the directions from which the beam grazed the face, which it would be difficult to attribute to molecular impacts of the residual gas; and in the action of such an external, and to all ordinary perceptions quite uncounterpoised, force (supposing radiation really to produce it), a field of new discoveries relating to direct mechanical effects of the luminiferous ether would obviously present itself, which would be of the highest interest and consequence. But as regards the interpretation of any effects which might be observed, especially in connection with new views of the nature of potential energy which they might open out, I prefer now to refrain from offering any hints or suggestions, knowing that any inquiry which offers prospects of studying force under a new aspect, cannot be guided and directed beforehand, so as either to establish or confute any of the already well-proved laws of its action, but that in the broad principles which the science of energy presents for our consideration and development it could only be prosecuted as a new science, a new branch of general physics contributing something like its predecessors (heat, radiation, chemical action, electricity, &c., but *what* we should attempt in vain to picture to ourselves) in the capacious science of energy, as a new ascent towards that lofty pinancle to which in common with several other natural sciences energetics also proposes to raise itself in the end, to contemplate the Divine works of True Beneficence and to discern in the stately Temples of the Universe the allotted place of man.

These are some of the teachings of the radiometer which rose up before me when in an unguarded moment I asked myself the question: What change from the point of view of energy conservation would it introduce into our view of the experiment if, supposing that a force were found to actuate the vane of a radiometer, which was a direct effect of radiation, we were to sacrifice the integrity of Newton's third law of motion by assuming the existence of a new class of forces which act alone unaccompanied by any equal and opposite reaction?[1] The answer here must be that *if energy is still to be conserved* (that is to say, if we can *point out the source and destination* of all the work that is performed), *there must be a law in* these outer forces connecting them *with known physical agents* in such a manner that as much work is done upon them in any assigned change of configuration as is supplied by those physical agents in the change, and as the internal forces and other agents in the changing system also furnish by their action. (See Prof. Clerk Maxwell's definition of a "conservative system" in "Matter and Motion," p. 59, where the action of internal forces is excluded by supposing the system to

[1] Reaction is not meant here, of course, to imply Newton's imaginary "resistance of an acceleration;" but the real active tendency of some equal opposite force only, is meant to be understood.

return to its original configuration). In other words, we cannot suppose energy to be conserved unless we *connect the new forces by some fixed laws* with known and already determinate physical agents, and we must be content to regard the system *as non-conservative* until the necessary physical connection is assigned and introduced which will account for the free forces that we have observed, and will allow us to comprehend their action under the known laws of inanimate natural agency. This way of dealing with the work of "external forces" on a system which the new science of energy has devised, and shown to be the only one which in these cases can be generally employed, has perhaps contributed (but only by the unavoidable abstruseness and abstractness which belong to the new science itself) to invest with something of the appearance of a "phantom" and with an air of mystery, the character of force, and the laws of its operation as they have been universally studied in mechanics. But rightly regarded according to the simple principles of philosophical consistency and progress, which the new science of energy recognises in its probable extensions, there can be no doubt that it will really tend to establish more clearly than before the familiar notions of mechanics, and to open out fields of application of the time-honoured laws of motion and of force in unforeseen directions, in which their certainty and truth will continue to be felt as surely and to be described as simply now and hereafter, as they were in the days of Galileo and of Newton.

Newcastle-on-Tyne A. S. HERSCHEL

Faraday's "Experimental Researches"

MR. SILVANUS P. THOMPSON, of Bristol, has made, in NATURE (vol. xvii. p. 304) an inexplicable attack upon my issue of Faraday's "Experimental Researches in Electricity," 3 vols. 8vo. 1839–55, unwarranted by logic or facts.

Mr. Thompson ordered my issue, which is advertised as "*a perfect copy*" of Faraday's work, through a Bristol bookseller, to whom it was charged at the trade price of 36*s*. Mr. Thompson declined to ratify his purchase, and there the matter should have ended, as I would readily have cancelled the transaction with his agent.

Mr. Thompson says that I *profess* to supply *a perfect copy* of Faraday's "Experimental Researches," implying that my professions are deceitful. I am at a loss to understand his meaning, because the fact is that I can and do supply perfect copies.

The history of the book is as follows:—Faraday's "Experimental Researches," 3 vols., appeared in 1839–55, in 3 vols. 8vo, with plates, and in course of time two of the volumes fell out of print, which raised the market value of occasional copies to seven and eight guineas. Availing myself of the opportunity of buying from Mrs. Faraday the copyright and existing stock, I completed, by facsimile reprint, a small number of copies, as is plainly stated on the title-pages of vols 1 and 2. I alone possess the right to reprint the whole or a portion of the work.

It was at Mrs. Faraday's express wish that only such a reprint has been executed, and I was further advised to that course by eminent Fellows of the Royal Society. Mr. Thompson's innuendo of wilful deception is an infamous slander unworthy of a man of science.

I consider I deserve the thanks of all purchasers of Faraday's "Researches" for having invested my capital in the long dormant copyright of this work, and having thus put it in the power of students to obtain "perfect copies" at a moderate price.

Messrs. Taylor and Francis, the printers of the former edition, executed for me the reprint of the first two volumes (the stock of the original third volume not having been exhausted.) The original dates were preserved to show that no alterations had been made, and to preclude the notion, which Mrs. Faraday desired to avoid, that she had sanctioned a veritable new edition.

After this explanation I do not doubt that Mr. Thompson will see he has deceived himself, and I expect that he will apologise for his ill-advised attack upon my genuine and authorised re-issue, and admit that it is indeed a perfect copy of Faraday's great work. BERNARD QUARITCH

Singing in the Ears

THIS consists of two or more continuous or alternating tones originating within the ear, very faint and sounding like a tea-kettle just beginning to boil, or a distant orchestra tuning. It is heard when there is undue pressure of the circulation in the head, as after long mental application, or upon hanging the head downwards. To my ear these tones bear constant musical relations to each other, and as the phenomenon bears directly on the theory of the *mechanism* of *audition*, its verification is a matter of importance.

Will the readers of NATURE who can observe it write me what are the *intervals* of *pitch* (*i.e.* thirds, fourths, octaves, fifths, &c.) between the different tones heard together or alternating?

XENOS CLARK

916, Washington Street, San Francisco, Cal.

Meteor

ABOUT 12.47 A.M. (Irish time) on Monday the 18th inst., I observed a brilliant meteor when looking north from the south side of Dublin. It resembled a bluish white ball with an apparent diameter of about one-fifth that of the moon, which was shining brightly at the time, and left behind it a continuous yellowish luminous train. When first sighted it appeared at an angular distance of about 15° from the polar star, and appeared to be in the constellation of the Dragon, about midway between the brilliant star of the constellation of Lyra and the polar star, somewhat below the line joining these constellations.

Its path was apparently a line about 5° from the vertical, and inclined from west towards east, and I lost sight of it when at an angle of 10° with the horizon by intervening buildings. Its brilliancy surpassed that of the moon, which at the time was bright enough to allow of distinguishing printed characters. It did not burst while in sight, and I heard no report.

Royal College of Science, Dublin H. HATFIELD

Eucalyptus

I have only just observed Dr. Calmy's letter in your impression of the 7th inst. (p. 283). The febrile attacks to which I alluded in NATURE (vol. xvii. p. 10) were sufficiently serious to incapacitate shepherds and stockmen for anything like continuous work for two or three weeks, and on some days the men were quite prostrated. The mosquito of which I spoke as not being banished by the presence of Eucalyptus is that species of Culex whose larval state is passed in water (the larvæ may even be seen in rain-water collected in decayed parts of trees), and I cannot call to mind a single place from which these pests were absent, trees being present. No doubt they may be carried many miles by the wind from their place of birth; but the real question is whether any species of gum so drains the land as to banish both mosquito and malaria by drying wet soil. If so, how is it that we find in Australia swamps which have existed for apparently an indefinite time, and do not look in the least likely to dry up, though the "blue" gum grows all round them, where the mosquito is rampant and malarious fever not by any means rare? I entirely agree with Dr. Calmy that the mosquito may be a "real danger to the rash traveller." One not acclimatised would suffer agonies among the mangrove swamps of Moreton Bay. I have had my own hands so paralysed by the poison that I could not close them without difficulty; and a new arrival, whom I took there on a duck-shooting expedition, was almost blinded, and became seriously ill for some days, though he was exposed to the attacks of the insects only a few hours. Whatever may be the case in Algeria or the Campagna, no one familiar with Australia will give the gum-trees there credit for having banished swamps, malaria, or mosquitos. Is not the Newfoundland mosquito of the pine forests to which Dr. Calmy alludes bred in water? ARTHUR NICOLS

February 20

Telephone Experiments

THE following experiments with the ordinary small portable telephone may interest your readers.

Experiment 1. Connect a small strip of zinc by a thin covered wire to one of the binding screws of the instrument, and connect in the same way to the other binding screw a plate of metal with a rough edge; a saw does well. Place the end of the piece of zinc in the mouth, or hold it between moist fingers. Take a shilling between the fingers of the other hand and pass it along the teeth of the saw. The sound is clearly heard in the telephone. If instead of a shilling, a sovereign or a penny be used, the result is much the same, but if a piece of zinc be substituted, the sound in the telephone, if not lost, becomes very feeble.

Experiment 2. With the apparatus as before, let a number of persons, taking hand in hand, form a chain. At one end of this chain the zinc is held, and at the other the shilling. When the saw is rubbed the sound is heard in the telephone so long as the hands are held, but on leaving go anywhere in the chain the telephone becomes silent. This experiment is successful with

eight persons, and no doubt would be with a larger number. The hands should be moistened.

These experiments show in a simple and striking way that in the telephone we have an instrument which is sensitive to very minute electric impulses. W. CARPMAEL

24, Southampton Buildings, W.C.

ELIAS MAGNUS FRIES

BY the death of Fries, Sweden has lost one more of the line of eminent botanists whose labours have thrown a lustre so great upon Scandinavian science. Well versed in all domains of phanerogamic botany, and especially skilled in his native plants, it was amongst the cryptogams he spent the more active years of his long life. While lichenology owes to him valuable illustrations, fungology received at his hands a large element of its construction. In the acquisition, description, and systematic arrangement of the larger fungi he exhibited a zeal, a tact, and a perspicuity which seem to have left comparatively little to be done in later times, either by way of addition or improvement.

Elias Magnus Fries was born in Småland on August 15, 1794. His father, pastor of the church at Femsjö, was an ardent and accomplished botanist. As there were no boys of his own age whom the young Fries could make companions, he constantly accompanied his father in his walks, and was in his earliest years made intimate with all the flowering plants of a district diversified by forest, mountain, marsh, and river. About the age of twelve he lighted upon an especially brilliant Hydnum, and was then first incited to the study of the Agarics and their allies, that abound in his native land more than in any other region of Europe. Before he left his school at Wexio he knew, and had given temporary names to, nearly 400 species. In 1811 he entered the University of Lund, where he had Schwartz, Agardh, and Rezius as his teachers; and in 1814 was chosen Docent of Botany. In this year he published his "Novitiæ Floræ Suecicæ," first part, the second part following in 1823. In 1815 appeared his "Observationes Mycologicæ," the first important result of his fungological researches. In the following year, dissatisfied with the method of Persoon, he began to construct an entirely new system. As a first fruit he wrote his "Specimen Systematis Mycologicæ," a tract of a few pages, and, in outline, his great work the "Systema Mycologicum," the first volume of which appeared in 1821 and the last in 1829. In 1825 he sent forth the "Systema Orbis Vegetabilis," first part, a work not further completed, and in 1828 the "Elenchus Fungorum," a commentary on the Systema. In 1831 was published "Lichenographia Europæa Reformata," and in 1838 his second great work, the "Epricisis Systematis Mycologici." About this time he completed the manuscript of a "Synopsis Ascomycetum," in which he had included upwards of 600 new species. Owing to his impatience of the critiques of Corda, Kunze, and the German fungologists who had begun to avail themselves of the new aid of the perfected microscope, an assistance which Fries denied himself, he refrained from publishing it, but one may hope this valuable MS. may still exist. In 1834 he was made Professor of Practical Economy at Upsala, from which place he gave out the "Flora Scanica." He was sent to the Rigsdag in 1844 and 1848 as representative of his university, and was made a member of the Swedish Royal Academy in 1847 In 1851 he succeeded to the chair of Botany at Upsal, vacated by Wahlenberg, which he resigned only a few years before his death to his son. In 1846 he published the "Summa Vegetabilium Scandinaviæ," and in 1860, "Sveriges ätliga och giftiga Svampar," with fine coloured plates. A project of the Royal Society of Holm to publish at its expense drawings of all species of Hymenomycetes under Fries's direction, induced him to write a third and fuller description of the Agarics, of which he printed only 100 copies, under the title of "Monographia Hymenomycetum Suecicæ," in 1863. The first fasciculus, however, of the corresponding "Icones," appeared only in 1867; a second volume was commenced towards the end of last year. Fries lived at Upsal all the latter years of his life, in good health, and in constant correspondence with the botanists of this and other countries, taking, so far as his age permitted, all his early interest in his favourite Agarics. Thus he published, in 1874, a second edition of his "Epicrisis," including in it all the later found European species.

He died, after a short illness, on the 8th inst.

THE TELEPHONE, AN INSTRUMENT OF PRECISION

THE applications to which the telephone may in future be put cannot yet be all foreseen. I have to-day had its value shown to me in a remarkable way. 1. I used a thermo-electric intermittent current by drawing a hot end of copper wire along a rasp completing the circuit. A telephone was put into the circuit, in another room, and every time that the wire was drawn along the rasp a hoarse croaking was heard in the telephone. 2. I used a thermopile with a Bunsen burner shining on it from a distance of six feet. The current was rendered intermittent by the file, and the sound was most distinctly heard. A Thomson's reflecting galvanometer was introduced into the circuit which showed that the currents were extremely small. 3. The feeblest attainable currents were now tried. The thermopile was removed, and without any artificial application of heat it was shown by the galvanometer that the natural differences in the temperatures of the different junctions in the circuit were sufficient to generate feeble electric currents only just perceptible with the mirror galvanometer. These were easily detected by aid of the rasp and the telephone. Even when contact was simply made and broken with the hand, a click was heard in the telephone. 4. Lastly, these feeble currents were rendered still more insignificant by passing them through the body of a friend who held one end of the wire in each hand, and still the effects were faintly audible. Here the galvanometer, which was still in circuit, hardly gave any indication.

I have now added the telephone to the list of apparatus in the laboratory, considering it to be perhaps the most delicate test of an electric current which we possess.

In these experiments only one telephone is used, viz., at the receiving end. Employed in this way with a powerful current sent from the other end of the line, we may hope to have messages sent through submarine cables much more rapidly than at present. Probably it will be best to have the intermittent nature of the current maintained by an induction-coil, or by a spring rubbing against a continuously rotating cog-wheel, when the current is allowed to pass only when required by the depression of a key which communicates to the listener at the receiving end the long and short dashes of the Morse alphabet.

I ought to mention that I believe the person who first used a thermo-electric current with a telephone was Prof. Tait. GEORGE FORBES

Andersonian College, Glasgow, February 13

OUR ASTRONOMICAL COLUMN

LOHRMANN'S LUNAR CHARTS.—At last astronomers are put in possession of the charts of the moon's surface, commenced by W. G. Lohrmann, of Dresden, in 1821. They are now completed in twenty-five sections; but previously only one part, containing four topographical sections, had been published. This was issued at Leipsic in 1824; a small general chart was lithographed at Dresden at a later period. It is through the active

interference of Prof. Julius Schmidt, the Director of the Observatory at Athens, whose elaborate lunar work is well known, that the complete publication of Lohrmann's charts has been effected. A pretty detailed prospectus has been circulated. The price of the entire work will be 2*l.* 10*s.*

THE PERIODICAL COMET, 1873 II.—The interesting comet of short period discovered by Tempel on July 3, 1873, was, it is understood, taken in hand by one of the able astronomers attached to the Observatory of Vienna, in which case an ephemeris may soon be expected. The last calculation of elements assigned a period of revolution of only 1,850 days, and it is possible that observations in November, 1873, may have indicated a still shorter period, so that the comet may again arrive at perihelion very early in the summer. Four days after the perihelion passage this comet makes a very near approach to the orbit of Mars, but the recent discovery of satellites to this planet detracts from the importance which would otherwise have attached to a study of the comet's motion, in the hope of eventually improving our knowledge of the planet's mass.

MINOR PLANETS.—Of all the members of this group the elements of which have been calculated, No. 153, *Hilda*, discovered by Palisa at Pola on November 2, 1875, makes by far the nearest approach to the orbit of Jupiter, and on this account it is desirable the planet should be kept under observation at successive oppositions. So far, it does not appear to have been recognised during the present one, though an ephemeris extending to February 20, was given in number 84 of the *Berlin Circular*; this will have been owing, no doubt, to its situation in a part of the sky for which we have no charts showing very small stars. According to the estimation made by Palisa on the night of discovery when the planet's distance from the earth was 3·22, and from the sun 4·20, its brightness at present will be equal to that of a star of the thirteenth magnitude. Subjoined is a continuation of the ephemeris deduced from the best orbit yet available, that by Kühnert, which is founded on observations from November 2 to December 30, 1875:—

HILDA: AT GREENWICH MIDNIGHT.

	R.A. h. m. s.	N.P.D. ° ′	Dist. from Earth.
February 28	7 57 36	78 49·2	3·723
March 2	7 56 57	78 44·5	3·760
,, 4	7 56 21	78 39·9	3·780
,, 6	7 55 48	78 35·3	3·801
,, 8	7 55 18	78 30·8	3·823
,, 10	7 54 52	78 26·5	3·845
,, 12	7 54 30	78 22·3	3·868
,, 14	7 54 12	78 18·1	3·893

Mr. W. Godward, of the *Nautical Almanac* Office, availing himself of the observations of Ceres which have been made at the Royal Observatory, Greenwich, at every opposition between 1857 and 1876, has corrected the elements of this, the first discovered of the small planets, and has succeeded in representing its course during the interval of about twenty years, with a precision which we do not remember to have seen attained in any previous investigation of the like nature. The residual errors in R.A. and declination in no case amount to five seconds of arc. Applying the corrections given by Mr. Godward in the *Monthly Notices* of the Royal Astronomical Society for January last, there result the following doubtless very exact elements of Piazzi's planet for 1878:—

Epoch, 1878, November 16·0 G.M.T.

	° ′ ″	
Mean longitude	47 50 24·5	
Longitude of perihelion	149 40 57·1	From M. Eq. of Epoch.
,, ascending node	80 47 43·1	
Inclination	10 37 17·9	
Angle of eccentricity	4 30 57·2	
Mean daily motion	771″·31117	

From the *Berlin Circular* No. 85, it appears that with the exception of the small planet observed by Prof. Peters on February 6, which proves to be *Antigone*, the planets lately observed are new. Their numbers and discoverers, with dates of discovery and magnitudes, are:—

No. 180	Perrotin	Jan. 29	12·0m.
No. 181	Cottenot	Feb. 2	10·0m.
No. 182	Palisa	Feb. 7	10·5m.
No. 183	Palisa	Feb. 8	12·0m.

As we anticipated, No. 180 proves to be distinct from *Urda*, which remains to be recovered.

BIOLOGICAL NOTES

THE ORIGIN OF THE CARBON OF PLANTS.—Mr. J. W. Moll has made in Prof. Sach's laboratory at Würzburg, some researches on this subject during the summer of 1876. A detailed account of these, with the conclusions at which he has arrived, is promised in the *Landwirthschaftliche Jahrbücher von Nathusius und Thiel*, but a brief account will be found in the last number of the *Archives Néerlandaises*, tome xii., 4me livre. A plant with green-coloured cells can, under the influence of light, take the carbon it requires from the atmosphere, releasing in the act of doing so, so much oxygen. This is a fact, too well vouched for by the experiments of Boussingault, Vogel, Rauwenhoff, and Harting, to admit of a doubt, but the quantity of carbon dioxide in our atmosphere is very small, and the quantity of carbon stored up during say a summer's growth in some large forest, is very great. Moreover, the roots of such plants are fixed in a soil which is highly charged with carbonaceous products, so the question quite naturally arises, may not the roots take up some of these atoms of carbon ready to their hand? or may they not at least take up the carbon in the form of carbon dioxide, send this up the green granules in the leaves, and so give them a more abundant supply than they could get from the surrounding air? Besides, is it not a fact that most plants seem to thrive in a fine rich leaf mould, and may not its richness in carbon be partly the cause? One of the first questions Mr. Moll set himself to answer was—Can leaves decompose carbon dioxide which is furnished to the root of the stem from which the leaves spring? Now, starting with assent to Prof. Sach's discovery that the starch of the chlorophyll granule is the first visible product of the fixation of some carbon atoms, there was here a ready method of proving whether this were so or not. In the course of several experiments it was contrived that leaves destitute of these starch granules should be in an atmosphere deprived of carbon dioxide, while at the same time they were well exposed to the influence of light. The roots were fixed in moist soil well supplied with carbon dioxide, and the result was that under these circumstances no starch granules were formed; and in a modification of this experiment, where one portion of a leaf was allowed to be exposed to ordinary air, that portion at once set to forming its starch. Botanists no doubt will welcome the publication of the experiments of which we have now only the brief result; doubtless more research will end in more discoveries in this most interesting field, for how can one account for the fact that some plants do, as we might say, fatten by feeding on carbon atoms, although these very plants cannot take these atoms when in union with oxygen?

FERNS AND MOSSES.—Hofmeister's work on the "Higher Flowering Plants (Cryptogamia)" is truly indispensable to every scientific botanist, and, thanks to Mr. Curry, the English student has it at his command. It commences with an account of a not uncommon little plant called *Anthoceros lævis*, and it finishes with an account of those cryptogams very high in rank and vast in size, known to us as cone-bearers, and of which the churchyard yew or the giant Wellingtonia may serve as

types. Of these cryptogams none are better known than the ferns and the mosses, and as the reader of Hofmeister's work, or, as we are but too glad to be able to add, of most of the very recent handbooks of botany, well know, both of these groups have this in common—that they pass, as it were, through two existences, one of which we may call the "fruit-bearing" stage (the sexual stage), and the other the "spore-bearing" stage (the a-sexual stage). The former of these two is the stage so apparent to us all in flowering plants, where, as a product of the fertilisation of the contents of the carpel by the pollen from the stamens, we have the fruit. In the ferns, as a rule, this first stage is one in which the plant, as it were, thinks only of producing its male and female cells, and the growth of the plant is lost in the care which it takes to continue the species. Shake a spore from the frond of some immense tree-fern, let it germinate, and the plant which will grow thereout will be a little green thing not so big as the top of one's thumb; but it will form its "archegonium" and its "antheridium," and the contents of the latter fertilising the contents of the former, the result will be a plant which in time will equal the large tree-fern in size, but which at this, its great vegetative stage, will never produce aught but spores. In the mosses this state of things is different. The moss-stems which we gather as objects of beauty or use, these are the fruit-producing stages; these concern themselves with growth as well as with what is usually antagonistic to growth, reproduction; and in the second stage, which in the ferns is the only one popularly known, we have but a short-lived, small-sized, spore-producing plant, sometimes quite hid away in the lovely foliage of the moss plant, sometimes starting up from it, and then known popularly as its fruit, but really only its spore-producing stage. It is only very recently that Dr. Karl Goebel (*Botanische Zeitung*, October, 1877) has called attention to the development of the prothallium (sexual stage) of a delicate little fern called *Gymnogramme leptophylla*, which is to be found in Jersey, along both shores of the Mediterranean, and probably in all suitable localities in Africa, Asia, Australia, and South America. It and a few other species are annuals, so that at once we see that their a-sexual stage, which is also their vegetative one, is quite limited. Moreover, their little stems are often not more than an inch in height and the texture of the frond is almost pellucid. It thus approaches the mosses in the feebleness of this stage; but the most interesting fact brought to light by Dr. Goebel is that the sexual stage, generally in the ferns so evanescent, is here absolutely somewhat long-lived, and more, that it is even somewhat vegetative, something like that of *Anthoceros lævis*. Such a form, which makes a bridge to thus connect the two groups (ferns and mosses), is of great importance, and Dr. Goebel's memoir, which is illustrated, is not only of great value from the accuracy of its details and from his deductions therefrom, but also as showing how much can be done even with apparently well-known forms.

Prof. Grimm on the Fauna of the Caspian.—We notice the appearance of the second part of Prof. O. A. Grimm's (Russian) work on the Aralo-Caspian Expedition. It is devoted exclusively to the Caspian and to its fauna, and contains the description of worms, sponges, and molluscs, discovered during the expedition, together with a general sketch of the vertical and horizontal distribution of Caspian molluscs. Prof. Grimm divides them into three regions, out of which the lowest one (deep sea) corresponds to older forms of fossils, whilst the upper one has its nearest relatives in youngest forms of fossils. In a concluding chapter Prof. Grimm discusses the interesting question as to the influence of conditions of life on morphological structure, and shows by many illustrations the accommodation of forms to varied conditions at different depths. The work is illustrated by many drawings.

Transformation of Cartilage into Bone.—The last *Bulletin* of the Belgian Academy of Sciences (vol. xliv. No. 11) contains a very valuable paper of Dr. Leboucq, Superintendent of Anatomical Researches at the University of Ghent, on the mode of formation of the bone tissue in the long bones of mammalians, in which the author discusses and resolves by his researches, based on a new principle, the much-debated question whether the embryonal cartilage is substituted by a new tissue, or is directly transformed into a bone. The great difficulty of rendering the minute cartilage cells apparent among other cells, is resolved by the author by his employing soda, and decalcifying the sections with acidulated glycerine; the cells thus preserve their shape, and receive a beautiful colour, as is seen from a chromolithographed plate accompanying the paper. By using this method the author was enabled to prove that the minute cartilage cells take an active part in the formation of bones, quite performing the part of osteoblastes. The researches were carried out in the Ghent Laboratory, under the direction of Prof. van Bambecke, and the paper is accompanied by a very favourable comment thereon by Prof. van Beneden.

Owls.—M. Alphonse Milne-Edwards has recently read before the Academy of Sciences of Paris two ornithological papers of interest. One on the affinities of the Owl, *Pholidus badius*, demonstrates, from its skeleton, that it belongs, quite contrary to the general opinion of naturalists, to the Bubonidæ, near to *Syrnium* and *Nyctale*, and not to the Strigidæ. In the species the posterior margin of the sternum has two pairs of well-developed notches, and the furcula is not complete at its symphysial extremity. This being the case, the genus *Strix* is now the only member of the family of the Strigidæ, and the pectination of the inner edge of the nail of the third toe found in it is no longer to be taken as of much importance in the group. In the second paper a new genus of Strigine Owls is described, from Madagascar, and named *Heliodilus*.

Algæ of the White Sea.—At a recent meeting of the St. Petersburg Society of Naturalists, M. Chr. Gobi read an interesting paper on the algæ of the White Sea. The number of species he has discovered reaches seventy, of which ten are green algæ, six *Fucus*, and twenty-nine red algæ. The algæ of the White Sea are a mixture of representatives of the Arctic and of the Atlantic basins, as well as of fresh water and salt water forms, the mixed characters of the flora being especially obvious with respect to the green algæ.

GEOGRAPHICAL NOTES

Lapland.—An important exploration of Russian Lapland is being carried out by the Swedish lieutenant Sandeberg. Hitherto only the coast of the region has been known with anything like accuracy, the interior features being set down solely from conjecture. Lieut. Sandeberg commenced his work in 1876, and we learn from the *Geographische Blätter* (Heft 1, 1878) of the Bremen Society, it will be continued till 1880. The country will be carefully explored and accurate observations taken, which will enable Lapland to be at last mapped satisfactorily. Lieut. Sandeberg is accompanied by several zoologists who are investigating minutely both the mainland, island, and sea fauna, and have already made considerable additions to our knowledge in this direction. During the last two summers Sandeberg has found seventy-eight new species of birds in the Kola peninsula, of which one at least is stated to be quite new to science. Large collections in other departments have also been made. Previous to Sandeberg, no educated European has explored Russian Lapland, which is of such great importance to the zoologist,

geologist, botanist, and archæologist. Among other finds it may be mentioned that near Golotizk, on the east coast of the White Sea, he found a great ancient manufactory of flint implements of the stone age, of the purest and highest Scandinavian forms, which previously had been seldom found east of the Baltic, and never on the coast of the Arctic Ocean or the White Sea. The collections will be divided between the State Museums of Russia, Sweden, and Norway, all three countries affording facilities for the conduct of the expedition.

CHINA.—In accordance with the terms of the Chefoo Convention, Her Majesty's minister at Peking, about a year ago, sent to Chung-king, in the Chinese province of Szechuen, which lies at the junction of the River Ho-tow with the Yang-tsze Kiang, Mr. E. Colborne Baber, of her Majesty's Consular Service, who was one of the interpreters attached to the Yünnan mission, and who, before proceeding on that bootless errand, was at considerable pains to qualify himself for scientific exploration. Mr. Baber started last July on an expedition in the western districts of the province. But little was heard of Mr. Baber's doings until the end of the year, except from a private letter in which he described himself as floating down the River Min, among low hills covered with fir and insect wax trees, and in sight of (though at a distance of sixty miles on the south-west) the holy mountain of Omi, on the borders of Thibet. On December 27 the *North China Herald*, of Shanghai, published a portion of another letter from Mr. Baber, in which he mentions that, from the point just named, he made north-west and from Ya-chow began to veer south. Passing Ning-yüan-foo he went to Hwa-li-chow; then turned east and crossed the Yang-tsze into Yünnan, not far from Tung-chwan. Thence through the wildest and poorest country imaginable, the great slave-hunting ground from which the Lolos carry off their Chinese bondsmen—a country of shepherds, potatoes, poisonous honey, lonely downs, great snowy mountains, silver mines, and almost incessant rains, Mr. Baber tracked the course of the Upper Yang-tsze to Ping-shan. No European, he says, has ever been in that region before, not even the Jesuit surveyors, and the course of the Yang-tsze, there called the Gold River (Kin-sha Kiang), as laid down on their maps, is a bold assumption and altogether incorrect. Mr. Baber adds that "a line, drawn south-west from a mile or two above Ping-shan, will indicate its general direction, but it winds about among those grand gorges with the most haughty contempt for the Jesuits' maps."

MOUNT TONGARIRO.—The celebrated burning mountain of New Zealand, Tongariro, has at last been explored by an Englishman, Mr. P. F. Connelly. The volcano is regarded as *tapu*, or sacred, by the Maoris, who have hitherto resisted all attempts to explore the mountain on the part of the colonists. The volcano is situated nearly in the centre of North Island, and though only 6,500 feet high, is less accessible than either Mount Edgecumbe or Ruapehu, both of which exceed 10,000 feet in height. Mr. Connelly overcame all resistance, and by the help of some chiefs more friendly than the rest, succeeded in thoroughly exploring the crater, took a number of sketches and photographs of the locality, and determined the positions of the most important peaks.

AFRICAN EXPLORATION.—The King of the Belgians has sent to M. Quatrefages a telegram stating that two other Belgian officers should proceed to Zanzibar within a few days, to supply the places of the unfortunate MM. Crêspel and Maes, whose death we announced last week. Telegraphic orders have been sent to the remaining members of the expedition to continue their journey to Tanganyika. The Paris Geographical Society, anxious to acknowledge such a determined policy, have resolved to take steps to accelerate the public subscription instituted on behalf of international African exploration. It has been resolved also to establish a local committee on a very large scale; not less than a hundred persons of distinction will be selected, with power to add to their number.

PARIS GEOGRAPHICAL SOCIETY.—The distribution of prizes will take place not in April, as usual, but at the meeting to inaugurate the Society's hotel, now building. It will be ready in the month of September or October next. The gold medal will be awarded, as already reported, to Mr. Stanley, but another gold medal of the same value will be given to the veteran M. Vivien de Saint Martin, the celebrated geographer, for the many valuable works published by him during the last thirty years, and principally "L'Année Géographique."

AMERICAN GEOGRAPHICAL SOCIETY.—We have received two numbers of the *Bulletin* of this Society, containing the proceedings of the meetings for the first half of 1877. One number is devoted to the admirable summary of geographical work for 1876, which constituted the address of the President, Chief Justice Daly, and to which we alluded at the time. In the other number (No. 4) the principal paper is on the volcanoes of the U.S. Pacific coast, by Mr. S. F. Emmons.

MAPS OF THE SEAT OF WAR.—The Russo-Turkish war has called forth a very large number of maps of the Balkan peninsula. We learn that a Russian gentleman has made a collection of maps of the seat of war, numbering more than 150, and will exhibit the collection at Paris. The largest number of such maps has been published in Germany, and the most detailed maps appear to be those published in Finland.

ARCTIC EXPLORATION.—Mr. James Gordon Bennett has petitioned the U.S. Congress to grant the American register to the steamer *Pandora* for an Arctic expedition under the command of American naval officers.

SOCIAL ELECTRICAL NERVES[1]

THE efficient carrying out in a large city of any extended system of telegraphic communication for police, fire, and social purposes demands an intimate acquaintance with existing systems, so as to insure the establishment of only the most perfect organisation. In an ordinary telegraphic communication between two or more stations a line wire connects the terminal station with the instruments in the circuit, and the distant end of this wire is in connection with the earth, while the other end, after connection through the instrument, passes to one pole of a battery, the other pole of which is also in connection with the earth. Thus the electrical circuit is completed partly by the line wire and partly by the earth wire. Such is an ordinary circuit. At times when telegraphic communication is required only for short distances, as in houses and buildings, a second wire takes the place of the earth circuit. In the auto-kinetic system for the introduction of fire, police, and social telegraphs upon an extended scale an essential feature is the employment of two parallel wires, laid over a city and suburbs, starting from a central station to the various district stations, and from thence ramifying in every direction so as to embrace the most important areas for the purposes required. Each of these two wires has its special duty to perform. One is employed for the purpose of starting the instrument, which may therefore be termed the "starting" wire. The other is used for the transmission of the message, and may be termed the "transmitting" wire. It is by this novel arrangement that the auto-kinetic system enables any number of speaking stations to be placed upon a circuit without possibility of interference. Thus in each district of a

[1] Continued from p. 306.

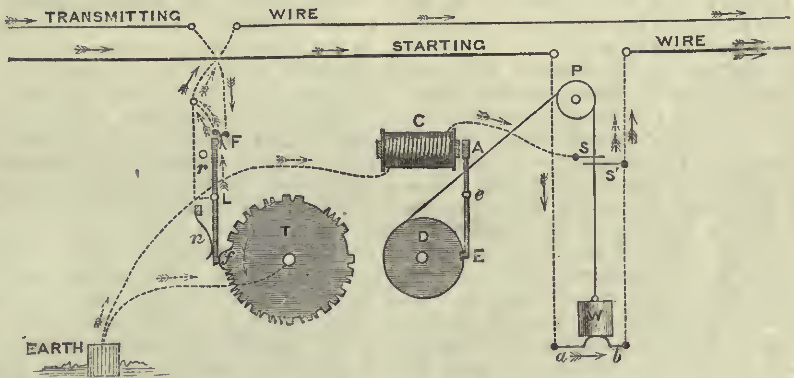
city—say Glasgow—the head police and fire station in the central district will be in direct communication with the sub-stations in the northern, western, southern, eastern, and St. Rollox districts, and each of these again will be local centres, and command a host of street, fire, and police "call" stations placed at convenient distances along the thoroughfares in their respective districts.

Another distinctive principle of the auto-kinetic system is that which has already been noticed, namely, the "starting" and "transmitting" wire traversing the streets together. If we suppose a number of speaking stations to be required along the route—say 300—it is evident that at each point where an instrument is placed the two wires will require to be brought into the instrument—the one to start the instrument when a communication is to be sent, and the other to pick up and automatically transmit that message to the district centre. We have before stated that no instrument is "in circuit" except when speaking, and then only during the time occupied in the transmission of the message. It therefore follows that at each station along the line, while in a state of rest, the electrical continuity of both the "starting" and "transmitting" wire must be maintained independent of the apparatus.

A general outline of the manner of employing these two wires will explain the system.

We will commence first with the *modus operandi* of the "starting" wire, then with that of the "transmitting" wire, and finally describe the nature of the automatic apparatus by which the novel and important features of the auto-kinetic system are obtained.

Assume for a moment that the apparatus is inclosed in an iron erection somewhat similar to that of a street pillar letter-box, and that in the inside of this box one portion of the instrument consists of an ordinary electro-magnetic coil C, an armature A, and detent E working on a centre *e* locking into a small arrangement of wheel-work and barrel D, without any maintaining power; and that one end of the wire of this electro-magnetic coil is connected permanently with the earth, the other end being attached to a metallic spring S placed in position to form a contact, under certain conditions, with a second spring S′ in connection with the "lead in" from the starting wire nearest to the district sub-station. In this position as regards the instrument it is a broken circuit, and of course, therefore, the instrument is out of connection. If now we trace the other "lead in" from the "starting" wire and conceive it to be brought into the pillar-post and carried up to the second spring S′, and as from this spring a connection exists with the "starting" wire, a continuous metallic circuit is established through the pillar-box, indicated in the illustration by the ⋙⟶ as far as the passing of "starting" electric currents is concerned from any other instrument along the circuit which may have to travel the wire without affecting the instrument under notice, which



Arrangement of Circuits—"Call Station" Instrument.

remains electrically out of circuit by reason of the break in continuity at the spring S attached to the coil wire. We will now advance a step further, and assume that the continuity of the "lead in" of the "starting" wire furthest from the district station is only maintained so long as a weight W rests upon the two ends *a* and *b*. It will therefore be evident that were the weight W raised, the through continuity at *a b* maintained by the weight would be destroyed, and until so far raised as to mechanically press the springs S and S′ together—the coil wire spring and the lead in "starting" wire spring—the through circuit on the starting wire is broken. Now the weight W being raised what follows? The moment that the springs S and S′ are brought into contact electrical continuity is established between the district station and this instrument, and the battery current flows from that station along the starting wire, passing all intermediate instruments which are necessarily out of circuit, enters the electro-magnetic coil C, and throws the instrument to earth as a terminal with the district centre, at once cutting off all interference that might arise from other instruments along the line speaking at the same time.

Thus, for the moment that the two springs S and S′ are in contact, the current has entered the coil C, the armature A has been attracted, and the detent E being withdrawn has liberated the drum D which commences to revolve, having been wound up by the elevation of the weight W.

The liberation of the detent, therefore, instantly causes the weight W to descend, and the act of falling separates the two springs S and S′, the starting wire, as far as this particular instrument is concerned, being thrown off, and the through circuit also remaining broken until the weight has descended and closed the contact by pressure at *ab*.

Before we proceed, let us suppose that during this interval of time between the raising of the weight and its falling, some other instrument or instruments along the line had been called into requisition, what would happen? The weight W at each instrument would have been raised, placing the springs S and S′ in metallic contact, and the instrument thrown into position ready for *speaking*; but as no current could pass along the starting wire until continuity had been restored at the *ab* of the first speaking instrument, which for the moment had become a terminal instrument, no current could enter the coil C of the second instrument; and as the detent E could not be released, the weight would remain suspended, until the arrival of the current along the starting wire attracted the armature and released the drum; in due course the second instrument is immediately thrown off the circuit, and succeeded automatically by a third, fourth, or any number along the line in succession, according to their distance from the district centre or battery station. It will therefore be seen that as the battery current always is in readiness to

follow down the "starting" wire from the central station, were twenty or thirty instruments set in action simultaneously; that nearest the central station will record first, and as, in the act of recording, it becomes a terminal, the remainder will follow in the order of their distance along the "starting" wire from the central station. Thus were twenty fires to occur in a district at the same time, and twenty "call" instruments were to be simultaneously put into requisition, the whole twenty would record their several messages without interference at the central station, following one another in successive intervals of time, determined by the automatic falling of the weight on to the circuit poles *a* and *b*. Now, as this interval of time for each instrument is about three seconds, the whole twenty messages would be automatically recorded in about one minute, or less time than it has taken to read the account of what would take place.

We have now traced the action of the "starting" wire, which may be stated to perform its functions mechanically by the act of raising a weight momentarily placing it in circuit with the instrument, which becomes immediately a terminal station; and again by the falling of the weight the instrument is thrown off, and the through circuit along the "starting" wire restored.

We proceed to examine into the action of the second or "transmitting" wire, and explain the process by which, on the starting of the instrument, this picks up automatically the message, and, after transmission, resumes its continuity as a through metallic circuit in relation to the other instruments on the line.

It will be seen, by reference to the diagram, that until the moment that the instrument is thrown on to the circuit of the "starting" wire by the raising of the weight, W, the through continuity of the "transmitting" wire is maintained as indicated by the arrows ⋙⟶, and that the instrument is completely cut out of the circuit. Now the act of starting the drum, D, by the action of the "starting" current, sets in motion, by the force of the descending weight, the small train of wheels before mentioned, the use of which is to give motion to the disc, T, bearing the code message upon its circumference. This disc makes exactly one revolution in the interval of time occupied by the falling of the weight, the distance between the springs, S S′, and the circuit contact, *a b*, upon the restoration of which, the drum, D, is again locked by the detent, E, and the message disc has assumed its normal position.

We will now trace the action of this message disc. The through continuity of the transmitting wire, when the disc is at rest, is maintained by the pressure of the lever, L, upon the two circuit springs, F. This pressure is exerted so long as the disc is in position by means of the insulated stud, *f*, upon which the lower end of the lever, L, rests. The moment, however, that the disc, T, revolves, the lever, L, falls back upon the pin, *r*, the contact between the springs, F, is broken, and the transmitting wire is thrown to earth, through the disc. As the end of the lever, L, comes into metallic contact with the "make" and "break" prominences upon its circumference, currents flow to the central station along the circuit in the direction indicated by the dotted arrows. Now as these currents are passed in groups to represent letters and words, a code message, or any code message out of a given number, may be automatically printed at the receiving instrument at the central station. In the example given, the currents passed are the Morse signals, indicating the position of the calling station, namely, MAIN ST., GORBALS, which would at once inform the central station of the exact locality of the fire. The weight once more at rest, the transmitting wire is no longer to earth at that call station, but is again thrown into circuit by the contact of the springs, F.

From the explanations given, several very important results have been established. Every instrument while transmitting a code message is for the moment made a terminal station, all other instruments on the same circuit being thrown off so as to avoid all possibility of interference. At no time is any greater resistance thrown into the circuit than that of the single instrument employed to transmit the code message. Again, only a very small amount of battery power at the central station is required, only one instrument being in action at the same time. Whatever number of instruments on a circuit may be called into requisition at the same time, they will all automatically record their messages one after the other in succession, commencing with that nearest to the battery station, and be all automatically started and brought to rest, without any mechanical complication of parts or delicate electrical adjustments. Such results have never before been obtained and at once place the *auto-kinetic* system in advance of every other.

A general description of the apparatus, as placed in the hands of the public will now be comparatively easy to understand, and the stability and simplicity of its construction at once recognised.

A messenger presenting himself before any one of the street pillar "call stations" will, on opening the iron door, find a dial plate on which some eighteen or twenty printed sentences are enamelled in bold characters; first, the name of the street indicating the position of the "call station," as in the example given—"Main street, Gorbals"—and following in rotation others, such as "warehouse on fire," "dwelling-house on fire," "mill on fire," "theatre on fire," "send more assistance," "fire got under," "police constable needed," &c. Opposite to each message will be found a substantial knob or handle, something like the "draw-stop" of an organ. The pulling out of the handle opposite the particular code message required causes the instrument to transmit that message to the central station, where it is printed upon a self-recording Morse receiver; and intimation is given to the inspector on duty of the arrival of a "call" by the ringing of a bell during the time the message is being printed. The description previously given of the interior construction of the instrument will readily explain that the pulling out of the handle simply raises the weight, and that the final transmission of the message is indicated by the return of the handle to its normal position upon the descent of the weight. As any number of code message discs may be mounted upon the same shaft, and as each disc would have its special make and break lever acting upon the circuit springs, F, there is little additional complication in the internal arrangement from a plurality of code signals being introduced, the working parts being mostly common to either one or twenty discs. The advantages above described are not the only features of importance in connection with the auto-kinetic system.

A corporation carrying out such a system as described for street police, and fire "call stations," may derive a considerable annual revenue from introducing special "private fire call" instruments into all the large mills, warehouses, works, and more important private dwellings at a small annual charge to the respective owners. As the number of such "private call" stations introduced upon the circuit is practically without limit, irrespective of the money return to a corporation in a commercial view, the great security to property against any very serious loss by fire is a matter of vast importance. It is well known that the annual losses by fire amount to enormous sums, and often thousands of pounds may be lost by a few minutes' delay in giving early intimation of the outbreak to the brigade.

The auto-kinetic system of using the two wires whereby only one speaking instrument can be in circuit at a time, renders them likewise peculiarly adapted for the employment of the telephone in introducing a social system of communication between offices and works, or for the legal profession between the courts and their various

offices, as all messages of the most private nature can be sent without publicity, an advantage possessed by no other system.

It is unnecessary to point out any other of the many practical applications to which this auto-kinetic system may be applied. It is a system that must shortly extend its social metallic nerves to all the large centres of commerce and manufacture in this kingdom, and its various applications will then become more fully developed and known.

THE RAIN-TREE OF MOYOBAMBA

SOME little while since a paragraph went the round of the papers, describing, on the authority of the United States Consul in the province of Loreto, a tree existing in the forests near Moyobamba, in Northern Peru.

According to the *Madras Times* and *Overland Mail* of December 15, 1877, "The tree is stated to absorb and condense the humidity of the atmosphere with astonishing energy, and it is said that the water may frequently be seen to ooze from the trunk, and fall in rain from its branches in such quantity that the ground beneath is converted into a perfect swamp. The tree is said to possess this property in the highest degree during the summer season principally, when the rivers are low and water is scarce, and the Consul therefore suggests that the tree should be planted in the arid regions of Peru, for the benefit of the farmers there."

As always happens in cases of this kind, there have not been wanting those who have taken this singular story quite seriously, and the India Office has applied to the Royal Gardens, Kew, on behalf of the Agri-Horticultural Society of Madras for information about the tree. It may be interesting to some of the readers of NATURE, and it will certainly save future correspondence, if I explain once for all what I have been able to ascertain as to the origin of the fable and the amount of truth which it contains.

Pœppig's "Reise in Chile und Peru" (2 vols., 1835), which contains much useful botanical information, apparently makes no reference to the subject.

I am indebted to Dr. Francis Darwin for pointing out to me a very similar account which appears in the *Botanische Zeitung*, January 21, 1876, pp. 35, 36, in which Prof. Ernst, of the University of Caracas, records his observations upon a tree of *Pithecolobium* (*Calliandra*) *Saman*, Benth.

"In the month of April the young leaves are still delicate and transparent. During the whole day a fine spray of rain is to be noticed under the tree, even in the driest air, so that the strongly-tinted iron-clay soil is distinctly moist. The phenomenon diminishes with the development of the leaves, and ceases when they are fully grown."

I found that the specimens of this tree in the Kew Herbarium brought its range close to Moyobamba, as they included some gathered by the traveller Spruce, near the neighbouring town of Tarapoto. It appeared probable, therefore, that the Tamia-caspi—the name given in one variant of the story—was *Pithecolobium Saman*, though the cause of the rain was more mysterious than ever. Being vouched for by so competent an observer as Prof. Ernst, its occurrence could not well be denied, while on the other hand, the *Pithecolobium* being a well-known cultivated tree in the West Indian Islands, it was quite clear that if the "raining" from its foliage were a normal occurrence, it would long ago have been put on record.

Mr. Spruce has, however, obligingly supplied me from the astonishing stores of information which he possesses with the true history of the whole matter, and he has also been so good as to allow me to communicate to the readers of NATURE the substance of what he has told me.

"The Tamia-caspi, or rain tree of the Eastern Peruvian Andes, is not a myth, but a fact, although not exactly in the way popular rumour has lately presented it. I did not know there was any doubt as to the true origin of the 'rain.' I first witnessed the phenomenon in September, 1855, when residing at Tarapoto (lat. 6½° S., long. 76° 20′, W.), a town or large village a few days eastward of Moyobamba, and little more than 1,000 feet above the sea-level. I had gone one morning at daybreak, with two assistants, into the adjacent wooded hills to botanise. . . . . A little after seven o'clock, we came under a lowish spreading tree, from which with a perfectly clear sky overhead a smart rain was falling. A glance upwards showed a multitude of cicadas sucking the juices of the tender young branches and leaves, and squirting forth slender streams of limpid fluid. We had barely time to note this when we were assailed by swarms of large black ants, which bit and stung fiercely, and obliged us to beat a retreat, my companions calling out as they ran 'Tamia-Caspi! Tamia-Caspi!' When we had shaken off our assailants, I ventured to approach the spot so near as to make out that the ants were greedily licking up the fluid as it fell. . . .

"My two Peruvians were already familiar with the phenomenon, and they knew very well that almost any tree, when in a state to afford food to the nearly omnivorous cicada, might become (*pro tem.*) a Tamia-caspi, or rain-tree. This particular tree was evidently, from its foliage, an *Acacia*, but as I never saw it in flower or fruit, I cannot say of what species. I came on cicadas, similarly occupied, a few times afterwards, and on trees of very different kinds, but never without the pugnacious ants on the ground beneath. Among the trees on which I have seen cicadas feed, is one closely allied to the acacias, the beautiful *Pithecolobium Saman.* The young branches are very succulent, and they bear elegant bipinnate leaves. . . . The pods are greedily eaten by deer and cattle. Another leguminous tree visited by cicadas is *Andira inermis*, and there are many more of the same and other families which I cannot specify. Perhaps they avoid only such as have poisonous or strongly resinous juices; and those which are permanently tenanted by ferocious ants such as all *Polygoneæ*, the leguminous *Platymiscium*, and a few others. . . . These ants rarely leave the tree which affords them food and shelter, and they jealously repel all intruders, the slightest scratch on the smooth bark sufficing to call their sentinels to the spot. They are quite distinct from the robust marauding ants that drink the cicadas' ejectamenta.

"I have no doubt you have above the true explanation of the Tamia-caspi, or rain-tree. As to the drip from a tree causing a little bog to form underneath and around it, that is a very common circumstance in various parts of the Amazon Valley, in flats and hollows, wherever there is a thin covering of humus, or a non-absorbent sub-soil, and the crown of foliage is so dense as to greatly impede evaporation beneath it. On such sites the Achuál palm (*Mauritia flexuosa*) common enough between Moyobamba and Tarapoto, as well as on the savannahs of the Orinoco, and in subriparial forests of the Amazons—affords a striking example of this property, as has already been remarked by Gumilla, Velasco, Humboldt, and others. Finally, although I never heard the name Tamia-Caspi applied to any particular kind of tree, during a residence of two years in the region where it is now said to be a speciality, it is quite possible that in the space of twenty-one years that have elapsed since I left Eastern Peru, that name may have been given to some tree with a greater drip than ordinary; but I expect the cicada will still be found responsible for 'the moisture pouring from the leaves and branches in an abundant shower'—the same as it was in my time."

Mr. Spruce's notes are so precise and careful that there is little difficulty in accepting his explanation of the rain-tree. It is, however, hard to understand the omission of all insect agency in the equally careful account given by

Prof. Ernst, who attributes the "rain" to secretion from glands on the footstalk of the leaf on which drops of liquid are found, which are rapidly renewed on being removed with blotting paper. It is curious that precisely the same question has been the subject of controversy in the Old World with respect to honey-dew. It is generally believed that this is the result of the aggregate ejecta of Aphides feeding on the juices of the lime. So competent an observer, however, as Boussingault was of opinion that honey-dew was a spontaneous exudation, and it seems not impossible that the lime, as well as the *Pithecolobium Saman* may, under some abnormal circumstances, exude a sugary secretion which insects would eagerly feed on.[1]

W. T. Thiselton Dyer

NOTES

We have to record still another great loss to science in the death on Tuesday, at Rome, of Father Secchi, the eminent astronomer, whose serious illness we recently recorded. We can do no more at present but announce the sad event.

About 355*l.* have been subscribed to the Darwin Memorial Fund, the idea of which, our readers may remember, originated at Cambridge on the occasion of conferring the degree of LL.D. on Mr. Darwin. We would again draw the attention of our readers to the fund; many of them, we are sure, will be glad to contribute to it, and those who desire to do so should lose no time in sending their subscriptions to the treasurer and secretary, Mr. A. G. Dew-Smith, Trinity College, Cambridge.

A Committee of members of the several classes of the French Institute, together with a number of eminent scientific men, has been formed to promote the erection of a monument to Leverrier in the grounds of the Paris Observatory. It is expected that foreigners as well as Frenchmen will subscribe.

Prof. Flower's Hunterian Lectures at the Royal College of Surgeons this year will treat of the Comparative Anatomy of Man, more particularly of the Osteological and other Physical Peculiarities of the Races of Australia and the Pacific Ocean. The first two lectures will be devoted to an exposition of the principal methods of craniological research, exemplified by a series of fifty Australian and as many European skulls. The account of the structure of each race will be preceded by a notice of the principal facts of its history and social condition. The lectures commence on Monday next at 4 o'clock, and will be continued at the same hour on Mondays, Wednesdays, and Fridays, till March 28. Any one interested in the subject is admitted.

The Philosophic Faculty of the University of Zurich has just conferred the degree of Doctor Philos. *honoris causa* on Mr. J. J. Wild, formerly of the scientific staff of H.M.S. *Challenger*, and author of the recent work, "Thalassa," embodying some of the results of that expedition.

The Photographic Society have awarded to Capt. Abney a silver Progress Medal for having made the greatest advance in the science of photography during the past year.

The third general meeting of Polish naturalists and physicians will take place at Cracow this year. The two former meetings were held at Posen and Lemberg respectively.

The Committee of the French Association for the Advancement of Science held a meeting last Thursday. The 16th of August was appointed for the opening of the session, which will be presided over by M. Fremy. The general and sectional meetings will take place at the Hôtel des Beaux Arts, Paris, which contains an immense number of rooms tastefully decorated with fine pictures. The Committee has distributed 8,850 francs among a number of inventors who are constructing machines or scientific apparatus for exhibition. A number of other *encouragements* for similar purposes will be distributed; among the scientific men who will be assisted we are in a position to mention the name of M. Mouchot, for establishing on a large scale his celebrated solar steam-engine.

The annual session of the Deutsche anthropologische Gesellschaft for 1878 begins at Hamburg on August 11. The meetings on the 12th, 13th and 14th take place at Kiel, and those on the 15th and 16th at Lübeck.

In the January session of the Berlin anthropologische Gesellschaft, Prof. G. Fritsch delivered an exhaustive address on the subject of Bushman drawings, in which he compared his own observations in the Cape Colony with the late discoveries of Rev. C. G. Büttner in the neighbourhood of Ameib, in the Damara region. These combined results show the widely extended presence of these drawings in South Africa and the existence of a surprising familiarity with perspective and the principles of grouping. In view of the fact that the Bushmen are probably the most degraded race of mankind now existing, dwelling as they do in caves and living from hand to mouth, these evidences of the first principles of art among them possess no small degree of value as explanatory of numerous attempts at illustration before the stone and bronze ages. This is especially the case with the cave dwellers of the so-called reindeer epoch, whose remains have been uncovered recently in France and Switzerland. Anthropologists have had frequent discussions during the past year with regard to the origin of the sketches of animals in the cave of Thainingen, supposed to date back to this epoch; and the opinion has been stoutly maintained that the human race at this stage of development was utterly unable to produce works of this kind. This view will scarcely be tenable in light of these late discoveries among the Bushmen, who are certainly not advanced beyond the stone-age.

It is expected that the British Archæological Association will hold its annual congress next summer at Wisbeach, to which it has been courteously invited by the Mayor and Corporation. If this arrangement should be definitely made the Prince of Wales will be asked to allow his name to be used as the patron of the congress.

The Russian division in the Paris Exposition will contain a most interesting anthropological collection, the material for which is now being gathered by a Commission in Moscow. Among the more prominent features are an enormous cranial collection from the various parts of the empire, and a model of a Russian barrow. The latter is being executed by the sculptor, Ssewojugin, in natural size, and will offer a perfect imitation of the skeletons, ornaments, weapons, &c., as usually found in these ancient remains. The Russian educational system will be likewise very fully represented, as was the case in 1876.

The official report of the Munich Session of the German Scientific Association, which took place last September, has just appeared. It forms a volume of 264 quarto pages, and has been prepared with unusual care. Reports of all addresses delivered have been furnished by the speakers themselves, who numbered considerably over a hundred. The number of members and participants in the last session was 1,800, of whom 650 were from Munich or its vicinity. We notice that the Society is exceedingly strict in the observance of one of its statutes stating that it shall possess no property with the exception of its archives, for the receipts exactly cover the expenses.

The death is announced of Major-General Sir Andrew Scott Waugh, F.R.S., of the Royal Engineers, at the age of sixty-eight. He entered the Bengal Engineers in 1827, and assisted in the making of the great Trigonometrical Survey of India in 1832. He also took a leading part under Sir George Everest

[1] I have translated Boussingault's paper, and collected the evidence on both sides, in the *Journal* of the Royal Horticultural Society, new series, vol. iv. pp. 1-7.

in the measurement of the great Indian arc for determining the figure and dimensions of the earth. In 1843 he was appointed Surveyor-General of India and Superintendent of the Trigonometrical Survey. He received the honour of knighthood in 1860, and the Gold Medal of the Royal Geographical Society in 1857–58.

MM. HENRY brothers, the celebrated astronomers, have invented a telegraphic warning apparatus, which can be used for telephones. It is powerful, cheap, and simple, and musical sounds emitted can be heard at a distance without placing the ear at the opening of the mouth-piece.

THE *Gardener's Chronicle* announces that M. Thuret's fine garden at Antibes has fortunately become the property of the French nation, and will be constituted a Mediterranean branch, as it were, of the Jardin des Plantes at Paris. The direction will be in the hands of M. Naudin, now of Collioure, who in this new field of action will have greater scope than before for his experiments in naturalisation. The object is to maintain the garden as a botanic and experimental garden, where all new introductions may be tried and distributed to other gardens.

IT is stated that the German poet, Friedrich Bodenstedt, the author of the charming "Lieder des Mirza Schaffy," is now engaged in translating the poems of the Persian philosoper Omer Cheijan. The latter was born at Nishaboor, in the twelfth century, and was one of the greatest astronomers and philosophers of his time. He recorded the results of his studies in verse.

AT a village near the well-known German watering place, Langenschwalbach (in the Prussian province of Nassau) some interesting experiments have been recently made with the common nettle (*Urtica dioica*). They consisted in working this weed in the same manner as hemp; the fibres obtained were fine as silk, while they yielded nothing to hemp fibres as regards durability. A considerable area has now been planted with nettles at the locality named.

SOME highly interesting antiquities were recently found near Wisby, on the Swedish island of Gottland, in the Baltic. Excavations are being made for a new railway, and in a gravel pit, about a foot under the surface, a copper casket was found, which contained two sets of bronze weights, each set consisting of five different pieces, and belonging to an old Arabic monetary system. Besides these weights there was a peculiar magnifying glass in the box, while on the top of all there were found two balance scales, a larger and a smaller balance beam, the former with chains, the latter with flaxen strings, which were still preserved. All the objects were artistically finished and made of bronze.

PROF. LEIDY has been engaged, in connection with Dr. Hayden's expedition during the past season, in exploring the region about Fort Bridger, Uintah Mountains, and the Salt Lake Basin, with special reference to the occurrence there of rhizopods. These have been for several years the special object of Prof. Leidy's attention, and his extensive manuscripts, with many coloured drawings, will probably be published before long.

MR. W. H. HOLMES, the artist of Dr. Hayden's party, has been prosecuting explorations among the Pueblo villages, both ancient and modern, in Northern New Mexico and Arizona, and has collected data for making models in plaster of the pueblos of Taos and Acorna, which will probably be added to the superb series of these archæological restorations deposited by Prof. Hayden in the National Museum.

WE understand that the National Entomological Exhibition, which will be opened at the Royal Aquarium, Westminster, on March 9 is likely to be a great success. Already several thousand square feet of space have been applied for.

IT is surprising to hear that M. Ruhmkorf's workshop has been sold by auction at the ridiculous price of 42*l.*

A NOVEL use of the telegraph has lately been adopted by the Norwegian Government. As is well known, the herring fishery forms one of the most important sources of income for the country, the captures being made as the great shoals come from the depths of the sea to deposit their spawn in the Norwegian fiords. It frequently happens that the object of their visit is accomplished, and they return to the ocean before news of their arrival reaches the fishers on distant parts of the coast. This difficulty is now obviated by the construction of a telegraphic line, 200 kilometres in length, composed chiefly of submarine cables, by means of which the fishers along the whole coast are enabled to gather at once on the approach of a shoal to any part ticular fiord. The abundant captures made in this way show the investment in telegraphic wire to have been a most profitable speculation.

BARON VON BIBRA states in the *Journal für praktische Chemie*, that he has been enabled to restore the handwriting in old manuscripts, by washing them with a solution of tannin, and drying at 75° C. He has likewise found that nitro-benzene can be used for the restoration of antique paintings, whether painted on wood or canvas.

A STRANGE little work has just been published at Weimar (Weissbach); its title is "Das Buch der Katzen," its author Herr Gustav Michel. In six letters the author gives an interesting account of the somewhat rich material, treating the same in turn from a scientific, historic, domestic, religious, and mythological point of view.

IN a communication to the American Philosophical Society on the 1st inst., by Mr. A. Wilcocks, of Louisiana, the author describes an interesting observation which he made of a shadow cast by Venus, against a white wall, in a piazza. "The shadow of a hand," he states, "distant twelve feet from the wall, I found perfectly sharp and well defined. And more striking still, the shadow of the twigs of a pecan tree, distant fifty yards, were also sharp. These last shadows were faint, from the effect of the diffused light of the sky which illumined the wall."

WE take the following interesting statistical data from the *Jahresbericht* for 1877 on the establishments of the world-known firm of Krupp at Essen, Rhenish Prussia. The number of workmen in the cast-steel works amounts to 8,500. There are 298 steam-engines with separate boilers in the establishment, and the total of their horse-power amounts to 11,000. Besides these there are 77 steam hammers at work varying in weight from 2 cwt. to 50 tons. The products in every 24 hours amount to about 12 English miles of rails with tyres, axles, wheels, springs in proportion, as well as 1,500 shells of various sizes and constructions. In one month 300 guns (of various bores) are produced. Since 1847 no less than 15,000 cannon have been made. The daily consumption of coal and coke is 1,800 tons. There are 21,000 gas flames on the works. A railway of 60 kilometres length, with [24 locomotive engines, and 700 carriages exclusively belongs to the establishment; there are also 44 different telegraph stations, and a fire brigade with 8 engines. A new shooting ground of 18 kilometres length is now being adapted near Meppen (Hanover). In the coal and other mines belonging to the firm there are 5,300 workmen. Their mines in northern Spain produce 200,000 tons of iron ore annually; 5 steamers belonging to the firm convey these ores to their destination. The metallurgical establishment contains 700 workmen. In 3,277 workmen's dwelling-houses built by the firm there live 16,200 men, women, and children. They are supplied with provisions, &c., at 22 stores at wholesale prices. The bakehouse produces about 195 tons of bread per day. Last, but not least, there are 4 general schools with 21 classes, and an industrial school for girls and women on the establishment.

Lord John Manners stated in the House of Commons on Thursday last that experiments have been made by officers of the Post-Office with the telephone, the result being that the instrument is not at present considered suitable for public telegraphy.

In Prof. Lebour's letter on Marine Fossils in the Gannister Beds of Northumberland, in last week's Nature, the word *country* should have been *county*. It is the first time marine forms have been found in this series in Northumberland.

The additions to the Zoological Society's Gardens during the pas week include two Black-winged Pea-Fowls (*Pavo nigripennis*) from Cochin China, presented by the Hon. A. S. G. Canning, F.Z.S.; a Javan Parrakeet (*Palæornis javanica*) from Muttra, North-West India, presented by Mr. Barthorp; two Red-vented Bulbuls (*Pycnonotus hæmorrhous*) from India, presented by Col. A. L. Annerley, F.Z.S.; two Leopards (*Felis pardus*) from Persia, deposited; two Barbary Wild Sheep (*Ovis tragelaphus*) from North Africa; two Pale-headed Parrakeets (*Platycercus pallidiceps*) from North-East Australia; four Turquoisine Parrakeets (*Euphema pulchella*) from New South Wales, purchased; two Tigers (*Felis tigris*), born in the Gardens.

ON COMPASS ADJUSTMENT IN IRON SHIPS [1]

II.

An important objection was made to me some years ago by Capt. Evans against the use of quadrantal correctors in the Navy, that they would prevent the taking of bearings by the prismatic azimuth arrangement, which forms part of the Admiralty standard compass. The azimuth mirror (Fig. 5) applied to the compass before you was designed to obviate that objection. Its use even for taking bearings of objects on the horizon is not interfered with by the globes constituting the quadrantal correctors, even if their highest points rise as high as five inches above the glass of the compass-bowl. It is founded on the principal of the camera lucida. The observer when taking a bearing turns the instrument round its vertical axis until the mirror and lens are fairly opposite to the object. He then looks through the lens at the degree divisions of the compass-card, and turns the mirror round its horizontal axis till he brings the image of the object to fall on the card. He then reads directly on the card the compass bearing of the object. Besides fulfilling the purpose for which it was originally designed, to allow bearings to be taken without impediment from the quadrantal correctors, the azimuth mirror has a great advantage in not requiring any adjustment of the instrument, such as that by which, in the prism compass the hair is brought to exactly cover the object. The focal length of the lens in the azimuth mirror is about 12 per cent. longer than the radius of the circle of the compass-card, and thus, by an elementary optical principle, it follows that two objects a degree asunder on the horizon will, by their images seen in the azimuth mirror, cover a space of 1°·12 of the divided circle of the compass-card seen through the lens. Hence, turning the azimuth instrument round its vertical axis through one degree will only alter the apparent bearing of an object on the horizon by ·12°. Thus it is not necessary to adjust it exactly to the direct position for the bearing of any particular object. If it be designedly put even as much as 4° awry on either side of the direct position, the error on the bearing would hardly amount to half a degree. If the instrument were to be used solely for taking bearings of objects on the horizon, the focal length of the lens should be made exactly equal to the radius of the circle, and thus even the small error of ·12° in the bearing for one degree of error in the setting would be avoided. But one of the most important uses of the azimuth instrument at sea is to correct the compass by bearings of sun or stars at altitudes of from 0° to 50° or 60° above the horizon. The actual focal length is chosen to suit an altitude of 27°, or thereabouts (this being the angle whose natural secant is 1·12). Thus if two objects whose altitudes are 27°, or thereabouts, and difference of azimuths 1°, are taken simultaneously in the azimuth mirror, their difference of bearings will be shown as one degree by the divided circle of the compass-card seen through the lens. Hence for taking the azimuth of star or sun at an altitude of 27°, or thereabouts, no setting of the azimuth mirror by turning round the vertical axis is necessary, except just to bring the object into the field of view, when its bearing will immediately be seen accurately shown on the divided circle of the compass-card. This is a very valuable quality for use in rough weather at sea, or when there are flying clouds which just allow a glimpse of the object, whether sun or star, to be caught, without allowing time to perform an adjustment, such as that of bringing the hair, or rather the estimated middle of the space traversed by the hair in the rolling of the ship, to coincide with the object. The same degree of error as on the horizon, but in the opposite direction, is produced by imperfect setting round the vertical axis in taking the bearing of an object at an elevation of 38°.

Thus for objects from the horizon up to 38° of altitude the error in the bearing is less than 12 per cent. of the error of the setting. For objects at a higher elevation than 38° the error rapidly increases; but even at 60° altitude the error on the bearing is a little less than half the error of the setting; and it is always easy, if desired, to make the error of the setting less than

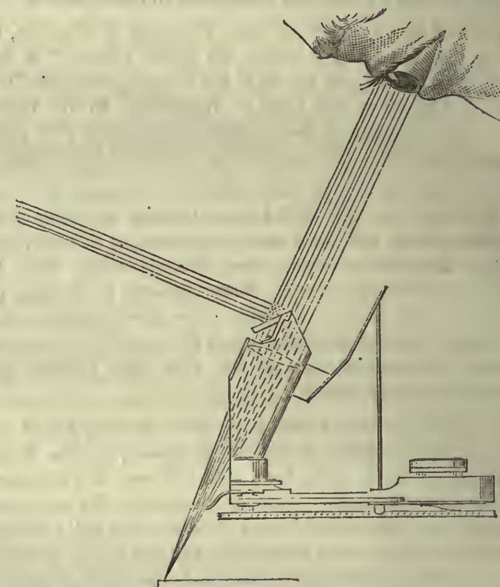


Fig. 5.

a degree by turning the instrument so that the red point, which you see below the lens, shall point within a degree of the position marked on the circle of the compass-card by the image of the object.

For taking star azimuths the azimuth mirror has the great advantage over the prism compass, with its then invisible hair, that the image of the object is thrown directly on the illuminated scale of the compass-card. The degree of illumination may be made less or more, according to faintness or brilliance of the object, by holding a binnacle lamp in the hand at a greater or less distance, and letting its light shine on the portion of the compass-card circle seen through the lens. Indeed, with the azimuth mirror it is easier to take the bearing of a moderately bright star by night than of the sun by day: the star is seen as a fine point on the degree division, or between two, and it is easy to read of its position instantly by estimation to the tenth of a degree. The easiest, as well as the most accurate of all, however, is the sun when bright enough and high enough above the horizon to give a good shadow on the compass-card. For this purpose is the stout silk thread which you see, attached to the framework of the azimuth mirror in such a position that when the instrument is properly placed on the glass of the compass-bowl, the thread is perpendicular to the glass and through the central bearing-point of the compass.

Another advantage of the azimuth mirror particularly important for taking bearings at sea when there is much motion, is

[1] Report of paper read to the Royal United Service Institution, February 4, by Sir Wm. Thomson, LL.D., F.R.S., P.R.S.E., Professor of Natural Philosophy in the University of Glasgow, and Fellow of St. Peter's College, Cambridge. Revised by the Author. [The Council of the R.U.S.I. have kindly permitted us to publish Sir W. Thomson's paper in advance, and have granted us the use of the illustrations.—Ed.] Continued from p. 334.

that with it it is not necessary to look through a small aperture in an instrument moving with the compass-bowl, as in the ordinary prism compass, or in the original nautical azimuth compass (described 280 years ago by Gilbert, Physician in Ordinary to Queen Elizabeth, in his great Latin book. "On the Magnet and on the Earth a great Magnet"), which is very much the same as that still in use in many of the best merchant steamers. In using the azimuth mirror the eye may be placed at any distance, of from an inch or two to two or three feet, from the compass, according to convenience, and in any position, and may be moved about freely through a considerable range on either side of the line of direct vision through the lens, without at all disturbing the accuracy of the observation. This last condition is secured by the lens being fixed in such a position of the instrument that the divided circle of the compass-card is in its principal focus. Thus the virtual image of the divided circle is at an infinite distance, and the images of distant objects seen coincidently with it by reflection in the plane mirror show no shifting on it, that is to say, no parallax, when the eye is moved from the central line to either side. From the geometrical and optical principles explained previously, it follows also that if the azimuth instrument be used for taking the bearing of an object whose altitude is less than 27°, then the effect of turning the frame carrying the lens and mirror in either direction will seem to carry the object in the same direction relatively to the degrees of the card; or in the contrary direction if the altitude exceeds 27°. But if the altitude of the object be just 27°, then the azimuth instrument may be turned through many degrees on either side of the compass-card, without sensibly altering the apparent positions of the objects on the degree-divisions.

II.—*An Adjustable Deflector for completely determining the Compass Error when Sights of Heavenly Bodies or Compass Marks on Shore are not available.*

About thirty years ago Sir Edward Sabine gave a method in which, by aid of deflecting magnets properly placed on projecting arms attached to the prism circle of the Admiralty standard compass, a partial determination of the error of the compass could be performed at any time, whether at sea or in harbour, without the aid of sights of heavenly bodies or compass marks on shore. The adjustable magnetic deflector before you is designed for carrying out in practice Sabine's method more rapidly and more accurately, and for extending it, by aid of Archibald Smith's theory, to the complete determination of the compass error, with the exception of the constant term "A" of the Admiralty notation, which in almost every practical case is zero, and can only have a sensible value in virtue of some very marked want of symmetry of the iron work in the neighbourhood of the compass.[1] When it exists it can easily be determined once for all and allowed for as if it were an index error of the compass card, and it will, therefore, to avoid circumlocutions in the statements which follow, be either supposed to be zero or allowed for as index error.

Fig 6.

[1] I had a curious case lately of the effect of unsymmetrical iron on a midship steering compass, due to a steam-launch about thirty feet long placed fore-and-aft on the port side of the deck with its bow forward and its stern five or six feet before the thwart-ship line through the position of the compass. Adjustment having been performed by means of the globes and magnetic correctors to correct the quadrantal error (D), and the semicircular error, it was found (as was expected) that the compass was correct on the east and west points, but showed equal westerly errors of about 3½° on the north and south points. There were, therefore, approximately equal negative values of "A" and "E" each 1¾°. The captain was, of course, warned of the change he would find when he was relieved of the steam-launch at Rangoon, the port of his destination. The explanation of the westerly deviation when the ship's head was north or south, by the inductive magnetism of the steam-launch, according to which its stern would be a true north pole when the ship is on the north course, and a true south pole when the ship is on the south course, is obvious from the annexed diagram, in which the letters *n*, *s*, denote true north pole and true south pole of induced magnetism in he steam-launch when the ship's head is north magnetic.

The new method is founded on the following four principles:—

1. If the directive force on the compass needles be constant on all courses of the ship, the compass is correct on all courses.

2. If the directive force be equal on five different courses, it will be equal on all courses.

3. Supposing the compass to be so nearly correct or to have been so far approximately adjusted, that there is not more than eight or ten degrees of error on any course, let the directive forces be measured on two opposite courses. If these forces are equal the compass is free from semicircular error on the two courses at right angles to those on which the forces were measured; if they are unequal there is a semicircular error on the courses at right angles to those on which the forces were measured, amounting to the same fraction of the radian (57·3°) that the difference of the measured forces is of their sum.

4. The difference of the sums of the directive forces on opposite courses in two lines at right angles to one another, divided by the sum of the four forces, is equal to the proportion which the quadrantal error, on the courses 45° from those on which the observations were made, bears to 57·3°.

The deflector may be used either under way or in swinging the ship at buoys. The whole process of correcting the compass by it is performed with the greatest ease and rapidity when under way with sea room enough to steer steadily on each course for a few minutes, and to turn rapidly from one course to another. For each operation the ship must be kept on one course for three or four minutes, if under way, by steering by aid of an auxiliary compass, otherwise by hawsers in the usual manner if swinging at buoys, or by means of steam-tugs. A variation of two or three degrees in the course during the operation will not make a third of a degree of error in the result as regards the final correction of the compass. The deflector reading is to be taken according to the detailed directions in sections 14 and 15 of the printed "Instructions." This reading may be taken direct on the small straight scale in the lower part of the instrument. The divided micrometer circle at the top is scarcely needed, as it is easy to estimate the direct reading on the straight scale to a tenth of a division, which is far more than accurate enough for all practical purposes. This reading with a proper constant added gives, in each case, the number measuring in arbitrary units the magnitude of the direct force on the compass for the particular course of the ship on which the observation is made.

The adjustment by aid of the deflector is quite as accurate as it can be by aid of compass marks or sights of sun or stars, though on a clear day at any time when the sun's altitude is less than 40°, or on any clear night, the adjuster will of course take advantage of sights of sun or stars, whether he helps himself also with the deflector or not.

III.—*New Form of Marine Dipping Needle for facilitating the Correction of the Heeling Error.*

This instrument is designed as a substitute for the vibrating vertical needle, hitherto in use for carrying out the observations of vertical force, whether on board ship or on shore, required for performing the operations described in Part iii. Section 4, and ths last three pages of Part iv. of the Admiralty Manual. It consists of a light bar-magnet or "needle" of hardened steel wire, supported by means of a very small aluminium cradle on a stretched platinum wire, of which the two straight parts on the two sides of the needle are, as nearly as may be, in a line through its centre of gravity. One flat end of the needle is painted white, with a black line through its middle parallel to the platinum wire. When the instrument is properly placed for use the platinum wire is horizontal, and the needle is brought into a horizontal position by turning one end of the platinum wire until the elastic force of the torsion balances the turning motive (or "couple") due to the vertical component of the magnetic force of the locality. A divided circle is used (as the torsion head of the original Coulomb's Torsion Balance) to measure the degrees of torsion to which, according to Coulomb's original discovery, the turning motive is proportional. Thus, the magnetic moment of the needle being constant, the vertical component of the magnetic force in the locality of the observation is measured simply in degrees or divisions of the torsion head. A glass plate, fixed in a vertical position parallel to the platinum wire and close to the painted end of the needle, has a horizontal score across it on the

level of the platinum wire. By aid of a totally reflecting prism, like that of the prismatic azimuth compass, with one side convex, the user of the instrument looking downwards sees when the black line on the end of the needle is exactly level with the score on the glass plate. This mode of sighting has proved very satisfactory; it is very easily and quickly used, and it is so sensitive that with the dimensions and magnetic power of the instrument before you it shows easily a variation of vertical force amounting to $\frac{1}{100}$ of the earth's vertical force in this locality. The accompanying printed instructions for the adjustment of my compass describe in sufficient detail the way of using it for correcting the heeling error.

In the instrument before you there is a divided paper circle in the bottom of the box to serve as a "dumb card," to be used with the azimuth mirror when there may be occasion for the use of a non-magnetic azimuth instrument. This appliance has nothing to do with the dipping needle, and is introduced because, while adding little or nothing to the cumbrousness of the instrument, it saves the adjuster the necessity for carrying a separate azimuth instrument with him.

(*To be continued.*)

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—From the University Calendar for 1878 we learn that the Undergraduates, who were last year 2,590, have now risen to 2,659, while the members of Convocation have increased from 4,870 to 5,026. During the year 320 have taken the degree of Master of Arts, and 446 that of Bachelor of Arts. The number of matriculations, which in 1868 was 579, and which in 1876 was 650, rose in 1877 to 770. But this increase was due to the number of candidates for a musical degree. The list of members of Congregation—that is, of the legislative body of resident members of Convocation—has increased, but only slightly. In 1876 they numbered 314; in 1877, 322. But the proportion between clergymen and laymen has considerably changed during the year. In 1876 there were 180 clergymen and 134 laymen; in 1877 the laymen have risen to 154, and the clergymen have fallen to 168. Of the whole body of Fellows (exclusive of Christ Church), resident and non-resident, there are at present 192 laymen and 116 clergymen.

CAMBRIDGE.—The Council of the Senate having had under consideration a letter from Prof. Hughes, Woodwardian Professor of Geology, representing the need for additional assistance, propose that an assistant be appointed, with a stipend of 200*l*. per annum, whose duties shall be to assist the Professor in the arrangement and care of the geological collections, to give such instruction and demonstrations as may be required, and to assist students making use of the museum. It is proposed to vest the appointment in the Professor, with the consent of the Vice-Chancellor.

EDINBURGH.—A site has been secured in Chambers Street, close by the University, for the erection of a new school of medicine for extra-academical teachers, on the spot formerly occupied by Minto House, so long the scene of the demonstrations and prelections of eminent extra-mural lecturers.

TAUNTON COLLEGE SCHOOL.—A microscopic cabinet by Smith and Beck, with other valuable apparatus, has been presented to the Rev. W. Tuckwell by his late assistant-masters at the Taunton College School, as an expression of their personal sympathy and their recognition of the services rendered by him to the higher education.

PRUSSIA.—January 20 was a red letter day for a number of professors in Prussian universities, no less than fifteen receiving orders of different ranks from the Emperor William.

DRESDEN.—On May 1 the Royal Polytechnic Institution at Dresden will celebrate the fiftieth anniversary of its foundation. Originally confined to the narrowest limits, the Institution has rapidly developed, and is now one of the most frequented polytechnic schools of Germany.

GREIFSWALD.—The attendance on the university shows a decrease as compared with the past summer. The students number 43 in the theological faculty, 73 in the legal, 126 in the philosophical, and 218 in the medical. The corps of professors and privat-docenten is at present 60. A library of 60,000 volumes, well equipped laboratories and collections, and ample revenues place Greifswald on a par with most German universities, but for a number of years it has failed singularly to compete in point of attendance with many poorer centres of study.

TÜBINGEN.—The university shows at present the highest winter attendance since its foundation. The students are divided as follows: Theology (evangelical), 215, (catholic), 108; law, 256; natural sciences and medicine, 222; philosophy, 145.

MÜNSTER.—Prof. R. Sturm, of the Darmstadt Polytechnic, has been appointed to the chair of mathematics, rendered vacant by the late death of Prof. Heis. The number of students at present is 312.

BERLIN.—Prof. Schwedener, of Tübingen, has received a call to Berlin to fill the second professorship for Botany lately created at the University.

VIENNA.—In the lately presented educational budget of Austria the sum of 50,000*l*. is appropriated for the erection of new buildings for the Vienna University.

DORPAT.—The hitherto rigorous rule of Russian universities requiring from all instructors the possession of Russian diplomas of the doctorate, &c., has been modified in the case of Dorpat, recognition being made of foreign degrees and professorial positions.

SIBERIA.—The Imperial Commission appointed to settle the long-debated question as to the University of Siberia, has definitively given the preference to Tomsk, against Omsk. We are glad to learn this result, because of the central position of Tomsk, its larger population, not exclusively administrative, as at Omsk, and the larger number of secondary schools. Several Siberian merchants have endowed the future University with considerable sums of money.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, January 31.—"On the Expression of the Product of any Two Legendre's Coefficients by means of a Series of Legendre's Coefficients," by Prof. J. C. Adams, F.R.S.

Royal Society, February 24.—"On the Use of the Reflection Grating in Eclipse Photography," by J. Norman Lockyer, F.R.S.

The results obtained by the Eclipse Expedition to Siam have led me to think that, possibly, the method of using the coronal atmosphere as a circular slit, suggested by Prof. Young and myself, for the Indian eclipse of 1871, might be applied under very favourable conditions, if the prism or train of prisms hitherto employed were replaced by one of those reflection gratings with which the generosity of Mr. Rutherfurd has endowed so many of our observers.

To test this notion I have made some experiments with a grating, which I owe to Mr. Rutherfurd's kindness, containing 17,280 lines to the inch. The results of these observations I have now the honour of laying before the Royal Society.

In front of the lens of an ordinary electric lamp, which lens was adjusted to throw a parallel beam, I have introduced a circular aperture, cut in cardboard, forming an almost complete ring, some two inches in interior diameter, the breadth of the ring being about $\frac{1}{8}$ inch. This was my artificial eclipse.

At a distance from the lamp of about thirteen yards, I mounted a $3\frac{3}{4}$ inch Cooke telescope, of fifty-four inches focal length. Some distance short of this focus I placed Mr. Rutherfurd's grating, and, where the first order spectrum fell, I placed a focussing screen. To adjust for sharp focus, in the first instance, the grating was so inclined to the axis of the telescope that the image of the ring reflected by the silver surface adjacent to the grating was thrown on to the screen. This done, the grating was placed at right angles to the axis, and the spectrum of the circular slit, illuminated by sodium vapour and carbon vapour, photographed for the first, second, and third orders on one side. The third order spectrum, showing the exquisite rings due to the carbon vapour flutings was produced in forty-two seconds. The first order spectrum, also submitted to the Society, was produced in the same period of time, and was very much over-exposed; it is, therefore, I think not expecting too much that we should be able to take a photograph of the eclipse, in the third order, in two minutes; but let us make it four. Similarly, we may hope for a photograph of the second order in two minutes, and it is, I

think, highly probable also that a photograph of the first order may be obtained in one minute.

It is clear then that, by mounting photographic plates on both sides of the axis, one solidly mounted equatorial of short focal length may enable us to obtain a large number, with varying lengths of exposure, of the next eclipse. I have insisted upon the solidity of the mounting because, if any one plate is to be exposed during the whole of totality, the instrument must not be violently disturbed or shaken while the eclipse is going on. I think, however, it is quite possible to obtain many photographs, of the lower order spectra, without any such disturbance. The same plate may be made to record three, or even four, exposures in the case of the first order, by merely raising or lowering it after a given time, so that a fresh portion of the same plate may be exposed, by means of a rapid screw or other equivalent contrivance. Similarly, the plates on which the spectra of the second order are to be recorded may be made to perform double duty.

Linnean Society, February 7.—Prof. Allman, F.R.S., president, in the chair.—Sir John Lubbock, Bart., read a paper, "Observations on the Habits of Ants," being his fifth contribution on this subject. In continuation of former experiments he finds that ants recognise old acquaintances and attack strangers. Their intelligence is questionable in cases where a thin circle of glycerine bars their access to honey which they have already visited by a paper bridge, for when the latter is taken away they do not pile up a few grains of earth and thus cross the barrier. Spite of the many observers and plentifulness of ants' nests, it is still doubtful how their nests commence. Sir John's experiments show that the workers of *Lasias flavus* will not adopt an old queen from another nest. But on the other hand, the queen of *Myrmica ruginodis* has the instinct of bringing up larvæ and the power of founding communities. As to intimating to each other discovery of food, he considers this does not necessarily imply any power of describing localities, but rather by a simple sign co-workers accompany each other to the treasure. They do not summon their brotherhood by sounds to a repast found by one or another. Their affection for friends is outbalanced by hatred of strangers. A few of each kept prisoners in separate bottles with wide meshed muslin over the mouths, those free outside again and again excitedly endeavoured to attack the latter, but used no means to free the former, their own companions. Further experiments prove scent more than sight guides them in following up food which has been shifted in position after its having been partaken of, and a return to the nest made. Ants avoid light when thrown into their nests, and they then congregate into the darkest corners. Taking advantage of this habit by a series of ingenious experiments—wherein strips of coloured glass, in other instances shallow cells containing coloured solutions, such as fuchsine, bichromate of potash, chloride of copper, &c., were used—Sir John arrives at the conclusion that they are influenced by the sensation of colour, though probably different from the effect produced in man. A predominate preference is given to red, green follows, yellow comes next, while to blue and violet there appears to be a decided aversion. The longevity of ants would seem greater than generally admitted, some specimens of *Formica fusca* being at least five years old.—Mr. Thiselton Dyer made a brief communication on the so-called "rain-tree" of Mogobamba, South America, an account of which we give elsewhere.—Then followed a paper "On the shell of the Bryozoa," by Mr. Arthur W. Waters. The points he more particularly drew attention to were:—The great difference of the young and old cells caused by a constant growth of shell-substance, so that the older zooecia become closed up. This growth progresses at various rates. Passing through the shell are tubes filled with corpuscles of the chylaqueous fluid, which thus become oxidised. The supposed nervous filament of the colonial connection the author believes to be rather for the supply of material from one part of the zoarium to another. He further suggests that the varying thickness of the plates in the walls of the colonial connection should be used as a factor in specific determination, and especially would it be useful in comparing recent and fossil forms. There is a possibility of the avicularia and adventitious tubes being homologous, and helping to maintain the vitality of the colony when the polypides have disappeared.—Messrs. A. G. Agar and C. Berjeau were elected Fellows of the Society.—The President having put the motion, it was unanimously resolved to present an address to Prof. C. T. Ernst von Siebold on his approaching jubilee.

Zoological Society, February 5.—Prof. Mivart, F.R.S., vice-president, in the chair.—Prof. Mivart read a paper entitled "Notes on the Fins of Elasmobranchs, with Considerations on the Nature and Homologies of Vertebrate Limbs," wherein the author detailed his dissections of the fins of Elasmobranchs, which dissections had convinced him that the paired and azygos fins are of similar nature. He represented them all to have resulted from the centripetal growth and coalescence of a primitively distinct series of cartilaginous rays developed in longitudinal folds, of which one was dorsal, one ventral, and two were lateral. He also advocated the view that the limb-girdles result from the further centripetal growth of the coalescing limb-cartilages, which growth seeks a *point d'appui*, the pectoral limb-girdles in fishes shooting upwards and downwards, as well as inwards to obtain a firm support, and, at the same time, to avoid the visceral cavity. He contended that the Archipterygium was not to be sought for in *Ceratodus*, which he by no means regarded as a primitive type of structure, but rather in *Raia* and especially in the ventrals of *Polyodon*. He objected to Gegenbauer's view that the metapterygium formed the limb axis of the cheiropterygium, advocating instead the propterygium, or, if not that, then the mesopterygium. He cited the varying conditions described as evidences of the presence of an innate intra-organic polar force as the main agent in morphological modifications.—A communication was read from Mr. W. A. Forbes, F.Z.S., containing an account of the birds collected by the *Challenger* Expedition at Cape York and on the neighbouring islands. The collection consisted of sixty-one skins referable to thirty-eight species, all, or nearly all, of which belonged to well-known Australian forms, one or two only being uncertain on account of the immature condition of the specimens.—A communication was read from Mr. Francis Nicholson, F.Z.S., in which he gave an account of a small collection of birds made in the neighbourhood of Abeokuta, West Africa. Amongst these was a new species of Finch which was proposed to be called *Amadina sharpei*.—The Rev. S. J. Whitmee, C.M.Z.S., read a paper on the mode of the modifications of anger, fear, &c., in fishes, and on the use of their spines, as observed by him during his residence in the Samoan Islands.—Messrs. P. L. Sclater and O. Salvin gave an account of the collection of birds made by Prof. Steere during his recent journey across South America, from Para to Callao. The 911 specimens obtained were stated to be referable to 362 species, of which five were described as apparently new to science, and proposed to be called *Oryzoborus atrirostris*, *Myiarchus semirufus*, *Furnarius pileatus*, *Capito steerii*, and *Crypturus transfasciatus*.—Prof. Garrod read a note on the anatomy of the Binturong, *Artictis binturong*, and the fourth portion of his series of notes on the anatomy of passerine birds.—Mr. Howard Saunders, F.Z.S., read a paper on the subfamily of the *Larinæ*, or Gulls, being a monographical revision of the group, which he considered to consist of the genera *Pagophila*, *Rissa*, *Larus*, *Rhodostethia*, and *Xema*, containing altogether forty-nine species. With regard to *Pagophila*, he drew attention to a structural peculiarity which appeared to have been previously unnoticed, *i.e.*, the junction of hallux to the inner toe by a serrated membrane. Mr. Saunders also remarked upon the occasional presence of a small but well-developed hind toe and claw in individuals of the Kittiwake (*Rissa tridactyla*) from Alaska.—A communication was read from Mr. Martin Jacoby, containing descriptions of some new species of phytophagous coleoptera.—Two communications were read from Lieut.-Col. R. H. Beddome, C.M.Z.S. The first gave a description of a new form in the family of Tree-agames from the higher ranges of the Anamallays, proposed to be named *Lophosarea anamallayana*. The second contained the descriptions of some new species of *Uropeltidæ*, from Southern India.

Anthropological Institute, February 12.—Mr. John Evans, D.C.L., F.R.S., president, in the chair.—Mr. H. C. Sorby, F.R.S., read a paper on the various colouring matter met with in human hair. In this paper the author described the manner in which the various coloured substances met with in human hair may be separated and distinguished. Four quite different and well characterised pigments have been obtained, but of these two serve to modify the tints of hair to only a very limited extent. The general colour is mainly due to a black and a brown-red pigment, both of which can be easily obtained in a separate form, and used like water colours, as shown by the numerous drawings which were exhibited. All the varying tints of black, brown, dark and lighter red, and most of the pale tints are easily proved

to be due to a variation in the total and relative amounts of these two substances as shown by a series of comparative analyses. The paper concluded with some remarks on the bearing of these facts on ethnology, and with a consideration of the probable explanation of certain changes in the colour of hair occasionally met with, but not yet fully understood.—The Director then read a paper by the Hon. Chas. C. Jones, jun., on bird-shaped mounds in Putnam County, Georgia.

Meteorological Society, February 20.—Mr. C. Greaves, F.G.S., president, in the chair.—Dr. Tripe read a paper on the winter climate of some English sea-side health resorts. The places selected were Scilly, Torquay, Penzance, Guernsey, Barnstaple, Ventnor, Llandudno, Ramsgate, and Hastings, and the climatic features of each were compared with those of London. The results of this discussion may be briefly summed up as follows, viz. :—The mean daily winter temperature of these seaside places, and especially of those situated on the coast of Devon and Scilly, is higher than at London; the mean daily maxima and minima are also higher, and especially the latter; so that the daily and monthly ranges of temperature are smaller; the mean humidity is less, the general direction of the wind about the same, but the number of rainy days and the rainfall are greater at the sea-side. As regards the wind, therefore, the chief point to be especially noticed is the amount of shelter afforded by high land, as at Ventnor, and especially of protection against the stormy and cold winds which ordinarily prevail at the end of February and in March. The soil also should be considered, as heavy rains at gravelly and chalky places are not so objectionable as on clayey ground.—The discussion on this paper was adjourned until the next meeting, which will be held on March 20.—The following were elected Fellows of the Society:—W. C. Baker, W. Berridge, W. M. Burke, Rev. J. A. L. Campbell, Prof. J. Eliot, Lieut. C. S. F. Fagan, C. H. Holden, Prof. H. J. S. Smith, Capt. W. Watson, C. Woollett, and Miss E. A. Ormerod.

Institution of Civil Engineers, February 12.—Mr. Bateman, president, in the chair.—The paper read was on the evaporative power of locomotive boilers, by Mr. J. A. Longridge, M. Inst. C.E.

PARIS

Academy of Sciences, February 18.—M. Fizeau in the chair.—The President gave an account of the funeral of M. Claude Bernard on the 16th inst., and the discourses pronounced by MM. Dumas, Mezières, and others. (These are published in the *Comptes Rendus*.)—Meridian observations of small planets at the Greenwich and Paris Observatories during the fourth quarter of 1877, by M. Villarceau.—On some applications of elliptic functions (continued), by M. Hermite.—Experimental researches on the fractures traversing the earth's crust, especially those known as joints and faults (continued), by M. Daubrée. His object is to show that torsion may account for many of those effects.—Tibio-calcanean resection, by M. Sedillot.—Refutation of M. Pasteur's criticism of his opinion as to the origin of alcoholic yeast and lactic yeast, by M. Trécul.—The vibrations of matter and the waves of the ether in vision, by M. Favé.—Remarks on the satellites of Mars, by M. Roche. He considers the first satellite comparable to the inner ring of Saturn in its origin; it owes its existence (as satellite) to being a little more separated from its planet.—On the law of reciprocity for invariants and covariants of binary quantics, by Prof. Sylvester.—On MM. Clebsch and Gordon's theory of associated forms, by the same.—Presence of magnetic spherules similar to those of atmospheric dust, in rocks belonging to ancient geological periods, by MM. Meunier and Tissandier. If the identity be admitted, we must infer that the layers of the earth's crust contain materials of cosmic origin which fell in a very distant epoch (such particles are found in the Devonian series). It is important to determine where they first appear.—On the vibratory forms of solid and liquid bodies, *à propos* of a note by M. Dubois, by M. Decharme. A claim of priority.—Separation of the non-ferruginous elements of rocks, based on their difference of specific gravity, by M. Thoulet. The specific gravity of most of the essential minerals of rocks varying between 2·2 and 3: these may be separated from each other by immersion in solutions which are without chemical action on them, but whose specific gravity is comprised between the same limits. Such are solutions of iodide of mercury in iodide of potassium. (Details of the method are given.)—On the state of phylloxerised vines in the commune of Mezel (Puy-de-Dôme), by M. Truchot.—Theory of Vesta: perturbations dependent on the first power of the perturbing masses, by M. Leveau.—On the special conditions in the contour of plates, by M. Boussinesq.—On the conditions for a quadratic form of n differentials to be transformed so that its coefficients lose a part or the whole of the variables they contain, by M. Levy.—On the summatory formula of Maclaurin and interpolar functions, by M. Genocchi.—On Bell telephones and string telephones, by M. Breguet. By attaching a string telephone (with parchment membrane) to any point of a Bell telephone, one may hear through it a person using a Bell telephone. Several string telephones may thus be connected. A mode of making the string telephone more practicable is described.—On telephony, by M. Salet. A telephone is described in which the movements of the two membranes are absolutely correspondent, the great electric resistance of liquids being utilised for the purpose.—On the ebullition of superposed liquids, by M. Gernez.—Extraction of gallium, by MM. Lecoq de Boisbaudran and Jungfleisch. The authors had obtained 62 grammes of metallic gallium by treating 4,300 kilogrammes of Bensberg blende (the method is described).—Method of volumetric determination of potash, by M. Carnot.—Dissociation of hydrate of chlorine, by M. Isambert. With regard to solubility of chlorine in water, he says that under 9° it is only the hydrate that is formed and dissolved in the water; above this temperature, at ordinary pressure, there is merely a solution of a gas in the water. Air passed through a solution of chlorine under 9° gradually carries off all the chlorine, as if there was solution and not combination.—Action of chloride of benzoyle on leucine, by M. Destrem.—On the identity of muscular inosite and vegetable sugars of the same composition, by MM. Tanret and Villiers.—On the preparation of amylene, by M. Etard.—Experimental researches on the maturation of the grape, by MM. Saint Pierre and Magnien.—On some volatile products of coal-pits set on fire, by M. Mayençon. He examined efflorescences round the fumeroles (the pits were in the Loire valley). The most abundant substances are ammoniacal compounds, arsenic, aluminium, iron, chlorine, and sulphur.—On the conditions of development of ligula, by M. Duchamp.—Sensations of light and colour in direct and indirect vision, by MM. Landolt and Charpentier.—On the geological constitution of the Island of Réunion (first part), by M. Velain. The succession of volcanic phenomena seems to be similar to that at Santorini.—Origin and distribution of limestone in maritime sands, by M. Contejean.—Barometric differences between neighbouring stations according to the direction of the wind (continued) by M. Renou.—On the flash of lightning which caused the burning of the belfry of Toucy (Yonne), on January 25, by M. Roché.

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THURSDAY, MARCH 7, 1878

REPRESENTATION OF SCIENCE AT THE PARIS EXHIBITION

WE are glad to know that the interest shown in the Loan Collection of Scientific Apparatus at South Kensington and the benefit to the nation at large to be derived from such displays have not been lost upon the organisers of the French part of the forthcoming Exhibition.

Among the most energetic and most enlightened of these organisers we must count M. Bardoux himself, the new Minister of Public Instruction, under whose auspices for the first time a well-developed scientific side will form part of an International Exhibition. Culture will be added to industry. Research will have its place, side by side with the applications of science.

The attempts to give prominence to this side of the exhibition on the part of the French are as remarkable as the complete neglect of everything touching science by our own Commission. For them apparently science does not exist, except the science that pays, in the shape of large engines and looms, fine stuffs, machine-made jewellery, and the like. England will have its Burlington Arcade, but not its Burlington House. We give the Commission credit for having "worked" the commercial world well; we only complain that the possibility of there being anything worth exhibiting from the scientific point of view never seems to have occurred to them.

The antithesis we have drawn between the Burlington Arcade and Burlington House well represents the great point of the forthcoming Exhibition. There will be a gigantic shop on the Champ de Mars, there will be a gigantic temple devoted to the pure sciences and to pure art in the Trocadero. The river will separate the source from the application; instruction in science and art from commerce and industry.

Hence it is that M. Bardoux, having already organised on a large scale the representation of the fine arts and public instruction, is now organising what is to be called the "scientific display." This part of the work, important though it be, will be rendered very simple to the Minister, as the matter will be left almost entirely in the hands of the men of science themselves, including, of course, those men of science who direct important branches of the public service as well as individual investigators.

Thus each Government department will show the way in which its scientific work is done. The three new Government observatories in Paris will exhibit either results or methods. There will be a complete collection illustrating the various scientific missions which France has undertaken during the present century, and all the publications, scientific, historic, and artistic, which have been published by the state will be there for all the world to see.

Not only, therefore, will there be a true Loan Collection of Scientific Apparatus, but the example set by the South Kensington Conferences wills also be followed. The enormous building in the Trocadero contains a lecture theatre capable of holding upwards of 4,000 people. This will be used for lectures in scientific and kindred subjects, for which arrangements are now being made. It is impossible that the Trocadero buildings can be ready by May 1, so there will be ample time for these arrangements and for the others, to which we may briefly allude.

The French Association for the Advancement of Science will conduct a large number of scientific experiments on a great scale, and a large number of exhibitors will take advantage of the meeting of that association to exhibit experiments relating to their special pursuits.

Every facility will also be given to scientific societies for summoning to a special congress those prosecuting the same line of research. The number of these useful assemblies is increasing daily. It would occupy too much space to give a list of all the societies which will hold such meetings, but many circulars illustrating the development of this sectional movement have already been printed.

Lecture-rooms will be furnished *gratis*, lectures will be advertised on a large scale, and, as far as possible, Government apparatus will be at the disposal of inventors for conducting the experiments required to illustrate their lectures. What has been done at the provisional Ethnographical Museum, to which we have already referred, may be considered as a fair specimen of what will be done on a larger scale at the Trocadero Palace and other suitable buildings.

It may be said that nothing will be spared to make the Exhibition useful to science and intelligible in its scientific aspects for the largest number of people.

Surely England might have been able to contribute something of interest to this most interesting side of the Exhibition? We surely must, after all, be merely a nation of shopkeepers seeing that our Royal Commissioners have doubted our capabilities in any other direction!

METROLOGY

Inductive Metrology; or, The Recovery of Ancient Measures from the Monuments. By W. M. Flinders Petrie. (London: Saunders, 1877.)

THIS work has a somewhat ambitious title, but it may fairly claim to be written upon a scientific basis, and it bears evidence of much study and laborious research. It is an attempt to carry out generally the method originated by Sir Isaac Newton, in his well-known Dissertation on Cubits, of determining the length of the ancient Egyptian cubit from some of the measured dimensions of the great pyramid. By a similar process the author has endeavoured to determine the ancient standards of linear measure in various countries from the measurements of remaining monuments. No allusions are made to weights and volumes, but only to linear quantities, as these alone are shown by the architectural remains.

In accordance with Whewell's definition of "induction," Mr. Petrie says that "inductive metrology ascertains the 'general truths' of the units of measure in use from the 'particular facts' of those multiples of measures which ancient remains preserve to us." He assumes that in the construction of all such works, if a measure existed, it would be used, and that whole numbers would be used in preference to fractions and round numbers in preference to uneven ones, merely for convenience in the work. We know

this to have been the case with regard to the various dimensions of the tabernacle constructed by Moses, of Solomon's Temple, and the later temple as described by Ezekiel, The length of the Royal Egyptian Cubit was determined by Sir Isaac Newton, from Greaves's measurements in the great pyramid, to have been between 20·62 and 20·78 English inches. Amongst these measurements the so-called King's Chamber was found to be 20 of such cubits in length, and 10 in breadth. The passages were 2 cubits broad. The principal gallery was 4 cubits broad, with a middle way of polished marble 2 cubits broad, and a raised bench on each side 1 cubit broad and 1 cubit high. In Newton's time, no direct evidence of the true length of the ancient Egyptian cubit had been brought to light. We now know from several ancient standard cubit rods since discovered, that the mean length of the royal Egyptian cubit was equal to 20·67 English inches. In this essay Mr. Petrie states that all the deduced units of measure were in every instance found in a similar way independently of any known standard, and have not been obtained by trying whether the measures would fit any known unit. As to standards of measure, they are only employed by the author to subject assumptions to proof, where such can be obtained.

The least number of measurements that suffice to give a unit with tolerable certainty is assumed to be three. Long lengths were found of little value in obtaining the unit, and moderately short lengths from about 2 to 20 feet are stated to be the best. After showing the several modes of ascertaining as nearly as may be the unit of measure from a given number of actual measurements, that is to say, the ratio between them, the process adopted has been to group together those units of any one country and age that seemed to be identical, or derived from and related to one another, and thence to deduce the mean unit. In every case the probable error has been computed and stated. This probable error is assumed to arise from original errors in planning and executing the work, and not in the more recent measurements, as with reasonable caution such errors may be tolerably avoided.

The extent of the work undertaken by the author may be judged of from the statement that more than 600 buildings and other remains have been examined and their constructors' units deduced from the mean results of over 4,000 measurements. A considerable number of them were made by the author, many being of objects in the British Museum. To insure correctness the English measure used by him was verified as to its accuracy at the Standards' Office.

The first series of groups relate to Egyptian architectural remains, generally from the fourth dynasty to the Roman period. The deduced units of the measurements of 101 monuments are stated in English inches and decimal parts of an inch, and the number of independent lengths from which each such unit was obtained is also specified. The deduced units of one of these groups, consisting of twenty-eight different monuments vary only from 20·42 to 20·84 inches, the mean being 20·64, thus agreeing very nearly with the ascertained length of the royal cubit = 20·67 inches. From the remaining monuments the author deduces other units several of which are multiples of the digit, the twenty-eighth part of the royal cubit. The common cubit, or cubit of a man, equal to 18·24 inches, has not yet been found inductively from remaining monuments.

The next series of monuments examined are those of Babylonia and Assyria, Persia and Syria. These countries are classed together as being intermixed in the style of their art and the nature of their architectural remains. The results of the measurements of 102 monuments are given, with various deduced units of measure. The Persian monuments are chiefly those of Persepolis. It may be more interesting to refer to the Syrian monuments as they include those of Judæa and Palestine, and of Moab. As an instance, the mean unit of 25·01 inches (varying from 24·57 to 25·55 inches) is found from six monuments, four of which are at Jerusalem. This is taken to be the mean length of the sacred Jewish cubit. It is to be observed that in his "Dissertation on Cubits," Sir Isaac Newton arrived at the conclusion that the length of this cubit was 24·83 inches. It is now generally considered to have been a little more than 25 inches, and it is supposed to have been the cubit measure taken from Chaldæa by the ancestors of the Jews, and to have continued in use by their posterity in Egypt and Palestine.

The countries that follow are Asia Minor and Greece. The first of these affords eleven different units from eighty-four measured monuments. Eight of these units are known to have been used by nations that ruled there, and the other three are connected with the units of adjacent countries. From Greece and its colony Sicily the results of the measurements of forty-nine objects are given, including Pelasgic and later monuments.

Italy, Africa, and Sardinia are next classed together. The results of the measurements of seventy-seven monuments are shown under the head of Italy, including Roman remains in Britain, Africa, and other countries probably constructed with Italian units of measure.

The mediæval remains in Ireland and England conclude the several classed groups of monuments measured. The measurements of twenty-nine round towers and churches connected with them in Ireland give two deduced units. Out of eighty-one measured old English remains the inch and foot were found to be the units in sixteen cases only, the mean inch unit being equal to 0·9998 of our present standard inch, showing that on the average the inch measure has not varied appreciably for centuries. Several other units of other countries are deduced from the remaining monuments.

The last series of measurements are those of rude stone remains and earthworks in various countries. At first sight it does not appear possible that such objects should lead to units of measure being derived from them; but the results show, in the author's opinion, that the more regularly constructed remains were made by a measure-using people.

After mentioning the results of measurements partly of the dimensions and partly of the relative positions of various ancient stone remains and earthworks in this country and in France, the results of about seventy measurements of the dimensions of ancient North American earthworks are stated to lead to a unit varying from 12·50 to 12·72 inches, with a mean of 12·6 inches, divided duodecimally. The mean unit of twelve Mexican measurements was 10·65 inches.

The author claims, as the chief results of his inductive examination, to have determined from the monuments the true values of the Sacred Hebrew or Royal Persian cubit, the Royal Egyptian cubit, the Egyptian digit, the Assyrian cubit, the ancient Greek foot, the Olympic foot, the Drusian foot, the Plinian foot, and the Pythic foot, together with the probable errors of these determinations.

He claims also to have found that the principal standard units of length were in more extended use than was previously known, and to have indicated the countries in which they were used. And also that he has brought to light many other units of length of which the knowledge had been previously lost.

It is not probable, however, that all persons who have given mature consideration to the contents of the work will concur in the stated results or be altogether satisfied with some of the mean units obtained. In every case a unit deduced from the actual measurements is stated, and not the measurements themselves. But a large proportion of these deduced units are not whole numbers of the mean unit obtained from them. Thus, taking one of the instances most favourable to the author's views, out of twenty-eight Egyptian monuments, from which the mean length of the royal Egyptian cubit is obtained, twelve only of the deduced units are whole numbers, the others being various fractions of the mean unit, and many of them, such as $\frac{1}{5}$, $\frac{1}{6}$, $\frac{1}{8}$, $\frac{1}{10}$, $\frac{1}{12}$, $\frac{2}{5}$, $\frac{5}{8}$, $\frac{1}{50}$, $\frac{4}{50}$ are fractions not marked upon any of the extant standard cubit rods, which are divided only into seven palms and twenty-eight digits.

The essay will be read with much interest and advantage by those persons who have given their attention to metrological science. It appears to be a valuable contribution to historical and ethnological literature, and to be a ground-work for further researches on the subject.

WOLF'S HISTORY OF ASTRONOMY

II.

Geschichte der Astronomie. Von Rudolf Wolf. (München : R. Oldenbourg, 1877.)

IN our former notice of this valuable addition to astronomical literature (NATURE, vol. xvii. p. 259) reference was made to the great amount of information which the author has compressed within a moderate space in the third and last section of his work which treats of "the newer astronomy." We propose here to take a brief survey of the principal contents of this portion of the volume to assist the reader's appreciation of the work.

The third section is subdivided into four chapters—9—12. The first commences, as before stated, with Sir Isaac Newton's discovery of the principle of universal gravitation, the publication of the "Principia," and the first application of the new theory to the orbits of comets by Halley, whose meritorious connection with the publication of Newton's immortal work is well known. This is followed by some account of the foundation of the Observatories of Greenwich and Paris, and soon afterwards of those of Berlin and Copenhagen, whereby so great an impetus was given to practical astronomy; of Richer's expedition to Cayenne for the determination of the solar parallax, from corresponding observations of the planet Mars, and the first ideas as to the applicability of transits of Venus for the solution of the same problem. The labours of the earlier workers in the Newtonian theory—of Bernoulli, Euler, Clairault, and others, are particularised; also Bradley's great discoveries of the aberration of light and the nutation of the earth's axis, together with his work in the field of observation, with the similar work of Tobias Mayer and Lacaille. Further on the same chapter treats of the labours of Lagrange, Laplace, Gauss, and others in theory, and of Herschel, Piazzi, Bessel, Struve, and others, in the practice of astronomy. We have some account of the "Theoria Motus," the "Fundamenta Astronomiæ," amongst classical works, and of progress made in the solar, lunar, and planetary theories, and formation of tables and ephemerides. Amongst the remaining varied contents of this chapter there are notices of the discovery of Neptune, stellar parallax, the connection between solar spots and the earth's magnetism, the application of photography to astronomical purposes, and the introduction of the spectroscope.

Chapter 10 is devoted to astronomical instruments and their uses, after some remarks upon methods of calculation introduced in modern practice. There are brief notices of instruments in their various forms, from the complicated heliometer to the simpler appliances in the hands of observers, with descriptions of many of the more important purposes for which they have been brought into use. The chapter concludes with a reference to Lacaille's memorable expedition to the Cape of Good Hope and the expeditions undertaken on occasion of the transits of Venus in 1761 and 1769.

Chapter 11, on "The Structure of the Heavens," is as varied in its contents, amongst which we may note: The periodicity of sun-spots, and the new views upon the physical constitution of the sun; the ring of small planets; the zodiacal light; the meteor-streams and their connection with comets; the physical condition of comets; the distribution of the stars; the Milky Way; solar motion in space; variable and double stars, and binary systems; stellar spectra, star clusters, and nebulæ.

In Chapter 12 we have an account of the principal modern literature, periodical and otherwise, bearing upon astronomical science in its various branches. There are notices of the works of Weidler, Lalande, Bailly, Montucla, Delambre, Littrow, Madler, and others, and of such works as the *Acta Eruditorum*, the *Monatliche Correspondenz* and the *Astronomische Nachrichten*.

It should be understood that the one chief advantage which the student is likely to derive from Prof. Rudolf Wolf's "History of Astronomy" will be a knowledge of the authors, methods, &c., with which it may be necessary for him to become acquainted in turning his attentisn to any particular department of astronomy, an advantage that may not be immediately apparent from the title of the work. Prof. Wolf does not enter into any amount of detail, nor indeed would it have been practicable within the limits of this volume. But as affording in comparatively brief space an accurate idea of the gradual progress and actual state of astronomical science and a valuable guide to any one entering upon its study, this book may be confidently recommended. J. R. HIND

[1] Continued from p. 259.

OUR BOOK SHELF

The Spectroscope and its Work. By Richard A. Proctor. Society for Promoting Christian Knowledge. (London: 1877.)

In a little work of 127 pp. Mr. Proctor has clearly and logically explained the principles of the science of spectroscopy, and has given a sketch of the main results of spectroscopic research into the nature of the sun, stars, and nebulæ.

One of the features of this book is, we think, the logical manner in which the principles of spectroscopic analysis are developed from the facts gained by observation and experiment; the steps of the various reasonings are succinctly but clearly stated; this is a point of much importance. In too many so-called scientific text-books there is a loose and illogical method of connecting facts, and conclusions drawn from these facts; by the perusal of such books the general reader is either strengthened in his prejudged conviction that science teaching is of little or no value as a mental exercise, or he is taught, often almost unconsciously, to believe that the generalisations of science and the facts of science rest upon exactly the same evidence. Another feature in Mr. Proctor's little book is the adoption, necessarily to but a limited extent, of the method of historically developing the facts of the science of which he treats. The leading steps in the history of the most important advances in spectroscopy are traced, frequently by quotation from the classical memoirs of the great workers in the science.

The book is divided into eight chapters, headed respectively "Analysis of Light," "Dark Spaces in the Spectrum," "Various Order of Spectra," "Interpretation of Solar Spectrum," "Solar Prominences, &c.," "Spectra of Stars, &c.," "Atmospheric Lines in Solar Spectrum," "Measuring Motions of Recession and Approach."

Whether as an introduction to the fuller study of spectroscopic analysis, or as a work from which the general reader may gain a clear, and, so far as it goes, complete view of the science, Mr. Proctor's work is deserving of the warmest recommendation.

M. M. Pattison Muir

The Great Thirst Land; a Ride through Natal, Orange Free State, Transvaal, and Kalahari Desert. By Parker Gillmore. (London: Cassell, Petter, and Galpin.)

Captain Gillmore's work is disappointing. The title suggests Major Butler's "Great Lone Land," but the result of a comparison of the two works would not be very favourable to Capt. Gillmore's. He has nothing new to tell the geographer, and many of the hunting stories are comparatively tame. The work is unnecessarily large, and could with advantage be compressed to half its present size. Still there are a number of observations on the Boers and the natives which will interest many, and there are a few good lion stories. The book is handsomely got up.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Strychnia and its Antidote

The following circumstance I think worth noticing. Wanting to banish some mice from a pantry, I placed on the floor at night a slice of bread spread over with butter in which I had mixed a threepenny packet of "Battle's vermin killer," which contains about a grain of strychnia along with flour and prussian blue. The following morning I was roused by a servant telling me that a favourite skye terrier was lying dead. I found that the mice had dragged the slice of bread underneath the locked door and that the dog had thus got at it and eaten part equal to about one-sixth of a grain of strychnia; it lay on its side perfectly rigid; an occasional tetanic spasm showed that life was not quite extinct. Having notes of the experiments made by direction of the British Medical Association last year, on the antagonism of medicines, and wherein it was conclusively proved that a fatal dose of strychnia could be neutralised by a fatal dose of chloral hydrate, and that the minimum fatal dose of the latter for a rabbit was twenty-one grains, I at once injected under the dog's skin forty-five grains of the chloral in solution, my dog being about twice the weight of a rabbit. In a quarter of an hour fancying the dog was dead, as the spasms had ceased and it lay apparently lifeless, I moved it with my foot, when it at once struggled to its feet and shortly after staggered to its usual corner by the parlour fire; it took some milk, and except for being quieter than usual seemed nothing the worse for the ordeal it had passed through.

That the fatal effects of a poisonous dose of strychnia was thus counteracted so successfully by what I should say was a poisonous dose of chloral, given hypodermically, is an interesting fact verifying the experiments I alluded to. Without such experiments on the lower animals, a medical man might often be found standing by helpless to aid his fellow-man under similar effects of poison.

Sudbury, Suffolk, February 27 — J. Sinclair Holden

Age of the Sun in Relation to Evolution

I think I may be permitted to point out that Dr. Croll has missed what I had intended to be the main feature of my criticism of his article on the "Age of the Sun in Relation to Evolution." I should therefore wish to reiterate that, in his theory, he takes no account of the proper motions of the stars in space. If it be true that suns or stars have been formed by the collision of bodies possessed of great energy, proper motion can be none other than the unused and unconverted energy of the original components. Supposing the forces, before impact, to be equal and opposite in direction, there can be no misunderstanding that the result will be the *entire* convertion of the "motion of translation to molecular motion," *i.e.*, heat; but this, according to the law of chances, must be of exceedingly rare occurrence. Yet, from our knowledge of the motions of the stars in space, this, or something very like this, has invariably occurred. Surely here is a *reductio ad absurdum.* In conclusion I will merely state that I have never yet claimed to have suggested a theory reconciling the age of the sun with prevailing opinions in geological science or with the hypothesis of evolution. Having felt the difficulty, I have endeavoured in some measure to stretch the interval wherein these may have had time to effect their changes, but I have not claimed to have succeeded to the desired extent. I am not, therefore, interested in replying to the former part of Dr. Croll's letter, and indeed, with certain minor reservations, have no hesitation in subscribing to it.

John J. Plummer

Orwell Dene, Nacton, February 28

The Zoological Station at Naples

Permit me to correct some statements made in Nature, vol. xvii. p. 329. The small steam launch was given to the Zoological Station by the Berlin Academy of Science, in exchange for a working table in the laboratory, which is to be placed at the disposal of the Academy for ten years. The Prussian government subscribed 300*l.* towards the expenses of the launch, which was built by Messrs. J. Thornycroft, Church Wharf, Chiswick, and has proved an excellent little craft.

As to the publications of the Zoological station, the *Prodromus Faunæ Mediterraneæ* will be a compendium of all the species hitherto observed in the Mediterranean, and recorded in scientific works. Its publication is mainly intended to facilitate the nomenclature of the chief work, the "Fauna and Flora of the Gulf of Naples and the Neighbouring Seas," which is to appear in monographs. The *Prodromus* has been undertaken by Prof. T. Victor Carus, whose great knowledge both of zoological literature and classification will answer its exactitude and completeness

The first monograph to be published is not on the *Elanophoræ*,

but on the *Ctenophoræ*, a family well known to all students of pelagic animals.
ANTON DOHRN
Naples, February 28

Faraday's "Experimental Researches"

IF your readers will compare Mr. Quaritch's letter in last week's NATURE with his advertisement of the two preceding weeks, they will see that it bears its condemnation on its own face. No words of mine can make it plainer than his do, that a *reprint* of an obsolete and valuable book was offered to the public as something not stated to be a reprint. Had that advertisement stated that the work was a "facsimile reprint," there could have been no ground either for complaint or for an indignation for which no apology is needed.
University College, Bristol
SILVANUS P. THOMPSON

Mimicry in Birds

ON the evening of the 24th inst. my attention was attracted by an interesting example of mimicry in the case of the starling. The first thing which attracted my attention was hearing the cry of a blackbird in distress, and on looking round, the only bird to be seen was a solitary starling, which, when I first observed it, was uttering its own note; but almost immediately thereafter it began to whistle loudly in imitation of the blackbird. After this, for the space of about half an hour, it kept up a constant succession of notes in mimicry of the chaffinch and sparrow, always, however, using its own note for the space of about half a minute between each change. I may add that it did not seem to have any particular order in which it repeated the various notes.

A gentleman in this neighbourhood tells me that last year he observed a similar occurrence in his garden; but this, so far as I can learn, is the only other instance of similar mimicry in this quarter. Perhaps some of your readers may be able to inform me if it it is of common occurrence elsewhere.
Edinburgh, February 26
J. STUART THOMSON

Great Waterfalls

SEEING Mr. Guillemard's inquiry (vol. xvii. p. 221), I refer him for accounts of the Falls of Tequendama, which I visited in 1851, to "Viajes Cientificos a los Andes Ecuatoriales, &c.," por M. Boussingault, traducidas por J. Acosta; Paris, 1849, and "New Granada: Twenty Months in the Andes," by Prof. Isaac F. Hotton. (New York: Harper and Brothers, 1857.)
New York, February
THOS. BLAND

SEVERAL NEW APPLICATIONS OF SCIENCE INTRODUCED INTO WAR

ON Saturday last, the Speaker of the House of Commons and a large following of members, visited the Portsmouth Dockyards, mainly for the purpose of witnessing some torpedo warfare; the *Inflexible* was also inspected. Near the starboard side of the ship, one of the sheds had been converted into a temporary lecture-room, and provided with numerous diagrams, a model of the ship, and a full-sized skeleton model of the 80-ton gun, 26 feet long, 6 feet broad at the breach, and having a calibre of 16 inches, with four of which it is intended to arm the turrets of the *Inflexible*. The diagrams were drawn on a scale of one-fourth of an inch, half an inch, and 6 inches (half-size) to the foot, and were designed with the object of enabling the visitors to draw a comparison between the structures of the *Dreadnought* and the *Inflexible*, and the respective thicknesses and disposition of their armour. As soon as the party had assembled around the drawings, Mr. W. B. Robinson, the Chief Constructor of the yard, stepped forward and delivered a brief illustrative lecture on the main points and differences of the two ships. He pointed out that while the length of the *Inflexible*—320 feet—was exactly the same as the *Dreadnought*, its beam—75 feet—was 11 feet 2 inches broader; that its volume of displacement was greater, its armour heavier, its turrets thicker, and its armament more formidable. While, however, the *Dreadnought* was armoured along the water-line, the citadel of the other ship, which was placed upon an armoured deck below, was the only protected portion of the *Inflexible* above the water. The arrangements of the turrets was also different; for whereas those of the smaller ship were placed along the middle line, the turrets of the larger were *écheloned* to starboard and port in order that all the four guns might be trained upon an enemy either direct ahead or direct astern. The weather deck between the turrets had also been raised, so that the guns could be loaded from below without the necessity of depressing their muzzles. She was supplied with steam and hand pumps, and with Friedland's injectors, which would enable her to discharge 5,300 tons of water an hour. She had 133 water-tight compartments, and water would be admitted into the double bottom to reduce the rolling of the ship. Her torpedoes would be discharged from submerged ports in the bows instead of from above the water in the side, as in the *Thunderer*. The ventilating arrangements will be of the most perfect kind; for, as Mr. Robinson remarked, while in the other ships the fresh air is pumped into all parts, no means are adopted for assisting the vitiated atmosphere out of the ship. In the *Inflexible*, however, the ventilation comprises both supply and exhaust arrangements. The air is brought down into an air chamber, or cave of Æolus, in the central part of the citadel, and is driven thence by steam fans through large pipes, which pass under the armour deck and up into the structures above, and by means of branch pipes fitted with cocks every compartment in the ship can receive an abundant supply. By these cocks an officer can regulate his air supply in much the same way that a householder on shore can regulate his water supply. The vitiated air is sucked up through pipes with perforated ends into the funnels, and thence through the citadel into the open air. The *Inflexible* will be brig-rigged, but her masts will be unshipped before going into action. Her engines are of 8,000-horse power, and she is expected to attain a speed of 14 knots.

Runs of the 16-inch Whitehead torpedo were next made. One was fired from a steam pinnace as in actual warfare, its course, which was in a straight line for about 200 yards, being distinctly traceable by the exhaust air-bubbles which it threw up. The other was discharged from the surface of the water for the purpose of showing how readily it sank automatically to the required depth. The next novelty submitted was the steam pinnace, which, without having a single man on board, can do everything but stoke and keep its own fires alight. Its engines are worked and its movements are controlled wholly by electricity, the cable which supplies it with its mysterious power being unwound from winches as the pinnace sails on its mission, "And drags at each remove a lengthening chain." Its principal use is to drop and explode countermines in the neighbourhood of an enemy's mines, and by destroying them clear a harbour for the approach of the fleet. It performed its work to the amazement of the beholders on Saturday. The countermines were represented by a couple of barrels containing small charges of gun-cotton, and with these slung over the sides it took its departure from the boat containing the battery and dropped the casks at a distance of about 200 yards, igniting at the same time the fuses which blew the barrels into match-wood, and returned obediently, like a "thing of life," to the controlling hand after having accomplished its duty. Near at hand in the basin the torpedo nettings for protecting ships against the locomotive torpedo were exhibited on the sides of the *Actæon*, while the prow of the *Bloodhound*, gunboat, was armed with the trawl with which it is proposed to pick up sunken mines. The notes of the bugle were next heard as a summons to quarters and for the *Thunderer* to be cleared for action. In an

incredibly short space of time the stanchions and guard-rails were flung down, the water-tight doors closed, and the couple of Gatling guns dismounted from their carriages on the superstructure deck and hoisted up to the tops, whence they could each discharge 200 shots per minute upon hostile boats. Presently the process of loading the guns was gone through, in the fore turret by hydraulic power, and in the after turret by hand gear; the turrets were rotated and the guns run out, and the snapping of the tube fuses told the spectators that a furious action had commenced.

The party was next conveyed to the *Vernon*, torpedo school, where Capt. Arthur and Commander Wilson had still more surprising wonders in the art of warfare for them to witness. First of all, Commander Wilson delivered a hurried lecture on the mysteries of torpedo science, explaining the differences between offensive and defensive torpedo warfare, the nature of the several explosive substances used, and the methods of firing torpedoes—mechanically, by means of glass tubes of sulphuric acid, which explode on coming in contact with chloride of potash, or glass tubes filled with potassium, which causes explosion on mixing with the water, or, electrically, by means of detonation produced by fulminate of mercury. Commander Wilson also showed by means of a model in a tank the method of ascertaining when a ship is over a sunken torpedo by means of cross-bearings, and how the mine is fired as soon as the telescope which is following the movements of the ship completes the electrical circuit. As the model, however, did not sink with great alacrity in its mimic ocean, Commander Wilson explained that the torpedo did not profess to destroy a ship instantly, but only to knock a hole in the bottom about the size of a barn door. At the conclusion of the lecture the company again went on deck, and saw discs of dry and damp gun-cotton harmlessly consumed, and how a solid block of wood could be shivered by the same material when exploded with a detonator. Next, on looking over the bulwarks, they beheld a practical illustration of the boat's crew "creeping" for an enemy's torpedo, the process consisting in dragging for the mooring chains, and, when found, destroying them by a discharge of gun-cotton. They were also shown the manner of attack with star torpedoes, firing lines of counter mines by "bumping" the circuit closer, and, lastly, how attacking boats can be destroyed by grenades fired by fuses held in the hand. These beautiful experiments closed the day's programme.

We are glad that so large a body of our legislators were present, and we are sure they could not fail to be impressed with the importance of science in its bearing on war. In fact it has become clear that the more war we have, or are likely to have, the more is science needed; and it would indeed be a very short-sighted policy for Government to cut down the very moderate supplies allowed to science for the purpose of providing war expenditure. We hope that after what they saw on Saturday the members of the House of Commons will have a higher opinion of the national value of scientific research than to adopt any such course.

METEOROLOGICAL NOTES

TORNADO IN CHESTER COUNTY, PENN., U.S.—Mr. Richard Darlington, of Ercildoun Seminary, has published an account of a remarkable tornado which swept over this portion of the United States on Sunday, July 1, 1877. The tornado appears to have been first felt a little to westward of the boundary line between Chester and Lancaster Counties, and to have thence run a course of twenty-two miles, first in a direction E. 20° S., then E. 15° S., and lastly, E. 7° S., moving bodily onwards at rates of from five to fifteen miles an hour, the average progressive movement being twelve miles an hour. The destruction of trees, houses, and other property is estimated at about 40,000 dols., the destruction being most complete in those cases where the tornado moved across a valley. An uninterrupted roar, like that of thunder, accompanied it during its whole course, which is sufficiently accounted for by the inherent energy of the tempest itself and the havoc it wrought throughout its course, such as twisting thick oak-trunks in two, tearing up tall trees by the roots, and whirling them aloft, and blowing down buildings and scattering their contents in all directions. The breadth of the tornado varied from 100 to 300 feet, the average being 200 feet, though the *débris* was scattered to a greater distance on either side. It appears to have originated between a south-west and a north-west wind, where a large dark cloud seemed to form in the atmosphere, suspended from which was a whitish funnel-shaped cloud whirling round in a terrible manner. The air was thick with the objects which were whirled aloft, the movements of which closely resembled buzzards sailing round. The rotary movement was to the left, and the cone appeared to be a cloud of vapour nearly white, connected at the upper end with a smooth surface of cloud somewhat darker. The upper portion of the cone appeared to move in a straight line and at a uniform rate, while the tail or lower end frequently bent in different directions, as if swayed from its true course by the hills and valleys it crossed. No rain fell in its track, but hailstones of a large size and in great quantity fell at intervals along its north side. Trees in the northern half of the track were generally thrown down with their tops to the south, while those on the southern side were thrown to the north; but at certain points, such as at Ercildoun, trees and other *débris* were thrown down in what appeared to be inextricable confusion. Some of the observers state that the *débris* ascended up the centre of the funnel-shaped cloud and fell back to the ground outside it, but the tornado was too sudden, brief, and appalling to admit of careful observations being made on this point, which is all-important in its bearing on the theory of tornadoes. No tornado is known previously to have traversed this part of the United States.

THE LAW AND ORIGIN OF THUNDERSTORMS.—In the Christmas issue of the *Bulletin International* of the Paris Observatory, there is an interesting note on this subject by Prof. Ch. V. Zenger, of Prague. He has examined the thunderstorms which occurred at Prague during the ten years ending 1849, and those at Vienna during the four years ending 1875, arranging the dates of their occurrence according to the semi-solar days each period of observation embraced, there being twenty-nine such semi-solar days in each year. The general result is that, dividing the semi-solar day into three equal portions, 47 per cent. of the whole thunderstorms occurred in one of these portions, 32 per cent. in another, while only 21 per cent. occurred in the third. Prof. Zenger is of opinion that this result points to a cosmical origin for the thunderstorm, operating, no doubt, on pre-existing terrestrial conditions, an opinion which receives some countenance from the relation subsisting between thunderstorms and auroral and magnetic perturbations. The subject is of sufficient importance to call for a wider and more exhaustive treatment.

MONTHLY METEOROLOGICAL BULLETIN OF THE MONTSOURIS OBSERVATORY, No. 69.—This number gives the observations for August last, which now include, for the first time, the hourly velocities of the wind in addition to the hourly temperatures and rainfall, which were added some months ago. The daily minimum velocity of the wind, 7·2 miles per hour, for August occurred about 6. A.M., and the maximum velocity, 13·1 miles per hour, about 2 P.M., the increase in the wind's velocity being thus nearly doubled between these hours. These hours are all but coincident with the hours of mean minimum and maximum temperature. The table of the hourly amounts of the rainfall is a peculiarly valuable one. Several years must, however, elapse before its full

value will be seen in determining the curve of the diurnal variation of the rainfall. We note, with much satisfaction, the continued prosecution of the important inquiry into the chemical climatology of Paris.

METEOROLOGY OF WESTERN AUSTRALIA.—We have recently received a most valuable addition to the meteorology of Australia, which is being so energetically worked out by Messrs. Todd, Ellery, Russell, and Macdonnell, in the form of a first Report of the Meteorology of Western Australia, by Mr. Malcolm Fraser, Surveyor-General for the Colony. The report contains a good summary of a pretty complete set of observations made during the whole of 1876 at Perth, and the barometric means for five months at Point King Lighthouse, on the south coast. The chief results are, for the summer months, mean monthly pressure—29·915 inches, temperature 74°·7, rainfall 0·54 inch, and wind velocity in miles 404; and for the winter months—pressure 30·177 inches, temperature 57°·5, rainfall 4·90 inches, and wind velocity 280 miles. The lowest temperature for the year was 34°·7, and the highest 112°·0, on February 20, and it may be noted that the mean daily maxima for this month was as high as 93°·7. Speaking generally, the winds in summer blow from the sea inland, and in winter from the land seawards, little rain falling in the former season, whereas in the latter season the rainfall is copious but not excessive. The smallest rainfall of any month was 0·04 inch in February, falling on one day, and the largest 8·45 inches in June, falling on nineteen days. It is contemplated to establish stations at Nickol Bay, Champion Bay, and York; but a still further extension of the system is required, not merely for the development of the climatology of the colony, of which we may be said to know next to nothing, but also from the important bearing of the meteorology of Western Australia on that of the whole continent of Australia, particularly on the system of weather warnings for that group of colonies.

OUR ASTRONOMICAL COLUMN

THE URANIAN SATELLITES, ARIEL AND UMBRIEL.—We continue the ephemeris of the two interior satellites of Uranus, making use of Prof. Newcomb's tables in the appendix to the Washington Observations for 1873. The positions and distances are for 9h. Greenwich mean time, when the planet will be near the meridian during the period over which the ephemeris extends; though these are given for every evening, the presence of the moon in this quarter of the heavens may interfere with observation on or about March 16.

	Ariel.				Umbriel.			
		°		"		°		"
March 8	Pos.	358	Dist.	12·6	Pos.	312	Dist.	7·9
9	,,	191	,,	15·2	,,	195	,,	20·6
10	,,	25	,,	12·4	,,	145	,,	9·1
11	,,	241	,,	6·2	,,	18	,,	20·0
12	,,	153	,,	7·4	,,	334	,,	10·6
13	,,	0	,,	13·3	,,	201	,,	19·1
14	,,	193	,,	15·1	,,	161	,,	12·1
15	,,	19	,,	11·5	,,	23	,,	18·1
16	,,	256	,,	5·5	,,	347	,,	13·6
17	,,	150	,,	8·4	,,	206	,,	16·9
18	,,	3	,,	13·9	,,	171	,,	15·1
19	,,	195	,,	14·8	,,	30	,,	15·6
20	,,	33	,,	10·5	,,	355	,,	16·5
21	,,	272	,,	5·1	,,	214	,,	14·1
22	,,	165	,,	9·4	,,	178	,,	17·7
23	,,	5	,,	14·4	,,	39	,,	12·6
24	,,	198	,,	14·4	,,	1	,,	18·8

THE TRANSIT OF MERCURY ON MAY 6.—The *Nautical Almanac* furnishes the usual elements of this phenomenon and the times of the contacts and of least distance of centres referred to the centre of the earth, with the necessary formulæ for reducing the moments of contact to any place upon the earth's surface. The following figures result for Greenwich, Edinburgh, and Dublin; Greenwich mean times at the respective observatories:—

	First External Contact. h. m. s.	First Internal Contact. h. m. s.
Greenwich	3 10 58	3 14 4
Edinburgh	3 11 0	3 14 6
Dublin	3 11 3	3 14 9

The least distance of the centres (4′ 47″) takes place at 6h. 58·5m. and, as the sun will set at 7h. 29m., 7h. 47m., and 7h. 36m. local mean times at these places respectively, rather more than half the transit will be visible. The final contacts may be well observed in America.

THE RADCLIFFE OBSERVATORY.—The Radcliffe Observer is again punctual in the distribution of his volume of Observations, Vol. xxxv., containing the work in the year 1875, having been in the hands of astronomers several weeks. The only new feature is the publication of observations of the solar spots; the distances from the sun's limbs are fixed by transits and by readings of the declination circle of the heliometer; descriptions and sketches of the forms of the spots are included. Nearly 1,200 stars were meridionally observed. At the end of the Introduction, Mr. Main has exhibited the apparent errors of Tabular R.A. of the moon's limbs, as given on the same day by the observers at Oxford and Greenwich in 1863 and 1864, and from 1870 to 1874 inclusive. As usual the meteorological observations taken at the Radcliffe Observatory are published in considerable detail.

THE HARVARD COLLEGE OBSERVATORY, U.S.—Prof. Pickering has issued a report of proceedings at this observatory during the year ended November 1, 1877, with an outline of the course of observations intended to be pursued in future with the 15-inch refractor and the meridian circle, the telescope of which has an aperture of eight inches. The newly-discovered satellites of Mars have been the objects to which most attention has been directed with the refractor, the observations consisting not only in a series of measures of positions and distances which Prof. Pickering believes to be second only to the very complete series obtained by the discoverer with the large Washington instrument, but in a numerous series of photometric comparisons with the planet on methods explained in the Report, by which the image of Mars was brought to the same degree of brightness as each satellite. It was remarked under favourable opportunities for comparison that the outer satellite did not partake of the red colour of Mars, which Prof. Pickering observes is "a curious result, and having an important value in any theory of the cause of the peculiar colour of Mars." The observations were not wholly reduced at the time the Report was issued, but an approximate reduction gave the diameter of the outer satellite about 5·9 miles, and that of the inner one, 6·5 miles. "As the darker colour of the outer satellite somewhat diminishes its light," it was considered safe to call it about six miles in diameter, and the inner satellite seven miles. These comparisons were made between August 27 and October 12. A large number of similar measurements of seven of the satellites of Saturn, including the very faint object, Hyperion, have also been obtained. Remarking that other classes of observation appear to be well cared for at various observatories in the United States (Dr. Peters being engaged in the determination of the small stars near the ecliptic, at Clinton; the great telescopes of Washington, Chicago, and Cincinnati, being used almost exclusively for micrometric measures; spectroscopy being the intended line of observation at Princeton College; and the telescopes of Mr. Rutherford and Dr. Draper being largely used for photographic purposes), Prof. Pickering intends to devote the Harvard refractor mainly to photometry as "a field almost wholly unexplored with large telescopes," in America or elsewhere. The meridian-circle appears to have been chiefly

employed in the determination of stars to the ninth magnitude inclusive in the zone included between $+ 50°$ and $+ 55°$ of declination, undertaken at the instance of the Astronomische Gesellschaft, and this work approaches a conclusion. Upwards of 40,000 observations have been made with the meridian-circle since it was mounted at the end of 1870. The personal establishment at Harvard College now consists of Prof. Edward C. Pickering, as director, assisted by Prof. Rogers, and Messrs. Searle, Waldo, and Upton. Vol. x. of the *Annals* of the Observatory has been published during the past year. Vol. ix., with photometric observations 1872–75, is to follow, and is nearly ready for issue.

GEOGRAPHICAL NOTES

THE ALBERT NYANZA.—In his recent examination of Albert Nyanza, to which we have already referred, Col. A. M. Mason examined every inlet or indentation of the coast-line. Starting from Magungo in the s.s. *Nyanza*, Col. Mason followed the western shore, and found it overhung by lofty mountains, notwithstanding which there seemed to be a large population. On the first day the party reached Nurswar, and on the next continued their route to the south-west; after a six hours' run, they found that the coast-line trended more to the south, forming a wide plain, which in some places was covered with very heavy, thick forests. On the third day they crossed a wide bay to Kavalee. Soon after leaving Kavalee, Col. Mason found that the coast-line turned to the eastward, and in two hours' time they reached a mass of ambatch (like Signor Gessi), and found the south end of the lake very shallow. In the south-west corner Col. Mason noticed a second large bay, and from a depression in the mountains and a thick line of forest, he fancied that there might be a river emptying into the lake at that point, but he could find no entrance, and this accorded with what he had been told at Kavalee, that no river joined the lake near there. On the morning of the fourth day, after entering a number of small, shallow bights, he finally reached a broad river, the waters of which were reddish in colour, with a slight northerly current. The width of the stream is about 400 yards, the banks high and well-defined, and clothed with forests. Col. Mason was only able to proceed up this river for one hour, owing to the shallowness of the water, and there seemed to be a mass of vegetation blocking the way to the south; to the south-east he observed an immense forest of date-palms, and to the south and south-west an undulating country, covered with large trees. After leaving this river he found that he had crossed the lake, and that their course turned to the northward. On both sides of the lake the mountains were found to diminish in altitude, and to the southward, at the foot of the lake and between the two ranges, was a large isolated mountain, which was found to be in N. lat. 1° 11′. It is clear, therefore, that Lake Albert does not extend, as has been asserted, to the first parallel of north latitude. In his northward course Col. Mason found that the mountains were not so high as on the western shore, and that in only one place were the cliffs as lofty as the highest on the opposite coast. There was a marked difference, too, in the vegetation; on the western shore the mountains are well covered with timber and verdure, and in many parts the natives have cleared places for cultivation, while on the east the mountains are barren, with neither timber nor vegetation. On the fifth day the party passed several large villages, one of which was said to be the residence of Kava Gonza, brother to Kaba Rega, and, soon after, the village of Tiaboa was reached, above which the country is flat, and the coast-line trends to the north. From his observations Col. Mason found that Kavalee, near the south-west angle of Lake Albert, was in N. lat. 1° 22′ 20″, and the south-east angle in N. lat. 1° 11′ 3″.

MR. STANLEY'S WORK.—Mr. Stanley is engaged in writing a full account of his most important journey across Africa; and at present he is doing so with characteristic energy. Already a large portion of his manuscript is in the printer's hands, and his work will doubtless be ready for publication in May next. Mr. Stanley carried with him through the whole of his arduous journey a heavy photographic apparatus, and succeeded in obtaining many very good negatives of views and groups on the great lakes and on the Congo. The interest of these pictures can scarcely be over-estimated. They will be reproduced as full-page woodcuts in the volumes, which will also contain an unusually large number of vivid scenes and incidents from excellent sketches made by Mr. Stanley himself. Perhaps the most important feature of the work will be the chart of the Congo, which has been so minutely and elaborately mapped, that it will require a scale of an inch and a half to a degree to embody in the smallest writing the information conveyed. Besides this large route map, which will be in two parts, the work will also contain several maps of a valuable and interesting character. The work will be published simultaneously, the *Publishers' Circular* informs us, or as nearly so as can be arranged, by Messrs. Sampson Low and Co. in England; by Messrs. Harper and Brothers, New York; in French by Messrs. Hachette and Co., Paris; in German by M. Brockhaus, Leipsic; in Danish by M. Mallings, of Christiania. Negotiations are also pending for translations into the Swedish, Spanish, Italian, and Russian languages. The title is, "Through the Dark Continent; the Sources of the Nile; around the Great Lakes, and down the Congo." We are pleased to see that the *Geographical Magazine* of this month handsomely acknowledges that its previous hard judgment on Mr. Stanley's conduct was unjustifiable.

SOUTH-WEST AFRICA.—In his monthly summary, Dr. Behm refers briefly to an important journey made by two Rhenish missionaries last summer between the Cunane river and 21° south lat. They found that the coast mountains, opposite Wallfisch Bay, extend far to the north-west, with a height of from 4,000 to 4,500 feet. The travellers have noted many important details in their map which will form an important supplement to existing maps of Africa, as the region traversed is almost unknown.

AFRICAN DWARFS.—Dr. O. Lenz contributes to the *Mittheilungen* of the Vienna Geographical Society for January an important paper on this subject. He describes his own observations on the Abongo of the Ogové, whose average height is 133–152 centimetres. Dr. Lenz concludes that all the dwarfish African peoples—the Abongo of the Ogové, the Dongo of the Sette River, the Bakke-Bakke of the Loango Coast—are only part of an original great negro people, who are also found in the interior under various names—as Kenkob in the Lufum country, Mala-Gilagé in the south of Bagirmi; and further east, as Akka, Doko, Berikomo, &c.; and that this great people, who were perhaps the aboriginal inhabitants, the true autochthones of equatorial Africa, have been supplanted and destroyed by other migratory peoples. Dr. Lenz places the Bushmen in a similar category.

THE NORTH-EAST PASSAGE.—Prof. Nordenskjöld and Mr. Dickson of Göteborg, recently paid a visit to Hull in order to make various preparations for their intended Arctic expedition. It is also announced that Lieut. Sandeberg intends to organise a scientific expedition to Kolgajeff, the Petchora, Hvideö, and the Siberian coast during the approaching summer; he has already hired a vessel for this purpose, and intends to be absent for about six months.

DR. LENZ.—The well-known African traveller, Dr Oskar Lenz, has been presented with the cross of the Albrecht Order by the King of Saxony.

POPULAR NATURAL HISTORY[1]

WE have to congratulate the publishers of this fine volume on its appearance, for, on a careful perusal, it strikes us as very eminently fitted to supply a known public want. On one or two previous occasions the same firm have published large and well-illustrated works on natural history, the descriptive portions of which were, to say the least possible of them, not in any way up to the science of the day; but we have lately gladly witnessed an infinitely more careful editing of such works on natural history as have been published by Cassell and Co., and the present work, so far as its descriptive portion is concerned, can boast of being written by men so well known as Duncan, Dallas, and Murie, while the illustrations,

Fig. 1.—The Negro Monkey (*Semnopithecus maurus*).

many of which are very beautiful, and the general style of the get-up of the volume may well be left to tell their own tale.

The work aims at being an encyclopædia of the Natural History of the Animal Kingdom, and this, which forms its first volume, contains an account of the apes and monkeys, by the Editor; of the lemurs, by Dr. Murie, and of the bats and insect-eating mammals, by Mr. Dallas. May we, in the interest of the success of the work, suggest, that in order to complete the publication of such a work, within a reasonable time, there ought to be different portions of it simultaneously published. Thus there would be nothing to hinder the volumes on fishes being published alongside of those of the mammals, and it would be decidedly well to publish those volumes that will treat of the sponges and corals long before this portion of the animal kingdom would in the ordinary course be reached; but we venture this only as a suggestion.

The Editor tells us that the volume before us is meant to explain the many interesting facts of the natural history of animals, and that every endeavour has been made to unite zoology with comparative anatomy; the aim is high and the idea is a good one, but the authors sometimes fail to come up to the standard they hold before them. It is perhaps not to be wondered at, for do not the very words of our English language seem to fight against the perfect accomplishment of such aims. Vertebra, says Dr. Duncan, is a Latin word, which means "a turning joint in the body," *or*, "a back bone." What idea will the English youth take away of this Latin word?

We regret a little to see the attempt to give each animal what we suppose we must call an English name, and we are tempted to ask, Which is it easier to say, *Troglodytes calvus* or *Nschiegombouve?* The former is the scientific name of a little-known ape; the latter is the name we are to make believe, as the young folk say, is English. It would have been better if both names had been equally conspicuous, then we could have taken our choice, and we can see no good reason for burying the scientific name in a foot-note.

As specimens of the style and illustrations, we have selected the following:—"The Negro Monkey (*Semnopithecus maurus*) (Fig. 1) is of an intensely black colour, except underneath, and at the root of the tail, where there is a grey tint. The paws are long, delicate, and silky, and become slightly grey on the head and back with old age. Like most black things it leads a troubled life, being chased and hunted, not, however, in the Javanese forests, and sometimes fifty or more individuals associate together. The Negro Monkeys make rude nests on trees, and are extremely timid, making off with great haste if they are disturbed. A long series of generations have been chased and killed by the natives of Java, and therefore the present Negro Monkeys are exceedingly shy, and bolt from the face of man at once. And yet, although thus timid and anxious to get out of the way, they have the reputation of being dangerous, and really unwittingly they may be so. On the approach of men they utter loud screams, and scamper off amongst the trees, helter-skelter. Now in doing this they break dead branches off, and sometimes a large fruit or nut comes tumbling down some score or two of feet. These are supposed to be thrown by the monkeys, but such is not the case. Having this bad character, the 'Negroes' are cudgelled with sticks, and killed in numbers very cruelly. Their pretty fur is much prized, and the chiefs of the country arrange the hunting parties, treating the monkeys really as beasts of the field. The skin is prepared by a simple process which the natives have learned from Europeans, and they conduct it with great skill. It affords a fur of a jet-black colour, covered with long silky hairs, which is used by the natives and Europeans there in ornamenting riding saddlery and in military decoration.

"When young they are of a brown or reddish tint, and thin grey tints appear preceding the intense black; they then eat buds and shoots and tender leaves, but in adult age they are fruit-consumers. When in captivity they are sullen and morose, and they will remain sulky for many months. This the natives know, and therefore they never try to tame them or to have them in their houses."

Another pretty illustration taken from the *Proceedings* of the London Zoological Society, is that of the Red-bellied Monkey (*Cercopithecus erythrogaster*), which is described as follows:—

"When living at the Zoological Gardens in the Regent's

[1] "Cassell's Natural History," edited by P. Martin Duncan, M.B. (Lond.), F.R.S. Vol i. Illustrated. (Cassell, Petter, and Galpin, London, Paris, and New York.)

Park, this pretty monkey, with a red chest and belly, and slim tail, was very timid, but it liked to be petted by the keeper, being somewhat distrustful of its more romping companions. It would take food out of his hand, and seemed pleased, and generally played with his fingers without attempting to bite. The canine teeth were very moderately grown (Fig. 2).

"This monkey inhabits Western Africa, and is at once known by the red belly and chest, the white beard and whiskers, and the black band across the forehead. It has, moreover, a yellow crown."

As a last illustration we select that of a most remarkable animal, the West African River Shrew (*Potamogale velox*, Fig. 3).

"This was originally described by its discoverer, M. du Chaillu, as a carnivore, under the name of *Cynogale velox*, but as its characters were very doubtful, the name *Potamogale* was suggested for it in case of its proving to belong to a distinct genus. The late Dr. Gray described it as a rodent under the name of *Mythomis*. Some years later Prof. Allman and Prof. Barboza du Bocage procured perfect specimens, and proved the animal to belong to the

FIG. 2.—Red-bellied Monkey (*Cercopithecus erythrogaster*).

insectivora, the latter naturalist describing it under the new name of *Bayonia velox*. Thus within a few years it received no less than three different names.

When the insectivorous nature of Du Chaillu's River Shrew was ascertained, it was found to be most nearly allied to the Centetidæ or Tanrecs, with special affinities to the West Indian Solenodens. It is, however, generally regarded as constituting a distinct family, characterised, among other things, by the less cylindrical skull, the absence of clavicles, the union of the two bones of the shank towards the extremity, the presence of anal glands, and the compressed form of the tail. The teeth, as in the true Tanrecs, are forty in number, but the molars differ considerably in form, as will be seen from the annexed figures.

This little beast, which has given rise to so much discussion among zoologists, and received so many names, is only a little larger than our common stoat, measuring about nine inches in length, exclusive of the powerful tail, which is of about the same length. In its appearance it very much reminds one of a miniature otter, from which, however, it differs considerably in the form of the head, which terminates in a broad flattened muzzle, having its sides furnished with a most luxuriant crop of stiff bristle-like whiskers. The hair of the upper part of the body and limbs is brown and soft, although rather coarse, and that of the lower surface yellowish ; and the coat consists of two kinds of hairs, namely, an inner coat of very fine short silky hairs, through which longer hairs of a very

peculiar structure project. These long hairs are very thin at the bulb, and increase very gradually in thickness for about one-third of their length, when they suddenly contract a little, and then expand into a flat lance-shaped blade, which terminates in a very fine point. This coarser part covers the whole body, the thick root of the tail, and the upper part of the limbs; the rest of the tail, the under side of the muzzle, and the upper surface of the feet are clothed with short, close hairs. The ears are of moderate size, the eyes very small, and the toes on all the feet, five in number, are armed with small sharp claws, and without webs, but the second and third toes on the hind feet are united as far as the end of the first phalanx.

The most remarkable peculiarity of the animal is its tail, which presents a most unusual development for an insectivorous mammal. Prof. Allman says:—"It is so thick at its base that the trunk seems uninterruptedly continued into it; but it soon becomes laterally compressed, and then grows gradually thinner and narrower towards the tip. . . . . Its lower edge is rounded, and its upper is continued into a membranous crest about one-eighth of an inch in height, and clothed with the same short, stiff, appressed hairs" as the rest of the tail.

This great development of the tail might of itself convince us that this organ is of great service to its owner, and such, from the account of the habits of the animal

Fig. 3.—West African River Shrew (*Potamogale velox*).

given by its discoverer, is evidently the case. M. du Chaillu says:—"This extraordinary animal (Fig. 3) is found in the mountains of the interior, or in the hilly country explored by me north and south of the equator. It is found along the water-courses of limpid and clear streams, where fish are abundant. It hides under rocks along these streams, lying in wait for fish. It swims through the water with a rapidity which astonished me; before the fish has time to move it is caught. On account of the rapidity of its movements I have given it the specific name of *Velox*. The animal returns to land with its prey almost as rapidly as it started from its place of concealment. The great motive power of the animal in the water seems to be in its tail."

So far as we have been able to read over this volume, we have found that great pains have been taken to record all the novel facts known about the animals here treated of. We perceive an account of the nest-building power of that most extraordinary Madagascar lemur, the Aye-Aye (*Cheiromys madagascarensis*) and the strange instances of mimicry about the bats, first noticed by Dr. Dobson, is to be found also noticed.

An index to each volume would be a very desirable addition.

NITRIFICATION

THE origin of salpetre is a subject which has vexed the minds of several generations of chemists. Nitrate of potassium, or salpetre, is found in nature as a white crust, appearing on certain rocks, old walls, and even upon the surface of the soil; from this mode of occurrence the name "salpetre" is doubtless derived. The largest natural source of salpetre is afforded by certain soils in India. Soil having a white film of salt on the surface is collected from the neighbourhood of house-drains and stables; the soil is washed with water, and the nitre crystallised from the solution. With this Indian salpetre England has been, till quite recently, almost exclusively supplied. The countries of Continental Europe, not having access to so considerable a natural source of nitre, have been obliged from early times to produce nitre for themselves. At first the earthen floors of cottages and stables were collected, washed, and nitrate of potassium obtained by treatment with wood-ashes and crystallisation; but the inconvenience of collecting such material, and its general poverty in nitre, soon led to attempts

at producing salpetre by artificial means. To Glauber, a chemist of the seventeenth century, apparently belongs the credit of first preparing nitre artificially. The process as carried out in the present day is in outline as follows:—Soil, containing more or less of vegetable mould and carbonate of calcium, is mixed with a certain proportion of stable manure or other refuse animal matter, and disposed in small heaps, care being taken that the mass of soil and manure shall be sufficiently porous to ensure the free admission of air: these heaps are protected from rain, and are from time to time watered with stable sewage. At the end of two or three years the earth is sufficiently rich in nitre to be worth extracting. This tedious process for manufacturing nitre has, during the last few years, been superseded to a considerable extent by the treatment of Peruvian nitrate of sodium with chloride of potassium, by which nitrate of potassium and chloride of sodium are produced.

It is evident that the artificial nitre-beds just described, merely perform, on an exaggerated scale, an operation which occurs naturally in all ordinary soils. The chemical analysis of drainage waters has taught us that such waters are characteristically rich in nitrates, and that the amount of nitric acid present stands generally in close relation to the quantity of nitrogenous manure previously applied to the soil. The published analyses of the drainage waters from the experimental wheat-field at Rothamsted, show that ammonium salts applied as manure are rapidly converted into nitrates by the soil, the quantity of nitric acid in the drainage water being proportional to the amount of ammonium salt applied. The recent application of soil for the purification of sewage is another striking example of the same action. The sewage, as poured upon the soil, contains ammonia, and putrescible organic matter rich in nitrogen; the sewage which has filtered through a few feet of porous soil is found to contain nitrates, but only traces of organic nitrogen or ammonia.

What explanation can we give of this phenomenon of nitrification? It is clearly a process in which nitrogen is oxidised into nitric acid; but how is this oxidation brought about? The old chemists believed that a decaying organic body evolved more or less of its nitrogen in a free state, and that this nitrogen, while nascent, combined with the oxygen of the air to form nitric acid. This view has been held by some down to the present day. Hofmann, in his Exhibition Report of 1862, offers the same explanation, only substituting for free air the oxygen condensed on the surface of porous bodies. This theory has been extended by some to include the ordinary nitrogen of the atmosphere, so that on their view nitric acid may be formed in soil from the nitrogen and oxygen of the atmosphere, without the intervention of other nitrogenous matter. According to others the oxidation of gaseous nitrogen is brought about not by ordinary oxygen, but by ozone. Other chemists have inclined to the belief that nitrogen is never oxidised in the soil except when in the form of ammonia, and that the nitrogen of organic matter is always converted into ammonia as a preliminary to nitrification. According to some experiments, the ferric oxide, which gives a red colour to so many of our soils, is itself an oxidising agent, and capable of converting ammonia into nitric acid.

We need not, however, enumerate all the opinions that have been held on this confessedly obscure subject. Many of the experiments which were thought to support certain views, now appear, in the light of recent evidence, of little value. Before, however, discussing the new facts recently contributed to the subject, we may just indicate those points which have been most clearly established.

There is very little evidence for supposing that gaseous nitrogen is ever converted into nitric acid in the soil. Nitrous and nitric acid are indeed produced by electric discharges through the atmosphere, thus originating the small amount of nitrates brought to the soil by rain, but this appears to be the only reaction capable of producing nitric acid from the direct union of oxygen and nitrogen. According to Carius even ozone is quite incapable of oxidising gaseous nitrogen. Ammonia is, on the other hand, oxidised by ozone, nitric acid being formed; but that ozone is an agent in soil transformations is certainly unproved, and appears very improbable. There remains the action of ferric oxide, already referred to. This reaction deserves further study; it cannot, however, be considered as generally important, since nitrification certainly occurs with vigour in soils practically destitute of ferric oxide.

The researches of successive generations of chemists had thus failed to give any satisfactory explanation of the important phenomenon of nitrification. The subject has quite lately been attacked by Schloesing and Müntz from an entirely new point of view; their results, published in the early part of last year, plainly indicate that nitrification, instead of being brought about by purely chemical forces is, in fact, the work of a living organism. The evidence adduced in support of this new view is very simple. These chemists show that nitrification, however active, is immediately stopped by the vapour of chloroform, a substance which previous study has shown to suspend the action of yeast, and of all organised ferments. They also find that when nitrification has thus been suspended for many weeks, it can be restarted by the addition of a small quantity of a nitrifying body. In a second communication they further prove that the temperature of boiling water is sufficient to destroy all power of nitrification, and that soil which has been once heated to this point produces, in air free from germs, carbonic acid and ammonia, but no nitrates. If, however, this soil is moistened with water containing a little unheated soil, the production of nitric acid again commences.

This new theory of nitrification has been investigated at Rothamsted with results completely confirmatory of the views put forward by these French chemists. It was found that the vapour of bisulphide of carbon, and of chloroform, effectually prevented nitrification in a moist garden soil through which air was frequently aspirated, while without these vapours the soil produced nitrates in considerable quantity. A solution of chloride of ammonium containing a little tartaric acid, phosphate of potassium, and carbonate of calcium, was also completely nitrified in a few weeks by the addition of a small quantity of soil taken from the "fairy-ring" of a meadow. This solution, when nitrified, was successfully used as seed to produce nitrification in other similar solutions, which, without this addition, produced no nitric acid. It was further shown that light was prejudicial to nitrification; solutions kept in a dark cupboard producing nitric acid, while similar solutions standing in daylight produced none.

The evidence has thus become very strong that the nitrates in soil owe their origin to oxidation brought about by living organisms. That mycoderms, in their processes of life, may exert a powerful oxidising action upon organc matter, we have already learnt through the researches of Pasteur and others. The most familiar example is that of the acetic fermentation. Vinegar is produced by the oxidation of alcohol during the growth of a very simple organism, the *Mycoderma aceti*, without the growth of such an organism no vinegar is ever formed. It is by similar low organisms that fermentation of all kinds is brought about. Putrefaction has also been shown to be equally dependent on the presence of microscopic organisms, and except under the conditions suitable for their rapid development putrefaction will not take place. With this abundant evidence before us of the energetic decomposition of organic matter, brought about by what we may term microscopic fungi, we can hardly be astonished to find that the same agency is capable of

oxidising the nitrogen of organic matter and of ammonia, and thus producing nitric acid.

The organisms which produce these wonderful changes consist of colourless cells; they are independent of daylight, for they derive their supply of carbon exclusively from organised matter, and from the decomposition of such matter they obtain the force necessary for life and growth. In these respects they differ entirely from green vegetation, in which sunlight is the source of all energy, and carbonic acid gas, decomposed by the aid of light, the material from which carbon is derived. The colourless and green organisms, however, equally require phosphoric acid, potash, and other ash constituents; and both appear to be capable of assimilating nitrogen in the form of ammonia.

Not only are these simple organisms independent of the aid of light, but light is, in some cases at least, actually fatal to their existence. This fact has quite recently been established by Downes and Blunt. They find that the *bacteria* present in an organic fluid may in many cases be entirely destroyed by exposure of the solution to daylight, and that even when this is not the case, their development is much retarded by such treatment. This observation is perfectly in accordance with the fact observed at Rothamsted, that nitrification did not proceed in solutions exposed to daylight. In the last communication of Schloesing and Müntz, it is stated that vegetable soil suspended in water by passing a stream of air through the mixture, undergoes nitrification both in light and darkness. No details of the experiment are given, but it seems probable that such a mixture would be more or less opaque, and the greater bulk of the material consequently at all times in partial darkness.

The microscopic organism producing nitrification has probably distinctive characters, and might be isolated by cultivation under conditions specially suitable to its growth, but more or less unfavourable to the life of other associated germs. Pasteur has pursued this method with success in the case of beer yeast, and has shown that with the pure yeast thus obtained an unchangeable beer may be manufactured, the organisms producing secondary changes having been excluded. The subject of nitrification has clearly reached a stage which demands the aid of the vegetable physiologist. R. WARINGTON

FOSSIL HUNTING AT BOURNEMOUTH

I HAVE recently deposited in the South Kensington Museum some unusually large specimens of fossil plant remains from Bournemouth and Studland. The matrix in which these are imbedded is friable, and the remains, in most cases, are extremely difficult to extract, so that a brief account of the process employed may be of use to would-be collectors. The largest specimen, part of the frond of a feather-palm, measures 4 ft. by 3 ft., and as this presented the greatest difficulties, I will more particularly describe the work which its preservation involved.

In digging last autumn at Bournemouth in a bed of dark clay about 60 feet above the sea-level, and about the same distance from the top of the cliff, we came across a well-preserved fragment of this specimen consisting of a portion of the stem with the bases of pinnæ attached. We included a younger athletic brother, a coast-guardsman whom I have long employed, as well as myself, and occasional other assistance. The tools we used were pick-axes, crow-bar, and spades. The place was a slightly projecting ledge, none too solid, with a steep cliff above and below. So soon as the fragment mentioned was brought to light by a stroke of the pick digging was stopped, and a careful examination was made by the aid of our knives to see in which direction the frond trended. Finding, fortunately, that the direction was towards the mass of the cliff, we determined to use our endeavours to extract it in as perfect a condition as might be. We therefore, at about mid-day, commenced to dig away the superincumbent mass until a slab was bared at least twice the size of that ultimately required, when we proceeded to clear down and lay bare the specimen. Loose sand blowing up in clouds, however, settled upon it and threatened to adhere so firmly to the wet clay that it was feared it might be found impossible to remove it, whilst the drying action of the wind caused it to crack and peel, notwithstanding all our efforts to keep it covered with damp paper and linen. It was then determined to remove the slab without exposing the leaf, leaving that operation until it was safely housed at home, and we therefore commenced the laborious operation of undermining this great slab and removing it in such pieces as from time to time broke away by their own weight from the main mass. For five hours these pieces kept breaking away in blocks of about one foot in thickness, and as much in weight as two or three of us could lift. At dusk our task was not more than two-thirds completed, but as wet was expected, it was determined to extract the whole that night if possible. Perhaps the most toilsome part of the work was carrying the pieces up the sixty feet of cliff. A hand-barrow having been improvised, it required our united efforts to convey each piece to the path above, and this was really hard work, and in addition I had great anxiety throughout lest the edges should be rubbed. Notwithstanding all our trouble we had the mortification of seeing our large lumps repeatedly break and subdivide. The work went on until about 9 P.M., when we found it impossible to continue, and therefore carefully covered up the remains of the slab, the vicinity of a populous town rendering this precaution necessary. The next day the whole of the pieces were removed in a cart from the coast-guards station to an out-house in our occupation. When they arrived there the prospect was far from hopeful. We had apparently but a truckful of lumps of black wet clay, a foot or more in thickness, and varying in diameter from a few inches to two or three feet, the majority without trace of the fossil upon them, or any marks or indication of how they were to be fitted together. Experience among these fossils has taught me not easily to despair, and I knew, moreover, from the care that had been taken, that the edges could not be much abraded, nor could any considerable pieces be missing. Our lodging contained a new and comparatively well-lit cellar, to which all was removed. A table was next made, six feet long and four feet wide, and portions of three days occupied in ascertaining how the pieces could be fitted together.

Two days were then lost in fastening the smaller pieces together into larger slabs, but it was found that these larger pieces would not come together properly in the box, their relative thickness, &c., being different. They were next reduced in thickness to about three inches and transferred to the box in which they now are, and fitted together as accurately as possible and fixed by glue and plaster of Paris, $\frac{3}{4}$ cwt. of the latter being used.

A great disappointment now awaited us. From standing and kneeling upon the slab whilst engaged in digging it out, the upper surface of the leaf was kneaded into the under surface, and would not part for weeks afterwards, until quite dry, and then in very small fragments only.

Another difficulty was that two other fronds were found at lower levels traversing the one we were endeavouring to save, and in some places these had been cleaned out before the mistake was discovered. The base of the frond, it will be seen, has been abandoned altogether, and not more than two-thirds is now preserved. The next thing was to get it to London safely, and the railway officials were cautioned as to the care required and the necessity of keeping it flat and right side up, and the case was

insured. Even while I was in the office insuring it a stupid porter tilted it over face downwards to my great grief, as I had little hope that the plaster would hold with such a weight if the case were subjected to this treatment on the way. I was relieved in my mind a few days later by its safe arrival at home. From this time patience alone was required, and by Christmas, with the aid of Mr. De Wilde, the whole leaf was uncovered and varnished and all the cracks filled in with modelling clay.

The other specimens were obtained in more or less the same way. The small feather palm was extracted whole with the assistance of Mr. Henry Keeping, of the Woodwardian Museum, but fell to pieces on the shutter to which it was transferred for carriage, and great care was needed to put them together as they now are. The Studland fan-palm being rotted by exposure on the face of the cliff and being penetrated everywhere by rootlets, fell into a hundred pieces, and only the centre of the leaf could be pieced together the rest being pulverised in its journey from Studland to Wareham.

J. S. GARDNER

FATHER SECCHI

SOME little time ago we announced the serious illness of Father Secchi, the well-known astronomer and Director of the Observatory of the Collegio Romano, at Rome; last week we chronicled his death, which occurred on the 26th ult. The illness which has thus terminated fatally, has cut him off, we may say, in the prime of his life, and in the midst of his work; for, till he was taken ill, there were no signs of any diminution of his energy, and he was only fifty-nine years of age when he died.

Secchi was born at Reggio, on June 29, 1818. Educated and trained from early youth as a Jesuit, we hear of him first in connection with science as Professor of Physics at Georgetown College, near Washington, and next as holding the same chair in the Roman College at Rome. It was in connection with the observatory attached to this institution that almost all Secchi's work for the last thirty years has been done. While the Roman College was in papal hands no funds were spared to make the observatory as complete as possible. Secchi had instruments and assistants in abundance, and his various series of "Memoirs" testify to his industry in many fields, while his position gave him great facilities for giving the widest publicity to his work. What he lacked in originality he made up in assiduity, and hence, although he has left no great life work on any one subject behind him, there is, we think, hardly any question which has turned up touching observations in astronomy, magnetism or meteorology on which a multitude of papers have not been written by his busy pen. Many of these papers are very admirable and show great penetration and power of generalisation as well as a wide grasp of many subjects.

Secchi's great interest in solar physics was doubtless aroused, when in America, by assisting Prof. Henry in making the first experiments on the heat radiated by different portions of the sun's disc by means of the thermo-electric pile. His interest in spectroscopy dates from Janssen's visit to Rome, when on his scientific mission to Italy and Greece. In both these branches of work Secchi has been an ardent observer and voluminous writer. He photographed the eclipse of 1860 in Spain, and observed the one of 1870 in Sicily. In 1867 he was in Paris exhibiting his universal meteorograph in the exhibition of that year, and giving lectures, some of which eventually formed the basis of his book on the Sun, a second edition of which appeared last year. Besides this book on the Sun, he has written others on the Unity of the Physical Forces, and on the Stars, the latter of which has not yet appeared.

When the States of the Church became Italian the Roman College was among the institutions which were turned to other uses by the new government. This now contains two most interesting museums, one of educational apparatus chiefly for primary instruction, and another for antiquities. The new Government, however, were extremely anxious not to interfere with Secchi's scientific labours and offered him the Chair of Astronomy in the new Roman University, at the same time granting ample funds for the prosecution of his inquiries. This Secchi accepted, but soon found his occupation gone, as he was commanded by the chief of the Jesuits to resign it, which he did. It is doubtful whether any *modus vivendi* would have been found if the king, whose foster-brother he was, had not stepped in between the Ministry and the Vatican, and suggested a compromise which would have left Secchi to continue his work under most favourable conditions, if the Jesuits had not again stepped in.

One of the most recent results of Secchi's energy has been the foundation of the Societá degli Spettroscopisti Italiani, a society specially constituted for recording daily spectroscopic observations of the sun, chiefly at the various observatories of Italy.

There is no doubt that in the death of Father Secchi observational astronomy has sustained a great loss. His industry and skill were largely rewarded during his lifetime. In 1867 he received the great French prize of 100,000 francs. He was a member of most scientific societies, including our own Royal Society, and it must not be forgotten that if there may have been traits of Secchi's character open to criticism, the exigencies of his post, rather than the inclinations of the man, may have been to blame.

NOTES

THE French expedition for the observation of the approaching transit of Mercury consists of M. C. André and M. Angot, who formed likewise part of the expedition to New Caledonia, on the occasion of the transit of Venus. Ogden, in the State of Utah, has been selected by the French Institute as the most favourable locality for the observation, and the expedition is already under way to its destination. A Parisian millionaire, well known for his generosity towards scientific objects, has contributed 30,000 francs to defray the expenses of the observation.

WE regret to learn of the dangerous illness of the well-known mineralogist, M. Delafosse Gabriel, professor at the Muséum d'Histoire Naturelle of Paris. He is now in his eighty-third year, and has been for twenty years a member of the French Academy of Sciences.

THE Royal Academy of Sciences at Berlin has elected the well-known Prof. Noeldecke, of Strassburg, a corresponding member.

ARRANGEMENTS are being made at Paris for the erection of a fitting monument to the late Claude Bernard. The initiative has been made by the *Société de Biologie*, of which Bernard was one of the founders, and over the meetings of which he has presided during the past eleven years. The committee appointed for the purpose contains prominent names from all the leading scientific institutions of Paris.

THE death is announced of Mr. Joseph Bonomi, the distinguished Egyptologist, which occurred at Wimbledon Park on Sunday last, at the age of eighty-two. For the last sixteen years Mr. Bonomi has acted as Curator of the Soane Museum in Lincoln's Inn Fields. Mr. Bonomi went out to Egypt as early as 1824, and spent eight years on the banks of the Nile, drawing and studying the ancient temples and their wonderful sculptures. During this time he had adopted the Arab costume and mode of living, and by this means he was able to go on in the prosecution of his studies with his then limited resources. He returned

to England, where he remained till the visit of Lepsius in 1842, when he went out as artist in that expedition, which resulted in a large folio work of about twenty vols., published under the auspices of the King of Prussia. The Egyptian Court at the Crystal Palace was erected from Mr. Bonomi's designs and under his superintendence. He was also employed in the British Museum in arranging the department of Egyptian antiquities. Mr. Bonomi has rendered great service, not only by his illustrations, but also by his writings on subjects connected with the various countries in the east which he visited. These will be found in the *Transactions* of the Royal Society of Literature, Syro-Egyptian Society, British Association, &c. Mr. Bonomi leaves a great mass of notes and sketches of hieroglyphics which may yet be of great value to Egyptology.

AT the General Monthly Meeting of the Royal Institution on Monday, the special thanks of the members were given to Mr. Warren De la Rue, D.C.L., for his donation of 50*l.* for the benefit of the Chemical Laboratory.

WE would remind our readers that subscriptions are still being received for the Simon Testimonial Fund. A marble bust is the form of testimonial that has been decided on, and a small copy of this will be presented to each subscriber of two guineas and upwards. No testimonial was ever better deserved. Subscriptions may be sent to Robarts, Lubbock, and Co., Lombard Street, to the Hon. Secretary, 1, Adam Street, Adelphi, or to the Treasurer, General Register Office, Somerset House.

THE Atlas of Colorado, soon to be issued by the U.S. Geological Survey of the Territories, under Prof. F. V. Hayden, embodies the results of the geological and geographical work of the survey during the years from 1873 to 1876 inclusive. This Atlas will contain the following maps :—1. A general drainage map of Colorado on a scale of twelve miles to the inch. 2. An economic map of the same region, having as its basis the above-mentioned drainage map. This map will indicate the areas of arable, pasture, timber, coal, mineral, and desert land in as great detail as possible on the scale. 3. A general geological map, on which the areas covered by the principal formations will be shown. The drainage map will form the basis for this also. 4. A map showing the scheme of the primary triangulation in the state. Scale twelve miles to the inch. 5. Six topographical sheets showing the same area as that covered by the general drainage map, but in much more detail. The scale of these sheets is four miles to an inch. The relief of the country is indicated by contour lines, at vertical intervals of 200 feet. The area covered by each of these sheets is 11,500 square miles. 6. Six geological sheets, of which the bases are the six topographical sheets just mentioned. On these the detailed geology is expressed by colours. With the appearance of this map, Colorado will be better known, topographically and geologically, than any other State.

ONE of the leading publishing houses of Paris is making arrangements for the speedy appearance of an enormous work, "Études sur l'Exposition de 1878," under the direction of M. E. Lacroix. This work is intended to be a complete record of the progress made in all the arts up to the present date, and its thoroughness and value have been assured by the promised co-operation of a large number of leading authorities. The French Ministers of Public Works, of Commerce, and of Agriculture have already promised all necessary assistance on the part of the Government, so that the undertaking will start under the most favourable auspices.

DR. SCHLIEMANN intends to resume his excavations at Hissarlik as soon as the country is at all safe to live in.

THE Society of Arts prize of 10*l.* for the best set of blowpipe apparatus that could be sold retail for one guinea, has been awarded to Messrs. Letcher, of Camborne and St. Day, Cornwall. A second prize, consisting of a bronze medal, has been awarded to Herr Osterland, of Freiberg.

THE establishing of a branch of the U.S. National Observatory, to be placed at some elevated point in the West, has lately been agitated, and much is expected as the result of its completion.

ON July 16 an International Exhibition of the Paper Trade will be opened at Berlin and will last until August 31. The programme of the exhibition is already finally settled, and the objects exhibited will be divided into eight different groups, viz. : 1. Raw materials and articles used for making paper, pasteboard, &c. 2. Machines and tools used for making and working paper. 3. Paper and boards of all descriptions. 4. Paper, as far as it is employed for printing, paper-hangings, &c. 5. Articles made of paper or *papier-mâché*. 6. Paper as used for technical or building purposes. 7. Writing and drawing materials. 8. Objects, books, &c., relating to the history and literature of paper. A number of prizes will be awarded for the best contributions.

THE system of agricultural weather-warnings in France, carried on under the direction of the Paris Observatory, continues to be rapidly developed and extended to all parts of the country. The warnings are now sent to 1,432 communes spread over all the departments of France except that of Lozère.

A NOVEL and valuable application of electricity, designed to prevent the possibility of collisions on railways, is now the subject of experiment in the Marseilles station. It consists of an electric mirror, in which all the movements on a line 100 kilometres in length are brought vividly before the eye, and enables the station-masters to follow exactly the progress of every train. By this means it is hoped that all accidents resulting from delays or too rapid runs can be entirely avoided, and arrangements are being made for the general introduction into the stations of the new invention.

A VIENNA mechanician has recently succeeded, after many fruitless trials, in constructing a sewing machine which does not require the person working at it to submit to the unpleasant and unhealthy necessity of constant bodily exertion, viz., setting the machine in motion by the foot. Since, for pecuniary reasons, the application of electricity, steam, or water power was impossible, the inventor of the new machine was restricted to gravitation or elasticity, and he, preferring the latter force, has contrived to make springs strong enough to keep an ordinary sized machine in motion for hours. A system of cog-wheels is arranged underneath the surface of the table upon which the machine is fixed, and by a handle at the side the spring is wound up with the greatest facility. The velocity at which the machine works is entirely at the option of the person using it, and can be regulated *ad libitum*, and in the simplest manner.

OPPORTUNITY has been taken recently by MM. Raehlmann and Witkowski to observe the eye-motions of persons asleep, new-born children, blind persons, and also in circumstances presenting some resemblance to sleep, viz., drowsiness, intoxication, chloroformic sleep, and epileptic attacks, these cases having in common the failure of the will or the power for binocular vision. In every instance strongly uncoordinated movements were observed. The result is regarded as opposed to the idea of a mechanism possessed at birth for producing regular motions of the eyes, and as agreeing with Helmholtz's view :—"Though each eye has a quite independent muscular mechanism . . . . we have only learned to perform those movements which are necessary for seeing a real point distinctly and simply." Where this interest is not yet present, as in newly-born infants, or where it disappears, as in the case of the blind, and in the sleeping, there occur divergences from the law of adaptation.

THE German Verein zur Beförderung des Gewerbfleisses has offered extensive prizes for the invention of substitutes for caoutchouc and gutta-percha.

NEWS from South America states that powerful waterspouts were recently experienced at Callao, doing considerable damage in the town and its environs. On January 27 Callao suffered again severely from a repetition of the phenomenon known as the "tidal wave," from which so much damage was done in May last. Much destruction of buildings has resulted. On January 23 a strong earthquake, lasting thirty seconds, was felt at Iquique and Arica; shocks recurred on the 24th and 25th. On December 31 a violent thunderstorm visited Lima, accompanied by torrents of rain. The latter phenomenon is of extreme rarity in that neighbourhood, and during the present century has occurred there only once, viz., in the year 1804.

THE *Moniteur Vinicole* announces the surprising fact that the wine-production of France has not been diminished of late years, in spite of the devastations of the pernicious insect, phylloxera. During the five years, 1862 to 1867 (before the vast spreading of the plague), the annual production averaged 54,747,405 hectolitres. During the five following years (1867 to 1872) it rose to 56,527,129 hectolitres. After 1872, since when the phylloxera invasion began to reach its maximum height, the average annual production has not sunk below 56,000,000 hectolitres. The total of last year's produce amounts to 56,405,363 hectolitres, as against 41,846,748 in 1876.

THE statistics of the German Imperial Telegraph Office for 1877 have just been published. When the Telegraph Office was united with the Post Office there were 1,688 telegraph stations in Germany. At the end of 1877 this number had risen to 3,287.

THOSE desirous of sending objects of natural history from Guatemala (Central America) to the forthcoming Paris Exhibition are requested to communicate with M. A. Boucard, of 55, Great Russell Street, W.C., until the 20th inst. After that date all communications should be sent to the following address:— Legacion de Guatemala, 3, Rue de Copenhague, Paris.

THE experiments on the practical value of the telephone, carried out by the German postal department, show that it is not adapted to supersede the telegraph on lines which are constantly in use. For local purposes and lines less frequently used it will, however, be introduced on a large scale, a large pecuniary saving being effected by the ease in obtaining officials who have not had to master the difficulties of telegraphy. The department has also introduced an apparatus for calling the official at the station to which a message is to be sent, so that an electric battery is now unnecessary for the purpose of summoning attention.

AUSTRALIAN colonists have noticed some strange peculiarities in bees imported from Europe, which, however unpleasant they may be to the agriculturist, are yet of the highest interest to naturalists. It appears that our European bees retain their industrious habits only for the first one or two years, when imported into Australia. While during that period they keep their hives in good order and yield a fair quantity of honey, they gradually cease to collect honey after that time, and soon become entirely barren.

MR. MURRAY has published in a neat little volume, Virchow's address at the German Association last autumn, on the Freedom of Science in the Modern State. We are glad this has been done, as the address is one well worthy the attention of men of science. It was our sense of its importance that induced us some months ago to publish in our columns a verbatim translation of the address, as well as translations of the addresses of Haeckel and Nägeli, on which Virchow's address is to a large extent a criticism and reply.

THE master of the *D. M'B. Park* (British barque), which arrived at West Cowes (I. W.), March 3, from Batavia, reports as follows:—January 29, at 7 A.M., in lat. 4.20 N., long. 21.45 W., saw several submarine volcanoes throwing large columns of water about 100 feet into the air, while the sea was in great commotion, as it is when there is a very strong under-current, the weather at the time being very cloudy, with rain, and nearly calm. The sound was like distant thunder.

VARIOUS theories have been offered of the sense of temperature. In a recent one by M. Hering it is represented that when at a given part of the skin we feel neither heat nor cold, the feeling of temperature at that part is, so to say, at *zero*. The main points of the theory are these: The feeling of temperature depends on the height, for the time being, of the temperature proper (*eigen Temperatur*) of the nervous apparatus of the skin, not on the rise or fall of this temperature (Weber) nor on the intensity and direction of the heat current (Vierordt). Every temperature of the nervous apparatus above the zero point is felt as heat, every one below as cold. The distinctness of the sensation of heat or cold increases with the distance of the temperature proper for the time being, from the zero temperature. The zero temperature is variable within certain limits. Every temperature of the nervous apparatus, felt as warm, causes a displacement of the zero point of the scale of sensation upwards, and every temperature felt as cold causes a displacement downwards. These ideas are developed by M. Hering, in a recent paper to the Vienna Academy.

M. LENGLEN, a physician of Arras, has recently described a remarkable perpetuation of physical traits. A certain M. Gamelon, in the last century, was sex-digital, having two thumbs on each hand and two great toes on each foot. The peculiarity was not noticeable in his son, but in each of the three subsequent generations it has been strongly marked, some of the children at present showing the malformation as distinctly as their great-great-grandfather. M. de Quatrefages has noticed, a few months since, a similar case in the animal kingdom. A six-toed cock having transmitted this peculiarity to his descendants, it has spread to such a degree, that in the district where it occurred the ordinary five-toed variety is no more to be met with.

LIME, strontian, and baryta have recently been obtained in the crystalline state by Dr. Brügelmann, of Düsseldorf (*Ann. der Phys.*, No. 11), by heating their nitrate salts till complete decomposition takes place. In this way are obtained the three oxides in (chiefly) microscopical crystals of the regular system, and exclusively hexahedra. While, however, in the case of strontian and baryta, this interesting fact and new example of isomorphism is recognisable only with aid of the microscope, the lime can be easily obtained in large crystals, observable with the naked eye. Dr. Brügelmann describes his method fully, as also the form and properties of the three crystallised alkaline earths.

IT was pointed out some time ago by M. Herwig, that when strong induction shocks are sent through liquids they do not pass conformably to Ohm's law; there is at first a retardation of the electricities in the electrodes, and the equilibration which at length occurs is somewhat like a discharge, as in the case of a large condenser. The phenomenon has of late been more fully studied by M. Herwig, who describes various interesting experiments with reference to it in the *Annalen der Physik*, No. 12.

M. GASTON PLANTÉ describes at length in the last number of the *Annales de Chimie et Physique*, his newly-discovered method for the engraving of glass, a process which promises to be of widely-extended application. His attention was first directed to this line of investigation by the observation that glass moistened with a solution of ordinary salt was strongly attacked by currents from secondary piles. As perfected, his process consists in

immersing a plate of glass in a shallow basin containing a concentrated solution of potassic nitrate. It is encircled by a platinum wire also covered by the liquid, and connected with the pole of a secondary battery of fifty elements. The other pole consisting likewise of platinum wire covered with an insulator is held in the hand and applied to those parts of the glass where it is designed to engrave. A flash of light is produced by every contact with the electrode, and a mark accompanies each luminous appearance. The depth and fineness of the lines described depend directly on the rapidity with which the electrode is moved, and the fineness of its point.

THE Deutsche Gesellschaft für öffentliche Gesundheitspflege has appointed a commission to co-operate with the government meteorological stations, in endeavouring to obtain the daily publication of the weather observations with probabilities for the following twenty-four hours, according to the American system.

THE additions to the Zoological Society's Gardens during the past week include two Brown Coatis (*Nasua nasica*) from South America, presented by the Hon. C. H. Wynn; a Palm Squirrel (*Sciurus palmarum*) from India, presented by Miss Barclay; two Rock Sparrows (*Petronia stulta*), South European, presented by Mr. D'Arcy Thompson; an Ocelot (*Felis pardalis*), a Red and Yellow Maccaw (*Ara chloroptera*), a Yellow Snake (*Chilobothrus inornatus*) from South America, two Black-capped Bitterns (*Butorides atricapilla*) from Africa, a Four-lined Snake (*Coluber quadrilineatus*) from Egypt, deposited; a Yellow-cheeked Amazon (*Chrysotis autumnalis*) from Honduras, purchased; a Red Kangaroo (*Macropus rufus*), an Indian Muntjac (*Cervulus muntjac*), born in the Gardens.

EXTENT AND PRINCIPAL ZONE OF THE AURORA BOREALIS

IN the *Wochenschrift für Astronomie*, Herr H. Fritz has recently compared his "Catalogue of Polar Lights," which contains notes of all auroræ which have been observed since 1846, with a publication of Herr A. Moberg, who gives an account of all auroræ observed in Finland during the years 1846 to 1855—some 1,100 in number. The comparison yields some interesting results which are not without importance for the theory of the phenomenon.

It appears that out of 2,035 days of the months from August to April, upon which auroræ were seen and which are entered in Herr Fritz's Catalogue, no less than 1,107 days were auroral days in Finland. Of these 1,107 auroræ 794 were simultaneously visible both in America and Europe, 101 only in Europe, while the remaining 212 were only seen in Finland. During the same period (1846 to 1855) 928 auroræ were seen in Europe or America which were not visible in Finland. All these figures of course refer only to the months from August to April, since during the summer months no phenomena of this kind can be observed in Finland on account of the brightness of the nights. As Herr Moberg's observations were collected from 128 different stations in Finland, we must conclude that only a very small number of auroræ remained unnoticed. We thus arrive at the conclusion that a great number of auroræ cannot have a very widely extended sphere, or that the causes of these phenomena must often be of a very local character (this is confirmed by several observations at high latitudes), while with another part of the phenomena the extension of their sphere or district of simultaneous visibility must be very considerable. The number of auroræ which were seen in Finland only—at least for which up to the present no data of observation elsewhere have been received—is very small (212, or only 19 per cent. of the total number seen in Finland). As the frequency of the phenomena increases—at the time of the maximum—the number of simultaneous observations in Finland and America rises, while the numbers of auroræ seen in Finland and Europe only, or of those exclusively seen in Finland, decrease. This agrees perfectly with the well-known law that with the increase of frequency of polar lights their intensity and sphere of visibility increase also. If we carefully take into account the less prominent phenomena the above proportions would be slightly modified, but most probably they would never prove that on any day when an aurora was visible only in a small district in Europe, another one was simultaneously seen in America. Thus the comparison made by Herr Fritz contradicts Renou's view that the phenomena in America and Europe change periodically.

Of 2,878 days on which auroræ were observed in America during 1826 to 1855, there are 1,065 on which auroræ were also seen in Europe, so that at least every third observation was simultaneous in both parts of the world. For the years during which more exact observations were made, viz., from 1846 to 1855, and again from 1868 to 1872, we find that during the former period out of 1,691 auroras 657 were simultaneously observed both in America and Europe; and during the latter period out of 715 no less than 397, or far more than half the number. If the catalogues were more perfect the number of simultaneous observations would, beyond doubt, be found to be still greater. Some observations made in Scotland give similar results to those dating from Finland, but their publication must at present be delayed, since the American data for comparison are still wanting.

The local occurrence of auroræ does not speak favourably for the hypothesis which places the phenomenon among the cosmical ones. Some ten years ago Herr Fritz published his views with regard to the geographical distribution of auroræ, and constructed a system of lines which he termed Isochasms—*i.e.*, curves of equal frequency of auroræ. The outlines of this system were as follow:—The zone of greatest frequency and intensity of auroræ began near Barrow point (72° north latitude) on the northern coast of America; thence it passed across the great Bear Lake towards Hudson's Bay, crossing the latter at 60° N. lat., passing over Nain, on the coast of Labrador, keeping south of Cape Farewell; its further course was between Iceland and the Far Oer to the vicinity of the North Cape in Norway, and thence into the Arctic Sea. According to the observations then in possession of Herr Fritz, the line passed round Novaya Zemlya and Cape Tsheljuskin, approached the north coast of Asia, in the eastern part of Siberia, in the longitude of Nischni Kolymsk, and thence returned to Barrow Point.

Now after ten years, in spite of the vastly accumulated material of careful observations, there appears no necessity to change Herr Fritz's system of curves in any essential detail; indeed certain parts of the same, which were at first only based on probability and supposition, the part of the principal zone between the north of Norway and Nishni Kolymsk as an instance, we now know with perfect certainty to be correct. Nearly identical, perhaps entirely so, with the line of greatest frequency is the line which marks the limit of visibility of auroræ towards the pole or the equator; since to the north of the line in question the auroræ are only seen in the direction of the equator.

PARIS ACADEMY PRIZES FOR 1878

I. EXTRAORDINARY PRIZES.—Grand prizes in the Mathematical Science.—1. The application of the theory of elliptic transcendants or abelians to the study of algebraic curves. 2. It is known that the great axis of the orbit which a planet describes round the sun is not affected by any secular inequality of the order of the two first powers of the disturbing masses. Examine if there exists in the value of this great axis secular inequalities of the order of the cube of the masses, and, in the case where these inequalities are not rigorously destroyed, give the means of calculating their sum, at least approximately. The prize is a medal of the value of 3,000 francs. 3. Study of the elasticity of crystallised bodies, from the double point of view of experiment and theory. Prize the same as No. 2.

Grand prizes in the Physical Sciences.—Study of the mode of distribution of marine animals on the littoral of France. A medal of 3,000 francs value.

An extraordinary prize of 6,000 francs will be awarded as a recompense for any progress calculated to increase the efficacy of the French naval forces.

II. MECHANICS.—1. The Poncelet prize of a medal of 2,000 francs value, and a complete copy of Poncelet's works, are awarded to the work contributing most to the progress of the mathematical sciences, pure or applied, published in the course of the ten years preceding the judgment of the Academy. 2. A Montyon prize, a medal of 427 francs value, will be awarded to any one who, in the judgment of the Academy, is most deserving, by inventing or improving instruments useful to the progress of agriculture, the mechanical arts, or the sciences. 3. The Plumey prize, a medal of 2,500 francs value, awarded to the

author of an improvement of steam-engines, or of any other invention contributing most to the progress of steam navigation. 4. The Bordin prize, a medal of 3,000 francs value, will be awarded for a satisfactory solution of the following problem:—To find the means of destroying, or at least seriously diminishing, the annoyance and the dangers arising from the products of combustion issuing from the chimneys of locomotive engines, steamships, and manufactories near towns.

III. Astronomy.—1. The Lalande prize, a medal of 542 francs value, will be awarded to the person who shall have made the most interesting observation, or to the memoir or the work most contributing to the progress of astronomy. 2. The Damoiseau prize, a medal of 5,000 francs value, will be awarded (in 1879) for a solution of the following question:—Revise the theory of the satellites of Jupiter; discuss the observations, and deduce from them the constants they contain, and particularly that which furnishes a direct determination of the rate of light; finally, construct special tables for each satellite. 3. The Valz prize, the proceeds of a sum of 10,000 francs, will be awarded for the most interesting astronomical observation made during the year.

IV. Physics.—The Bordin prize, a medal of 3,000 francs value, will be awarded for a solution to the following:—Various formulæ have been proposed to replace Ampère's law on the action of the elements of currents; discuss these various formulæ and the reasons which may be alleged for giving the preference to one of them. 2. Three Lacaize prizes of 10,000 francs each will be awarded (in 1879) to the works or memoirs which have contributed most to the progress of physiology, physics, or chemistry.

V. Chemistry.—The Jecker prize of 10,000 francs will be awarded to the researches which the Academy judges best calculated to accelerate the progress of organic chemistry.

VI. Botany.—1. The Barbier prize of 2,000 francs will be awarded to anyone who makes a valuable discovery in surgery, medicine, pharmacy, or botany, in connection with the art of healing. 2. The Alhumbert prize, a medal of 2,500 francs value; the subject of this prize is a study of the mode of nutrition of fungi. 3. The Desmazières prize of 1,600 francs will be awarded to the best or most useful writing on the whole or part of cryptogamy published during the year. 4. The Shore prize of 200 francs will be awarded to the author of the best memoir on the cellular cryptogams of Europe, or on the habits or anatomy of a European species of insect. 5. The Bordin prize of 3,000 francs has for its subject the following:—Explain by direct observations and by experiments the influence which the medium exercises on the structure of plant organs (roots, stem, leaves); study the variations which terrestrial plants undergo when raised in water and those which aquatic plants undergo when forced to live in air. Explain by direct experiments the special forms of several species of maritime flora.

In medicine and surgery the Bréant prize of 100,000 francs for a cure for Asiatic cholera still stands.

One or more Montyon prizes are awarded to works or discoveries which show the means of rendering an art or occupation less insalubrious.

The competition closes on June 1 each year. Works sent in are not returned, and the conditions as to the use of mottoes, concealment of names, &c., usual to such competitions are required.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

Science in Schools.—Sir John Lubbock has given notice on an early day to move in the House of Commons that it would be desirable to modify the Code of Education by adding elementary natural science to the subjects mentioned in Article 19, c. 1.

Breslau.—The number in attendance at the University during the present winter is 1,253, divided as follows among the faculties:—Theological, 101; legal, 432; medical, 168; philosophical, 552. The University possesses one of the most valuable libraries in Germany, numbering over 400,000 volumes and several thousand valuable manuscripts.

Bern and Zurich.—The former University is attended at present by 410 students, the latter by 318. Each University includes in its list nineteen female students, most of whom are preparing for medical examinations.

Libraries of German and Austrian Universities.—Most of the German States place annually considerable sums at the disposal of the University libraries. Bavaria gives her universities each 1,000*l.* for this purpose; Saxony grants 1,200*l.* to Leipzig; while in Prussia the sums vary from 600*l.* for Greifswald to 2,000*l.* for Göttingen. In Austria, although the existence of so many different languages in the empire makes special demands on the university libraries, the Government assistance has hitherto been exceedingly limited. We notice, however, that in a late session of the Reichstag a new policy has been adopted, and that the annual grants have been raised to 1,500*l.* for the Vienna library, 1,000*l.* for that at Prague, and 800*l.* for those in the other universities.

St. Petersburg.—On January 1, 1878, the University at St. Petersburg numbered 1,425 students, thirty-seven more than last year. One-seventh are in the department of History and Philology, three-sevenths in Natural Sciences and Mathematics, three-sevenths in Law, and one-forty-seventh in Asiatic languages. The number of professors is ninety-three. The students are mostly very poor, and the pecuniary help given to them by the University amounted during the year to the sum of 12,000*l.*; besides which, a private society of former students paid the fees for eighty-one persons.

Kieff.—The University celebrated, during the past month, the fifty-ninth year of its existence. Although so young it is well equipped with all the necessary adjuncts of a university, and its medical faculty is regarded as the first in Russia. The number of students at present is 773, an increase of 160 on the previous year. The majority are in the medical faculty. As in the other Russian universities, the students are recruited principally from the poorer classes, 203 being freed from the payment of lecture-fees, and 123 in addition receiving stipends amounting in the total to 36,000 roubles. A high school for ladies is at last to be opened at the University.

SCIENTIFIC SERIALS

Reale Istituto Lombardo di Scienze e Lettere. Rendiconti, vol. x. Fasc. xix.—Reduction of chlorates to chlorides without intervention of the so-called nascent state of hydrogen (second part), by M. Tommasi.—On the cooling of pulverulent metallic solids, by M. Cantoni.—On temperature in relation to actual energy and the state of aggregation, by M. Grassi.—Measurement of the resistance and graduation of any galvanometer, by M. Grassi.—A school experiment and means of evaporating rapidly large quantities of liquid, by M. Brugnatelli.

Fasc. xx.—Other experiments on the evaporation of a liquid, by M. Cantoni.—Hypertrophy and hyperplasia, by M. Sangalli.—On the first and most recent appearance in Lombardy of the Beccafico of Provence.

Kosmos, November, 1877.—On the relation of Greek nature-philosophy to modern natural science, by Prof. F. Schultze. Part 1, on the Ionic physiologists and the Pythagoreans.—On a mathematical law applicable to the theory of mutation, by J. Delbœuf.—On the variations of size of coloured floral envelopes, and their effect on the natural selection of flowers, by Hermann Müller.—A turning point in the early history of the human race, by J. H. Becker. Part 1, on the state of things preceding the turning point (before the discovery and use of fire).

December, 1877.—F. Schultze, on the relation of Greek natural philosophy to modern natural science, part 2, discussing Heraklitus and the Eleatic school.—W. Preyer, on the nature of life.—Fritz Müller, observations on Brazilian butterflies, part 3, dealing with the evolution of the Maracuja butterflies, and the phenomena presented by their larvæ, pupæ, and adult forms.—A. Maurer, on the origin of articulate sounds.—J. H. Becker, on the separation and reunion of races.—The number also contains a review of Mr. Darwin's work on the different forms of flowers, by Hermann Müller.

SOCIETIES AND ACADEMIES

London

Geological Society, February 6.—Prof. P. M. Duncan, F.R.S., president, in the chair.—James Adey Birds, Rev. George E. Comerford-Casey, M.A., Lieut.-Col. H. H. Godwin-Austen, Sir Willoughby Jones, Bart., and Henry Richard Ladell, M.A., were elected Fellows of the Society.—The following communi-

cations were read:—On some foraminifera from pleistocene beds in Ischia, by M. Ernest Vanden Broeck. Preceded by some geological remarks by A. W. Waters, F.G.S.—On the influence of the advent of a higher form of life in modifying the structure of an older and lower form, by Prof. Owen, C.B., F.R.S. In this paper the author, after referring to the general question of the modification of the structure of organic forms produced by the action of external influences, indicated that, in connection with this, changes in the nature of the prey of carnivorous animals ought to be taken into consideration. He inferred that cold-blooded aquatic animals formed a much greater proportion of the food of mesozoic than of neozoic crocodiles, and pointed out as connected therewith the well-marked distinction between the amphicœlian and procœlian type of vertebræ respectively characteristic of the two groups. The procœlian character of the trunk-vertebræ better adapts that part of the body to be sustained and moved in air, and may be connected with the incoming in tertiary times of mammalian prey inducing the crocodiles to rush on shore. The mesozoic crocodiles were encased in a much stronger and more complete dermal armour than their successors, doubtless for their protection from the great ichthyosaurs, pliosaurs, &c., which co-existed with them; but as these passed away at the close of the secondary epoch, the armour of the procœlian crocodiles has become more scanty, and the diminution of weight and rigidity thus caused would favour progression in air, and the rapidity of movement required for capturing mammalian prey on land. The difference in the position of the palato-nares, and in other related gular and palatal structures, between the mesozoic and neozoic crocodiles is apparently connected with the power possessed by the latter of holding submerged a powerful mammal without permitting the access of water to the posterior nostrils and windpipe of the crocodile; and hence the author is inclined to ascribe a fish-diet even to those massive-jawed crocodiles from the Purbeck (such as *Goniopholis crassidens* and *simus*), which in some respects might seem fitted to grapple with large and active mammals. The small size of the upper temporal apertures in tertiary and existing crocodiles is regarded by the author as a further proof in the same direction; these apertures are reduced by the progressive increase of the osseous roof of the temporal vacuities, which again is correlated with increase in the bulk and power of the temporal muscles, the main agents in biting and holding. The differences in the length and strength of the jaw, as a rule, testify in the same direction. Further, the fore limbs in mesozoic crocodiles are shorter than in neozoic species, indicating that the former were more strictly aquatic in their habits; the forelimbs in all crocodiles being closely applied to the body during rapid swimming, and small limbs being less obstructive than larger ones. On the other hand, they would be less efficient as a means of progression on land, and hence it may be inferred that the advent in tertiary times of mammals frequenting the water-side, tempting the crocodiles to make a rush upon the land to seize such passing prey, would lead to such strenuous action of the fore-limbs as would account for the increased size and power of those organs in the neozoic species. The author concluded with some remarks upon the influence of the above considerations upon our views as to the generic divisions of crocodiles.—Notes on a supposed crocodilian jaw from the coral rag of Weymouth, by E. Tully Newton, F.G.S., of H.M. Geological Survey. In this paper the author describes what he believes to be a fragment of a lower jaw of a crocodilian, obtained from a greyish brown sandy grit, probably belonging to bed 3 of Messrs. Blake and Hudleston's Sandford-Castle section.—Note on two skulls from the Wealden and Purbeck formations indicating a new sub-group of crocodilia, by J. W. Hulke, F.R.S., F.G.S. The author described a crocodilian skull obtained by Mr. H. Willett, F.G.S., from the Hastings sands near Cuckfield, in Sussex, and identified by that gentleman with *Goniopholis crassidens*, Owen; and another from the Purbecks near Swanage, in the collection of the British Museum, which he further compared with a third specimen from Brook, in the Isle of Wight.

February 15.—Annual General Meeting.—Prof. P. M. Duncan, F.R.S., president, in the chair.—The Secretaries read the Reports of the Council and of the Library and Museum Committee for the year 1877. The Society was described as in an exceedingly prosperous condition, and the income of the year was stated to have considerably exceeded the expenditure. The number of Fellows elected was fully up to the average. The Report further announced the receipt of a bequest of 500*l*. under the will of the late Mr. C. Lambert, which sum, with 150*l*. of the surplus of income, had been invested in consols for the benefit of the Society.—The Wollaston Gold Medal was presented to Dr. Thos. Wright, F.R.S., for his varied palæontological researches.—The President then presented the balance of the proceeds of the Wollaston Donation Fund to Mr. W. J. Sollas, M.A., F.G.S., in recognition of his careful morphological and mineralogical studies upon the fossil Spongida.—The President next handed the Murchison Medal to Mr. Warington W. Smyth for transmission to Dr. Hanns Bruno Geinitz, of Dresden for his researches in the geology and palæontology of the palæozoic and cretaceous formations of Saxony; and the balance of the proceeds of the Murchison Geological Fund to Mr. H. Hicks, F.G.S., for transmission to Mr. Charles Lapworth, F.G.S., for a most important communication upon the Silurian rocks of the South of Scotland, and the graptolites contained in them.—The President next handed to Mr. J. W. Hulke, F.R.S., the Lyell Medal and part of the Lyell Fund for transmission to Mr. George Busk, F.R.S., as a token of the Council's appreciation of his merits as a palæontologist.—The balance of the proceeds of the Lyell Fund was handed to Dr. Oldham, F.R.S., F.G.S., for transmission to Dr. W. Waagen, of Vienna, and who was lately on the Geological Survey of the East Indies. Dr. Waagen's labours in India have commended themselves to the Council on account of their great merit and interest.—The President then proceeded to read his anniversary address, in which he dwelt in considerable detail upon the influence of advanced morphological and zoological investigations upon our palæontological ideas and upon the geological inferences founded upon them.—The ballot for the Council and Officers was taken, and the following were duly elected for the ensuing year:—President, H. C. Sorby, F.R.S. Vice-Presidents: R. Etheridge, F.R.S., John Evans, F.R.S., Prof. J. Prestwich, F.R.S., Prof. A. C. Ramsay, F.R.S. Secretaries: Prof. T. G. Bonney, M.A., Prof. J. W. Judd, F.R.S. Foreign Secretary: Warington W. Smyth, F.R.S. Treasurer: J. Gwyn Jeffreys, F.R.S. Council: H. Bauerman, Prof. T. G. Bonney, M.A., Prof. W. Boyd Dawkins, F.R.S., Prof. P. Martin Duncan, F.R.S., R. Etheridge, F.R.S., John Evans, F.R.S., Henry Hicks, W. H. Hudleston, M.A., Prof. T. McKenny Hughes, M.A., J. W. Hulke, F.R.S., J. Gwyn Jeffreys, F.R.S., Prof. T. Rupert Jones, F.R.S., Prof. J. W. Judd, F.R.S., J. Morris, J. A. Phillips, Prof. J. Prestwich, F.R.S., F. G. H. Price, Prof. A. C. Ramsay, F.R.S., R. H. Scott, F.R.S., Warington W. Smyth, F.R.S., H. C. Sorby, F.R.S., Admiral T. A. B. Spratt, C.B., F.R.S., Rev. T. Wiltshire, F.L.S.

Zoological Society, February 19.—Prof. Mivart, F.R.S., vice-president, in the chair.—The Secretary exhibited the skin of a fine adult cassowary, which had been obtained at Wandammen, on the eastern coast of the Bay of Geelvink, New Guinea, and had just been acquired by the British Museum. The species to which it belonged was believed to be undescribed, and was proposed to be called *C. altijugus*, from its peculiar high-peaked helmet.—Mr. P. Geddes read a memoir on the mechanism of the odontophore in certain mollusca. In this paper the view of Cuvier—that the movements of the radula depend upon those of the underlying cartilages—was substantially revived, arguments being adduced against the more recent theory of Prof. Huxley, that it runs like a chain-saw, the cartilages merely forming a pulley-block. The use of bacteria as food by *Lymnæus* was also described by the author in this paper.—Prof. A. H. Garrod, F.R.S., read some notes on the anatomy of *Tolypeutes tricinctus*, and gave remarks on other *Dasypodidæ*. A new form of *Tolypeutes*, allied to *T. conurus*, was proposed to be called *T. muriei*.—A communication was read from Mr. J. H. Gurney, F.Z.S., containing notes on a specimen of *Polyborus*, lately living in the Society's Gardens.—A communication was read from Mr. D. G. Elliott, F.Z.S., containing the results of his study of the *Pteroclidæ*, or family of sand grouse. Nine species of *Pterocles* and two of *Syrrhaptes* were recognised as composing the family.—Messrs. F. Du Cane Godman and Osbert Salvin gave descriptions of new species of diurnal lepidoptera from Central America.—A communication was read from Mr. R. Bowdler Sharpe, giving an account of a small collection of birds from the Ellice Islands.—Mr. Edward R. Alston read a note on the dentition of *Cuscus*.—A communication was read from Mr. T. F. Cheeseman, containing the description of three new species of Opisthobranchiate mollusca from New Zealand.—Dr. F. Day communicated some remarks on the paper read by Mr. Whitmee

at the last meeting of the Society, on the manifestations of fear and anger by fishes.—A communication was read from the Marquis of Tweeddale, F.R.S., containing an account of a collection of birds made by Mr. A. H. Everett, in the Island of Negros, Philippines.—A second communication from the Marquis of Tweeddale contained the description of a new species of the genus Buceros, proposed to be called *B. semigaleatus*, from the Island of Leyle, Philippines.

Mineralogical Society, February 19.—Mr. H. C. Sorby, F.R.S., president, in the chair.—The president read a valuable and important paper on the determination of the minerals in thin sections of rocks by means of their refractive indices. In this paper he showed how the refractive indices might be determined with great accuracy in sections less than $\frac{1}{500}$ of an inch in thickness, cut for ordinary microscopic purposes.—The Rev. T. G. Bonney then read a paper on some specimens of Gabbro from the Pennine Alps, in which he pointed out the great changes which these rocks had undergone, and their similarity to the rocks of the Lizard district in Cornwall.—Mr. J. H. Collins read a paper on the classification of minerals, in which he advocated a primary chemical and a secondary mixed system of classification. This paper elicited an interesting discussion. Specimens in illustration of their papers were exhibited by the president and by the Rev. T. G. Bonney.—Dr. Foster exhibited specimens of carbonate of bismuth, and other minerals from new Cornish localities.

Photographic Society, February 12.—Annual Meeting.—James Glaisher, F.R.S., president, in the chair.—A silver progress medal was awarded to Capt. Abney, R.E., F.R.S., for having made the greatest advance in the science of photography during the past year.—Capt Abney exhibited a very large positive photograph taken from one of Janssen's negatives of the sun, which were taken by a five-inch telescopic objective of about seven feet focal length, corrected for the chemical, but not for the visual rays.—Mr. Edward Viles exhibited the microphotographic apparatus by which the large photograph (recently seen at the exhibition) of the proboscis of the blow-fly enlarged 200 diameters, was taken, the mechanism and use of the heliostat being minutely described.—Capt. Abney also exhibited and described two other forms of heliostats which he had used.

PARIS

Academy of Sciences, February 25.—M. Fizeau in the chair.—The President announced the opening of a subscription for a statue to M. Leverrier. The following papers were read:—On the carburation of nickel by the process of cementation, by M. Boussingault. Though combining with carbon, nickel does not acquire, like iron, the properties found in steel; nor is the cemented nickel rendered less oxidable. (M. Becquerel is examining its magnetic properties.)—On a new product of oxidation of lead, and on some phenomena of dissociation, by M. Debray. Sesquioxide of lead is transformed, gradually at 350°, and rapidly at 440° into minium, which is not susceptible of being hyperoxygenated in air, or even in pure oxygen. It cannot be said that any compound formed directly will necessarily undergo a limited decomposition at a given temperature.—Imitation of the characteristic cupules and erosions found on the surface of meteorites in an industrial operation, by action of a rapid current of air on incandescent stones, by M. Daubrée. In a new mode of manufacture of Portland cement, the stones raised to white heat are subjected to a current of cold air; the specimen (furnished by M. Hauenschild) showed a surface very like that of meteorites.—Note on a new *brochure* of M. Hirn on music and acoustics, by M. Faye.—On the recent communication of M. Broun, and a note of Mr. Jenkins relating to sun-spots and terrestrial magnetism, by M. Faye.—On telephones with battery, by M. Du Moncel. The author hopefully calls attention to MM. Pollard and Garnier's attempts to strengthen the sound; the sending telephone being on Edison's graphite system, while the receiver is a Bell telephone connected to the induced wire of a Ruhmkorff coil, the battery currents being passed through the primary wire. With pretty strong currents words can be heard 50 or 60 ctm. from the mouth of the telephone, and musical sounds several metres off.—The vibrations of matter and the waves of the ether in ebullition, by M. Favé.—Report on a memoir of M. Haton de la Goupillière.—On the lines generated in movement of a plane figure.—On some consequences of the constitution of the solar spectrum, by M. Cornu. If the sun's outer layer contain, like aerolites, a large amount of iron vapour, this metal probably has an appreciable action on our terrestrial magnetic phenomena. The central part of the earth seems to be formed of much denser materials than the crust, probably metallic matter; and the probable common origin of bodies of the solar system seems to point to iron being largely present, which would explain the earth's action on the magnetic needle. Again, the solar protuberances may correspond to illumination by induction (large magnetic masses being in rapid motion) of rarefied gaseous masses—an illumination easily produced in our laboratories by means of the weakest mechanical actions.—On differential actions of the first order and the first degree, by M. Darboux.—On the temporary variation of permanent magnetism, by M. Gaugain.—When a system (tube and core, or even full bar) magnetised at ordinary temperature is raised to 300° or 400°, the weakening of the magnetism is not exclusively due to a part of this magnetism being destroyed; it arises in part from the *inverse* magnetism being developed in the tube under influence of heat.—On the action of fluoride of boron on organic matters, by M. Landolph. — Transformation of bromated hydrocarbons of the series of ethylene into bromides of acids of the fatty series, by simple addition of oxygen, by M. Demole.—Analysis of the sulphurous waters of Aix, in Savoy, and of Marlioz, by M. Willm.—Action of oxygen on anatomical elements, by M. Bert. These elements are nourished by reducing the oxyhæmoglobic combination (and similarly to the butyric ferment); but if their substance be penetrated artificially with chemically free dissolved oxygen, they become incapable of taking oxygen from the matter which furnished it before, and die by a kind of asphyxia; in a word they are *anærobies*.—On local variations of the pulse in the forearm of man, by M. Mosso. He experiments with a *hydrosphygmograph*, which is a modification of his plethysmograph. The effects during intellectual effort, sleep, &c., are described.—On lactic fermentation of sugar of milk, by M. Richet. It seems that the gastric juice, by its dissolving action on caseine and perhaps another action yet unknown, gives lactic fermentation a surprising activity and rapidity.—Classification of Cestoïdes, by M. Perrier.

VIENNA

Imperial Academy of Sciences, January 3.—On the velocity of propagation of spark-waves, by MM. Mach, Tumlirz, and Kögler.—On orthogonal substitutions and some related to them, by M. Igel.—On ballooning, by M. Ettalp.—Three experiments with the telephone, by M. Sacher.

CONTENTS

THURSDAY, MARCH 14, 1878

THE LOCUST PLAGUE IN AMERICA

The Locust Plague in the United States; being more particularly a Treatise on the Rocky Mountain Locust, or so-called Grasshopper, as it occurs East of the Rocky Mountains, with Practical Recommendations for its Destruction. By Charles V. Riley, M.A., Ph.D., State Entomologist of Missouri, &c. With 45 Illustrations. (Chicago: Rand, McNally, and Co., 1877.)

THE greater part of this treatise has already appeared in the Entomological Reports published annually for some years past by Mr. Riley, as State Entomologist for Missouri, in which the information was given piecemeal from time to time as it was acquired. The whole is now brought together in a connected and systematic form, and we have in it a very complete and valuable treatise on the different kinds of locusts, whether species or varieties, which have proved destructive in North America. Ever since the discovery and colonisation of that continent the new settlements have been from time to time subject more or less to scarcities resulting from the invasions or migrations of these insects. These have gradually, however, become scarcer and scarcer, and confined more and more to the interior as the insects retreated before the advancing wave of civilisation and cultivation, until now their ravages do not extend eastwards beyond the 16th or 17th degree of longitude west of Washington; in other words, the regions lying to the east of the Mississippi are now nearly free from them, and it is only in those lying to the west of that river that their propagation and migrations take place on such a scale as seriously to affect the property and prosperity of the settlers. It is not that the species originally inhabiting the eastern coast have been gradually pushed back to the interior, but that the species peculiar to it have been reduced in number in the cultivated districts, and their *rôle* has been successively taken up by other species lying more inland as civilisation has gradually advanced. The species on which that mission has now devolved are two or three that have their home and permanent breeding-place in the Rocky Mountains—we say *permanent* in contradistinction to temporary breeding-place, because when they make their migrations, they often rest and breed at its furthest limit, the brood returning in the following year to the country from which their parents came, although not necessarily by the same route. The route by which they have hitherto invaded the countries to the east of their proper home in the Rocky Mountains has been from north-west to south-east. That by which the fresh-bred swarms sprung from the invaders have made their way back again next year, has been from south-east to north-west, but not absolutely in the same line by which their parents came, but either parallel to it or slightly divergent. Their course of invasion has been carefully traced for many years by Mr. Riley and others, and the fact of their return on their footsteps in this way is beyond question; but it is also beyond doubt that the new brood does not go back so strong or so numerous as their parents came. Their constitution appears to be sapped by the change of climate or condition of life; they are feeble and infested by parasites, so that a large proportion of them die a natural death—a consideration which doubtless explains why the vast swarms which have passed from one country to another in all ages and in all quarters of the globe, seem never to have made good a permanent footing in the country they have invaded; at all events never in numbers at all corresponding to the force of the intruders. This is no doubt but small consolation to settlers living on the borders of a locust-stricken land, but it is better than none—they would be still worse off if the locusts were to remain as a permanent incubus instead of only coming occasionally as a ravaging horde.

Of the amount of injury done by the invading hosts, especially during the more recent invasions of 1873 and following years, Mr. Riley gives a striking account. Where a territory of hundreds of miles in extent is struck with desolation in a few days or weeks through the ravages of an insect, it is scarcely possible to speak of it without exaggeration, and some qualification will almost certainly have to be made upon any estimate of the amount of damage supposed to have been sustained, especially when, as here, we know how little the data on which the estimates are founded are to be relied on. In Great Britain we have now an elaborate machinery by which reliable agricultural returns are obtained; the land, or most of it, has been measured and mapped out; the best means are taken to obtain true and correct returns, and when obtained they are checked by competent and trustworthy experts; so that no error of any magnitude can well creep in without detection. It is otherwise on the prairies west of Missouri. The admirable United States Surveys, although sufficiently perfect and on a sufficiently large scale to answer all general purposes, have no pretensions to such detail as we have adopted in our Ordnance Survey Maps, and no attempt is made to give the acreage of the different plots in cultivation (which, besides, would be useless, as it is an uncertain quantity, varying every year). At the best, therefore, there are no other means of estimating either the amount in cultivation or the amount of damage inflicted on it than an empirical estimate furnished by the farmers themselves, a mode of calculation open to many objections, and requiring much allowance. Still, giving the widest margin, enough remains behind to satisfy the hungriest appetite for startling results. If actual starvation did not come in the locusts' train, poverty and distress did. In 1874 the loss to three exposed, although thinly-peopled, states, Wyoming, Dakota, and Montana, is said to have been fifty millions of dollars; and in 1875 it was calculated that about three-quarters of a million of people were made sufferers on a strip of about twenty-five miles broad along the banks of the Missouri, from Omaha to Kansas.

Mr. Riley gives many statistics on such points. His information regarding the habits of the locusts and their enemies, and the best way of dealing with them, is also ample; and his scientific descriptions and natural history of the species in all their stages leave nothing to be desired. He even touches upon their value as food either with or without wild honey, and gives the results of his experience as to the best mode of cooking them. During a visit that he paid to this country, some two or three years ago, he brought some dried potted specimen with him; but that was scarcely fair play to the locusts,

and we shall not say what we thought of them. Let us still be just. If we are to condemn them, let it only be after a trial when they are fresh and good. We have indeed tried them in their native country, pounded up with acorns and mashed into balls by the digger Indians of California; but then acorns would destroy any dish for civilised food, so that we prefer to leave the question of their culinary merits an open one for some gastronomic jury, stipulating only for the right of challenging Mr. Riley, as one of its members, on the score of undue favour and partiality arising from too intimate an acquaintance and familiarity with the individuals under trial.

A further contribution to the subject treated of by Mr. Riley has reached us in the shape of the first two *Bulletins* of the United States Entomological Commission.

ANDREW MURRAY

ABNEY'S TREATISE ON PHOTOGRAPHY

A Treatise on Photography. By W. de Wiveleslie Abney, F.R.S. (London: Longmans and Co., 1878.)

ALL those interested in this most attractive study will welcome Capt. Abney's treatise on photography. Those who wish to become acquainted with the scientific principles on which the practice of photography depends will find in the opening chapters a clear and concise description of the theory of sensitive substances, and of the action of light on various compounds, whilst by studying the closing chapters of the volume they will be able to make themselves acquainted with the present state of our knowledge on the important subjects of actinometry, photo-spectroscopy, and the interesting discoveries made by the author and others on the sensitiveness of different salts, and the methods employed for obtaining pictures of the various portions of the spectrum. On the other hand, the artist photographer will find ample matter for interest in the chapter in which Capt. Abney most successfully lays down the rules which must guide the production of an artistic picture, pointing out the special difficulties under which the photographer lies in the choice of subjects in order to avoid incongruity or inartistic massing of light and shade, and showing the best mode of lighting and arranging the picture by choosing the right point of view for the camera. As an illustration of Capt. Abney's happy style and power of artistic treatment, we may quote the following description of a landscape:—

"In the next picture, we have the distance, or perhaps more strictly speaking, the middle distance as the point of interest. The horizon line is kept in the weakest part, the centre, of the picture. The trees in the foreground are so grouped that they frame the view with dark masses, relieved by the light foliage of some of the nearer bushes and shrubs. The foreground finishes at a distance of about $\frac{1}{4}$ from the bottom. More of it would take away from the value of the middle distance, as it would place it in the weakest part of the picture—viz., centrally; less of it would have rendered the picture bald, and have cut off part of the deeper shades which are so valuable in giving the effect of distance to the stream beyond. This picture would have been spoilt had the camera been so placed as to give more top foliage, since the bough which now partially crosses the picture at about $\frac{2}{3}$ the height, would have caused an ugly division, and also the tops of the distant trees, and the sky would have appeared. This latter, in views such as that under criticism, is objectionable, as patches of white give the eye an inclination to wander off towards it, and it would have been an insufficient precaution to have printed in clouds from another negative, owing to the difficulty that would exist in subduing at the same time the lights on the leaves of the near trees. As it is, the picture is in pictorial focus. By placing the stream to the right or left, the balance would have been wanting, and its general direction would have been altered to such an extent as to have given a feeling that it was a subsidiary part of the picture instead of an essential."

Another important section of the work is devoted to the necessary, but unavoidably dry descriptions of the very numerous photographic processes and manipulations now in vogue, of the construction of apparatus, and a statement of the general laws of geometrical optics so far as concerns the principles on which the construction and use of photographic lenses depend. On all these subjects we find Capt. Abney's statements clear and concise.

Then again no book on photography would be complete without an explanation of the various processes of photo-lithography and photo-engraving, and accordingly we find a short account of the more important of these interesting methods of reproducing photographic effects. To one of these photo-relief printing processes, that discovered by Warnerke, with, we believe, the author's co-operation, we would especially draw attention, the picture being remarkable for the beauty and delicacy, as well as for the force and depth of its tones. The details of this process are not yet published; it cannot, however, be doubted that it is capable of producing the finest effects of a steel or copper-plate engraving.

It is, however, the scientific side of Capt. Abney's book which will especially interest the readers of NATURE. The explanation of the effect of vibration as setting up chemical change in the molecule is clearly set forth in Chapter III. The case in which the atoms are in a stable though verging on an indifferent equilibrium as with the sensitive mixture of chlorine and hydrogen, being well illustrated by the equilibrium of a frustum of a pyramid standing base uppermost on as narrow section of the base as we please. In these cases a very small amount of work is needed to make the systems take up more stable positions. Then "extending our previous illustration, supposing we had a row of such frusta of pyramids, and that it was found that one pellet of a number (all being of equal weight) when striking one frustum with a certain velocity was able to cause it to fall, and also that in every case the accuracy of aim was undoubted, and that in falling one frustum did not strike its neighbour, then at any interval after the commencement of a bombardment the amount of work expended in projecting the pellets could be compared by simply counting the number of frusta which had fallen" (p. 12). The question of the action of vibrations synchronous with the oscillations of the molecule on the stability of the molecule is next discussed, and the explanation rendered clear by a description of Rankine's well-known contrivance of the heavy and light pendulums. The difference between the decomposition of explosives and of bodies employed for photographic purposes in respect to the nature of the disturbing vibrations is thus pointed out. Explosives are affected by long wave rays, photographic actions as a rule being only set up by waves

of short length. A description of the remarkable negative or reversing action effected by the red rays on the sensitised plate, first observed by H. Draper, is found in Chapter XXXIV. A partial explanation of this very interesting fact is given by the results of experiments lately made by Capt. Abney (*Phil. Mag.*, January, 1878), which show that the image can be rendered undevelopable by the oxidation of the altered silver compound forming it. Chastaing has also recently announced that he finds rapidity of oxidation promoted by the red rays. It is thus easy to see that the sensitive salt of silver which had been altered in chemical composition by a slight exposure to white light, would become oxidised where the red rays fell upon it, and that, in consequence, where the dark Fraunhofer's lines in the ultra red spectrum fell, the plate would remain unaffected and the presence of these invisible bands would become apparent.

Another subject of great interest, that of the production of coloured photographic images, is being attacked experimentally by Capt. Abney. The results of the experiments in this direction by Becquerel and Niépce de St. Victor are well known, and many of the visitors to the Loan Exhibition will remember the coloured photograph of dolls dressed in coloured clothes shown by the latter chemist. Abney believes that these tints are rather to be ascribed to different stages of oxidation of the film, than, as has hitherto been supposed, to the colours of thin plates. Then, again, on the subject of the recent discoveries by Vogel, Waterhouse, and others, as to the production of a film sensitive to the red rays by the addition of a red dye to the collodion, Capt. Abney has something original to say. He has found that the addition of certain resins, albumin, and other organic bodies, when combined with silver, tends to lower the limit of the impressible spectrum and the place of maximum sensibility; so much so, indeed, that it is possible to obtain an unreversed impression of the thermal spectrum. A beam of light was allowed to pass through ruby glass, and the spectrum was then thrown on a resinised plate in the ordinary manner, and a visible impression of rays in the red was obtained far beyond the limit of the visible spectrum, as is seen by a figure in the volume.

Enough has been said to show the value of Capt. Abney's treatise both from the scientific and artistic points of view. If we are to speak on the part of amateur photographers we would express a hope that the subject of the explanation of defects in negatives and their cure may be more fully treated of in the next edition. It is perhaps difficult for an accomplished photographer like the author to appreciate the difficulties of a beginner in the art, but the mere mention of some of the defects met with in negatives does not always, as the author states, suggest the cure to minds unfamiliar with the niceties of manipulation and procedure which to the expert come as a matter of course. We congratulate Capt. Abney on the appearance of this most useful volume. H. E. R.

OUR BOOK SHELF

Archæological Researches at Carnac, in Britanny. By James Miln. (Edinburgh: David Douglas.)

THIS beautiful book reflects great credit on its author. It would be difficult in the recent literature of archæology to point out a more salient example of the great gain which is sure to accrue to that branch of science from the introduction of the true scientific spirit, and attention to details. Carnac, in most people's minds, is associated with Druidical circles, and it was to see the wonderful alignments there that Mr. Miln visited the place. But while in the region the author was particularly struck with the remains belonging to a very different time, which were pointed out to Mr. Miln by a French archæologist. They are termed the mounds of the Bossenno. With characteristic energy Mr. Miln, who was determined to explore, endeavoured to buy in order that he might explore the better. In this, however, he was foiled, beset by too many difficulties. The permission to explore which he subsequently obtained does not appear to have been a very complete one, and after this big book full of matter our author states that much still remains to be done.

The results of the excavations so carefully carried out by Mr. Miln show that we have here the remains of a Gallo-Roman settlement, and he has reconstructed for us out of its ashes the condition of the people in former times. He has been enabled to give us precise information as to their food and the degree of luxury in which they indulged. Their worship, their ceremonies, and modes of manufacture, and the exact times between which the colony was in a flourishing condition are also fully discussed. He traces the local worship of Venus Genetrix, at the Mont St. Michael, in a most interesting manner. One of the oldest constructions which remains in Britanny is the chapel of St. Agatha. On the vault of the apse a few years ago was discovered one of the most curious frescoes which the Romans have left in Britanny. It represents Venus rising from a blue sea, surrounded by fishes and dolphins. This church, now dedicated to St. Vener, is styled "Ecclesia Sancti Veneris" in a twelfth century charter.

The beautiful illustrations comprise not only almost everything which was found, but large coloured plates of the chief coloured designs rescued here and there.

All antiquaries will do well to lay to heart the remarks on ancient pottery made by Mr. Miln *à propos* of his finds in the excavation which he designates A. He shows abundantly how much caution is requisite in such inquiries and how a careful sifting of facts brings order into what at first sight appears a hopeless jumble of objects. It is curious that some of the pottery he found there is similar to some in the Guildhall Museum, which was found at a depth of forty-two feet, when the ground was excavated for the foundations of the Royal Exchange.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

The Telephone

IN his interesting paper (NATURE, vol. xvii. p. 283) Mr. F. J. M. Page communicated as the result of his experiment to obtain indication of currents from a telephone by means of a mercury capillary tube, that the motion of the mercury was "always towards the end of the capillary." In the repetition of this experiment before the Physical Society on Saturday, February 16, Mr. Page found, however, that the mercury moved persistently in the opposite direction.

In the December (1876) number of the *Phil. Mag.* I showed that the motion of mercury in contact with dilute acid through which a current passes, is due to rapid circulation of the mercury set up by deoxidation of one part of its surface whilst another part is being oxidised; and that a very slight difference in the degree of oxidation is sufficient to produce an appreciable electromotive force.

When the mercury tube of the so-called electrometer is set up, the two surfaces of the mercury in contact with the acid are, I believe, almost always electrically unequal, that in the capillary being less oxidised than the other, and therefore positive to it. When the circuit is closed, a feeble current passes which, if it were strong enough, would move the mercury forwards. When a telephone is in action in the circuit, its equal and opposite currents combine alternately with the mercury current which strengthens the impulses in one direction and weakens those in the other; so that, whilst the sum of the telephone and mercury currents may be able to move the mercury in one direction, the difference of these currents is not able to move it in the other. Hence, I believe, arise the motions in question.

It of course follows that if, by accident, the potentials of the two mercury surfaces were equal, the telephone currents would produce no movement whatever in the mercury. Moreover if by variation of temperature, or by difference of strength of acid at the contact faces, or otherwise, the mercury surface in the capillary is rendered negative to the other surface, the accidental current set up will be in the opposite direction, and the tendency will be for the mercury to recede in the tube, as was observed in the experiment performed before the Physical Society.

Mr. Page's experiment will, I have no doubt, suggest a means of deducing the potentials of the telephone impulses.

ROBERT SABINE

AFTER reading the experiments of Prof. Forbes on the telephone, in NATURE, vol. xvii. p. 343, it occurred to me, as probably it has done to others, that this instrument might be employed in comparing the electrical resistances of wires. Accordingly, two weak cells were connected with the ordinary form of Wheatstone's bridge, and the telephone placed in the position usually occupied by the galvanometer. The current was rendered intermittent by a small electromagnetic apparatus belonging to an electric bell; the bell itself having been detached, the intermitter was placed in a separate room, and connected by long wires with the battery and bridge. The German silver wire of the bridge, having a resistance of ·2 ohms, was further lengthened at each end by resistance coils of ten ohms, and it was found that with a little practice one could easily compare two resistances of about two ohms within at least 1,000th of the true ratio.

It was found better to attach the sliding piece to the battery rather than the galvanometer, and it was exceedingly curious to notice the effect of moving the sliding piece so as to gradually diminish the difference of potential at the two terminals of the telephone, the sound diminishing until at last there seemed to be only a slight *uneasiness* produced in the ear, which ceased whenever the contact between the sliding piece and the German silver wire was broken. I have no doubt whatever that with a more delicate instrument than the one employed, which was apparently not nearly so sensitive as that used by Prof. Forbes, one could compare with considerable accuracy electrical resistances in this manner. Of course the telephone could also be employed instead of the galvanometer, in comparing the electromotive forces of batteries, and it is my intention to make more experiments in this direction.

By using a tuning-fork made to vibrate by electricity and a Helmholtz's resonator in conjunction with the telephone, the accuracy of testing may no doubt be largely increased.

HERBERT TOMLINSON

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1. IF the cavities above and below the iron disc of an ordinary telephone are filled with wadding, the instrument will transmit and speak with undiminished clearness.

2. On placing a finger on the iron disc opposite the magnet, the instrument will transmit and speak distinctly. It only ceases to act when sufficient pressure is applied to bring plate and magnet into contact.

3. Connecting the centre of the disc by means of a short thread with an extremely sensitive membrane no sound is given out by the latter when a message is transmitted.

4. Ten telephones were connected as represented in the following diagram, on the principle of a battery joined for surface or quantity.

From transmitter—

A, B, C, &c., telephones.

On receiving a message from the transmitter *it could distinctly be heard through any of the ten instruments, although the current had been split up ten times.* (I have no doubt that a greater number of telephones might thus be joined with almost equal effect; from want of instruments I have not been able to find out the limit.)

The following experiments were made with a double telephone, constructed by a battery of horse-shoe magnets with iron cores at their ends. The wires on the bobbins were wound in opposite directions, as on an ordinary electro-magnet.

5. *On connecting the similar poles of the coils* (*as* + *and* +) *and joining the remaining similar poles* (*as* — *and* —) *to line wires the instrument both transmitted and spoke with equal distinctness.*

6. On placing the armature on the horse-shoe magnet no loss of power was perceptible in either transmitting or receiving, nor was there any increase of power on augmenting the number of magnets.

7. If the inner and outer coils of an induction coil are respectively connected with a transmitting and receiving instrument, sound can be distinctly transmitted in either direction.

8. If an ordinary Leyden jar is interposed in the line wire, one end being in contact with the inner, the other with the outer coating, sound can be transmitted, but it is much weakened in strength.

9. Bringing the iron cores of the double telephone in contact with the disc and pressing with the fingers against the plate on the other side, a weak current from a Daniell cell produced a distinct click in the plate, and on drawing a wire from the cell over a file which formed part of the circuit, a rattling noise was produced in the instrument.

Experiments No. 1, 2, 3, and 9 tend to show the absence of mechanical vibration. For the Experiments Nos. 4 and 5 I fail to find a reasonable explanation. No. 6 shows that strength of the magnet has nothing to do with the force of the sound produced, the latter being simply the result of a difference of two opposing forces. Nos. 7 and 8 require no explanation.

The above notes are taken from a paper read by me before the Priestley Club on February 16.

Bradford Grammar School AUREL DE RATTI

IN NATURE, vol. xvii. p. 164, there was a notice of a telephonic alarum in the shape of a tuning-fork. This, however, requires a fixed and special telephone. The following method of attracting attention requires neither. I venture to send it you, as I have seen no notice of any one having tried it; but I can scarcely believe it to be the case, as the thing would suggest itself to any one studying the instrument. It is to include a magneto-electric machine in the circuit, when turning the handle produces a series of taps in the telephone audible at a considerable distance. I have not tried it for any long distance—merely fifty yards. The magneto-electric machine was placed in the observatory, and the telephone, or rather a battery of three telephones, in my study. The noise was heard at the further end of my dining room, the door of which faces that of the study.

Rugby A. PERCY SMITH

EXPERIMENTING with a pair of telephones the other day, I thought I would try if it were possible to utilise underground pipes as conductors. I therefore connected one terminal of each instrument with the gas and the other with the water-pipes, in two houses placed about thirty yards apart, and found that it was possible to carry on conversation by means of the instruments thus connected. The voices were not as distinct as if wire had been used, but singing was very plainly heard. I have not had the opportunity of trying a longer distance; perhaps some of your readers may test the matter further.

Bury, Lancashire WILLIAM STOCKDALE

"Mimicry in Birds"

OWING to the special meaning of late attached to the word "mimicry" by naturalists, the above heading seems liable to mislead when applied to the fact mentioned by Mr. J. Stuart Thomson (page 361). In answer to his inquiry perhaps you will allow me to quote the following from the fourth edition of Yarrell's "British Birds" (vol. ii. p. 229) with respect to the starling.

"Its song is as imitative as that of the vaunted Mocking-bird,

and in nothing perhaps is it more grateful than in the reminiscences it brings to our homes of its wilder associates far afield; for Starlings consort with many kinds of birds, learn their notes and frequently mingle them in their own strain."

And then as a foot-note :—

"Thus the well-known wail of the Lapwing, and the piping note of the Ringed Plover may be heard in places wholly unsuited to the habits of those birds. Messrs. Matthews mention Starlings imitating the cry of the Kestrel, Wryneck, Partridge, Moorhen, and Coot among other birds (Zool. p. 2430). Saxby says that in Shetland the notes of the Oyster-catcher, Golden Plover, Redshank, Curlew, Whimbrel, and Herring-Gull, are perfectly mimicked. Mr. Hooper, of Upton near Didcot, informs the editor that Starlings in that neighbourhood will render exactly the characteristic cry of the Quail and the Corn-Crake. The common sounds of the poultry-yard are often copied with more or less accuracy, and a Duck may be heard to quack, a Hen to cackle, and a Cock to crow from the topmost bough of a tall tree."

It follows that if a Starling can so well imitate the notes of the above-named birds, it would have still less difficulty with those of species much more nearly allied to it, as the Blackbird, Chaffinch, and Sparrow. ALFRED NEWTON

Magdalene College, Cambridge, March 9

The "Geographical" and the Public

QUITE accidentally this evening I noticed in NATURE that Capt. Evans was to read a paper on the Magnetism of the Earth, before the Royal Geographical Society at the London University. Having devoted considerable attention to the subject I was desirous of hearing the paper and hurried up to town. I found, however, that I could not obtain admittance without an order. I offered payment but that was useless. I explained to the doorkeeper that I had come a long distance, was most anxious to hear the paper, and did not know until then the terms of admission, otherwise, as many of my friends are Fellows, I would have supplied myself with the necessary order.

I offered my card and suggested that it might be sent in to Sir Henry Rawlinson, to whom I was known, or to the Secretary or some other official, but to all my endeavours there was a curt, not to say pert, reply.

It occurred to me that if I waited a short time some friend might possibly make his appearance and help me in my "pursuit of knowledge under difficulties." I had not waited many moments when I noticed the door-keeper despatch on an errand a lad who supported him. I was weak-minded enough to imagine he had relented, and that some official would come to my aid. An official did certainly come back with the lad—it was a policeman! who gave me a look which I interpreted to mean, "If you don't be off I'll 'run you in.'" A few words in a very low tone passed between the doorkeeper and himself, and as I had no desire to spend the night in Vine Street station, I departed, feeling that this was an *argumentum ad hominem* which I could not resist. X.

Temple, March 11

Hearing and Smell in Insects

ALL that I have observed leads me to believe that any sensitiveness shown by insects to sound is due to a diffused sensibility to vibration rather than to a differentiated sense like our own. This will sufficiently explain the behaviour of J. C.'s moths (NATURE, vol. xvii. p. 45), and my own larvæ (NATURE, vol. xvii. p. 102). In the one case the ringing glass, and in the other the vibrant wood of the feeding-box communicated the alarm. If anyone, an hour after his kitchen has been left in darkness and quiet, will enter it as gently as possible, without shoes or light, and then, having no contact with anything, other than the unavoidable one of his sock-muffled feet with the floor, will speak suddenly and sharply, I believe he will find that not a cockroach shows any signs of alarm. If, on the other hand, he should drop something heavy abruptly, or enter with his usual step in boots, there is a stampede; but even then nothing to compare with the commotion caused by the introduction of light.

As to smell, there can be no doubt, it seems to me, that it is a very finely-differentiated sense; residing, I suspect, to a great extent, in the antennæ, and probably capable of detecting qualities in substances of which our own analogous sense gives us no warning. The ichneumon flies are an example in point. One of the larger of these alighted inside my open window in the sunshine this afternoon, and I noticed, as often before, the incessant play of his antennæ as he hunted restlessly to and fro, apparently in search of larvæ, or pupæ, concealed under the wood. As the prey of some members of this tribe are always so hidden, and the egg is accurately laid therein, by means of the long ovipositor, without the aid of sight, some other sense, in great perfection, must guide them in their quest. But here is a quite conclusive instance.

I saw in Athens, March, 1864, in the collection of Mr. Merlin, then our vice-consul there, placed in juxta-position in one drawer in his cabinet, a wasp and spider, of which he told me that that species of spider is the habitual prey of that species of wasp, and that he hunts him by scent, nose down, precisely like a hound. He witnessed himself the chase from beginning to end in the case of the actual specimens I saw. It occurred in his own house, and was continued for some time, and across, as I understood him, more than one room. The spider, as soon as he found himself marked down, showed the greatest terror, running hither and thither, with many doubles and turns. These the wasp—a long, thin-bodied variety—followed accurately, turn by turn, never quitting the spider's track for an instant, recovering, when at fault, like a dog, until, after an exciting chase, he seized his exhausted prey, and the keenly-interested human observer secured both pursuer and victim. HENRY CECIL

Bregner, Bournemouth, March 2

OUR ASTRONOMICAL COLUMN

THE TOTAL SOLAR ECLIPSE OF JULY 29.—Prof. Newcomb has lately issued empirical corrections to Hansen's Lunar Tables, which he proposes to employ in the American Ephemeris for 1883. The errors of the tables have now attained such magnitude, and exhibit so steady an increase, that it becomes necessary to apply corrections, even though they may be of the otherwise unsatisfactory nature of empirical quantities, and it is probable that Prof. Newcomb may not be the only superintendent of an ephemeris who will adopt this course pending the formation of new lunar tables at, it may be hoped, no distant period.

At the time of the total solar eclipse which traverses the United States in July next, Mr. W. Godward finds the correction of the longitude of the moon deduced from Hansen's tables to be $-9''\cdot5$, and the correction of the latitude $+0''\cdot9$, according to Newcomb. Applying these corrections to the moon's place, and adopting Leverrier's diameter of the sun, with a somewhat reduced diameter of the moon from that given by Hansen's tables, which corresponds well in the calculation of eclipses, the following equations are found, which may be expected to give the times of beginning and ending of the total phase with considerable accuracy for any point not far distant from Denver, Colorado, the most important place traversed by the belt of totality.

$$\cos w = 59\cdot7050 - [1\cdot85211] \sin l + [1\cdot71204] \cos l, \cos (L + 216° \; 48'\cdot2)$$
$$t = 9\text{h. } 54\text{m. } 34\cdot2\text{s.} \mp [1\cdot93963] \sin w - [3\cdot56966] \sin l$$
$$- [3\cdot82402] \cos l, \cos (L + 256° \; 25'\cdot6).$$

Here l is the *geocentric* latitude of the point, L its west longitude from Greenwich, to be used with a *negative* sign, and the quantities within square brackets are logarithms; t is the Greenwich mean time of beginning or ending of totality, according as the upper or lower sign of the second term is used, $[1\cdot93963] \sin w$ representing the semi-duration of the total phase; and applying the longitude of the place for which we are calculating in the usual way, the local mean times result.

As an example of the method of using formulæ of reduction similar to the above, which is frequently a matter of doubt to the uninitiated, we may find from them the local mean times of beginning and ending of the total eclipse in 106° 14′ W., and 40° 23′ N., which, according to the *Nautical Almanac* elements, is the position of the central eclipse at 10h. 28m. Greenwich mean time.

The reduction of the geographical to the geocentric

latitude (which may be taken from tables found in many astronomical works—in Loomis's "Practical Astronomy," for instance, or in the *Berliner Jahrbuch* for 1852) is $-11'\cdot4$ and consequently $l = 40^\circ\ 11'\cdot6$.

W. long.	$-106^\circ\ 14'$	... ...	$-106^\circ\ 14'$
Constant	$+216^\circ\ 48'\cdot2$	... ...	$+256^\circ\ 25'\cdot6$
A	$+110^\circ\ 34'\cdot2$	B ...	$+150^\circ\ 11'\cdot6$
Constant ...	$-1\cdot85211$	Constant ...	$+1\cdot71204$
Log. sin l ...	$+9\cdot80981$	Log. cos l ..	$+9\cdot88302$
		Log. cos A ...	$-9\cdot54574$
	$-1\cdot66192$		
			$-1\cdot14080$
No.	$-45\cdot9111$		
	$-13\cdot8293$	No.	$-13\cdot8293$
	$-59\cdot7404$		
Constant ...	$+59\cdot7050$		
Sum	$-0\cdot0354$		
Log. cos w ...	$-8\cdot54900$		
w	$92^\circ\ 1'\cdot7$		

Constant	$1\cdot93963$	Constant	$-3\cdot56966$	Constant	$-3\cdot82402$
Log. sin w	$9\cdot99973$	Log. sin l	$+9\cdot80981$	Log. cos l	$+9\cdot88302$
				Log. cos B	$-9\cdot93837$
	$1\cdot93936$		$-3\cdot37947$		
					$+3\cdot64541$
No.	$87^s\cdot0$	No. ...	$-2395^s\cdot9$		
			$+4419^s\cdot9$	No. ...	$+4419^s\cdot9$
			$+2024\cdot0$		

	$+\ 33^m\ 44^s\cdot0$
Constant	$9^h\ 54^m\ 34^s\cdot2$
G.M.T. of middle of totality	$10^h\ 28^m\ 18^s\cdot2$
Long. W. *in time*	$7^h\ 4^m\ 56^s\cdot0$
	$3^h\ 23^m\ 22^s\cdot2$
Semi-duration	$1^m\ 27^s\cdot0$
Totality begins	$3^h\ 21^m\ 55^s\cdot2$ } Local M.T.
,, ends	$3^h\ 24^m\ 49^s\cdot2$ } Local M.T.

The duration of the total eclipse is therefore 2m. 54s. and the middle at 10h. 28m. 18s. G.M.T.; the *Nautical Almanac* has 2m. 55s. and 10h. 28m., so that the corrections which we have introduced into the calculation have had but very insignificant effect. It may be added that when cos w is found greater than unity, and therefore impossible, the place for which the calculation is made is beyond the zone of totality.

THE STAR LALANDE 31266–7.—Mr. J. E. Gore writes with reference to this object, which in the reduced Catalogue of Lalande appears as a *first* magnitude, with position for 1800 (by a mean of the two observations), R.A. 17h. 1m. 22·36s., N.P.D. 45° 57′ 21″·6, and suggests that it may prove to be a remarkable variable, since it is "shown 6m. in Flammarion's edition of Dien's Atlas." The introduction of such a star into the Catalogue is easily explained, and in fact is rather an old story. The two observations on p. 353 of the "Histoire Céleste" were really observations of Capella (*Chèvre*, as Lalande calls it) *sub polo*, and were erroneously reduced to 1800, as though the star had been observed above pole. It is singular that these two observations should have given rise to the introduction of a spurious star, since there are eight other observations of Capella *sub polo* in the same year, 1790, between April 19 and July 24, of which no use has been made by the computers in their reduction of the stars of the "Histoire Céleste." There is no sixth magnitude in the position of the reduced catalogue.

MINOR PLANETS.—There are yet two more members to be added to this group—No. 184, discovered by Herr Palisa at Pola, on February 28, and No. 185, by Prof. Peters, at Clinton, U.S., on the following night. The *Berlin Circular*, No. 86, contains elements of Nos. 181–183.

BIOLOGICAL NOTES

INLAND FISHERIES, AMERICA.—We are indebted to Mr. Theodore Lyman, one of the United States Commissioners on Inland Fisheries, for an early copy of the Twelfth Annual Report to the Commonwealth of Massachusetts. Among other interesting facts we gather from it that there is still a mystery about the young of the Californian salmon (*Salmo quinnat*), for, notwithstanding the hundreds of thousands that have been put into New England waters, no one has been able to say with certainty that a single smolt has been seen. In reference to the true salmon (*S. salar*) it is pleasant to know that the return of mature salmon to the waters of the Merrimac last year (1877) commences a new era in the history of fish-culture in America. From the observations taken many of these mature salmon were from eight to ten pounds weight each, and were from the parr put into the river in 1873; but some were seen from fifteen to eighteen pounds weight, and these were most probably the result of the first parr put into the river in 1872. From 1872 to 1876 upwards of 830,000 parr were put into the river, and hundreds of fine fish were seen passing up in the spring of 1877; and, so says the report, "it will be seen that what we have so long fought for, what the mass of people here have generally considered mere theories, visions of men who suffered from fish on the brain, has been fully substantiated. It is true it took a little longer than was at first thought, but now Massachusetts knows that while she was the first of the States to take an interest in fish-culture, so she has been the first to demonstrate the certainty of a good return, and she can restock those rivers where the fish have been for a long time killed out."

THE DEVELOPMENT OF NERVES.—Dr. A. M. Marshall is continuing his careful researches into the earliest stages of nerves in vertebrate embryos. He has recently published in the *Quarterly Journal of Microscopical Science* some of his latest results, obtained from embryos of the common fowl, treated with picric acid. He describes a distinct neural ridge at the top of the cerebro-spinal tube in the middle cerebral track, before it has even closed in, the embryo being barely two days old in development. This ridge afterwards becomes continuous along the whole brain and great part of the spinal cord, and many of the nerves undoubtedly arise from it. It appears in the highest degree probable that the olfactory nerve originates from the anterior part of this ridge; but Dr. Marshall is quite certain that there is no special olfactory vesicle in the chick. This is directly contrary to the received teaching, which speaks of an olfactory lobe of the brain, and does not compare the olfactory with other nerves. Dr. Marshall believes that the common olfactory nerve is really the nerve of the anterior cranial segment. The third nerve is for the first time developmentally traced by Dr. Marshall, and he finds it to be a strictly segmental nerve. The seventh and eighth nerves (facial and auditory) are shown to have a common origin; the auditory is really a branch of the facial. The history of the vagus nerve (pneumogastric) is regarded as suggesting very strongly that it is equivalent to several spinal nerves, and not merely to one.

FRENCH POLYZOA.—An important contribution to the history of a number of species of marine polyzoa will be found in a memoir on "Les Bryozoaires des Côtes de France," by M. L. Joliet, of the Zoological Laboratory at Roscoff, a spot so well known to every tourist in Britanny.

A *résumé* is given of the remarkable works that have appeared on the structure of the animals of this group in England, Sweden, and Germany, as well as in France. The interesting question of the part played by, as well as of the origin of, the brown bodies (*groddskaplar* of Nitsche) is very fully gone into. The specialist will know what a subject of debate lies here. Hincks, our best English authority, believing them to be special formations elaborated from the substance of the polyp; Claparède, that they are products of secretion; Nitsche that they are only the remains of decaying polyps. With the last of these views our author agrees: "Le corps brun est un résidu, le reste de la matière qui constituait un polypide après que celui-ci a subi la désorganisation." The nature of the nervous colonial system in the polyzoa is also investigated at full length, and M. Joliet feels compelled to doubt if this so-called system merits this name. The arguments for and against are too technical for us to epitomise. The growth and development of several species were specially investigated, and the entire memoir, to which is appended a list of the species collected (74) at Roscoff during the summers of 1876 and 1877, is well worthy of the attention of all interested in the study of these small but interesting polyps. It will be found in the recently-published *Archives de Zoologie Expérimentale* (Tome 6, No. 2).

STRUCTURE OF LINGULA.—Mr. E. Morse, Professor of Zoology in the Imperial University of Tokio, Japan, has discovered many facts quite new to science in the life history of this interesting form of Brachiopods. Perhaps the most important is his discovery of the auditory capsules. In the species of Lingula investigated, their position and general appearance recall those in certain tubicolous annelids as figured by Claparède. He has also cleared up many points in regard to their circulation, and maintains the absence of anything like a pulsatory organ, the circulation being entirely due to ciliary action.[1] In describing the habits of this species he mentions that, while partially buried in the sand, the free border of the pallial membranes join so as to leave but three large oval openings, one in the centre and one on either side; the bristles then arrange themselves so as to form these openings into funnels which arrest the mucous secretion from the animal, and a continuous current is to be seen passing down the side funnels and escaping by the central one. They can bury themselves very quickly in the sand, and the peduncle agglutinates a sand tube. Prof. Morse exhibited specimens in Boston on December 19, 1877, which had been brought living from Japan; the water had been only changed twice since August 19, and yet none had died. Their viability, therefore, seemed to be great. As Prof. Morse is now on his way back to his professorial duties at Japan, he will have the opportunity of still further prosecuting his researches into the structure and habits of these forms so interesting to both the palæontologist and zoologist.

GEOGRAPHICAL NOTES

NEW GUINEA.—A recent number of *Il Movimento* contains a letter from the Italian traveller, D'Albertis, dated from Thursday Island, in Torres Straits, on January 8 last, in which some account is given of his last expedition into New Guinea. Leaving Port Somerset on May 3, 1877, in his steam launch, *Neva*, it was not until the 21st of that month that he succeeded in entering the *embouchure* of the Fly River, where he was well received by the natives. But such was not the case when the *Neva* had advanced a little further up the river, for on June 1 a sudden and unprovoked attack was made on the vessel, and one of the Chinese crew seriously wounded. These attacks were frequently repeated during the further ascent of the river, though always successfully repelled without casualties. In July and August, when far in the interior, the expedition seems to have been unmolested, but on the subsequent descent of the stream the banks were found again beset by daring and hostile parties of warriors, whose efforts to hinder the return of the expedition brought on frequent skirmishes. Signor D'Albertis was also much inconvenienced by the dissensions of his crew, the greater part of whom deserted him, leaving only five to manage the vessel and to repel the attacks of the natives. Two of these also left him on returning to the mouth of the river, leaving him to accomplish the dangerous navigation of Torres Straits with only the engineer and one sailor. Eventually, however, with aid received from the native teachers on some of the islands in Torres Straits, he succeeded in reaching Thursday Island—now the calling-place of the Queensland mail steamers—on January 4 last. As regards the results of the expedition no details are given in this letter, but from certain expressions employed it would appear that gold in some quantity was obtained. Of this we shall, no doubt, be duly informed before long, as also of the zoological discoveries in which Signor D'Albertis has on former occasions been so successful.

[1] Semper has already fully demonstrated this fact.—F. P. W.

NEW AFRICAN EXPEDITION.—It is rumoured that the Council of the Royal Geographical Society are likely soon to send out a new expedition for the exploration of Africa. The region between Mombasa and Mount Kenia and Victoria Nyanza is mentioned as the probable field of this expedition.

AFRICAN EXPLORATION.—Abbé Debaise, who intends to cross Africa from Zanzibar to the Congo, has received a credit of 100,000 francs from the French government. This sum was voted to him on the proposal of M. Perrin, a radical member, who was supported by M. Gambetta, the leader of the Liberal party. The Abbé will leave Paris for Marseilles in a few days, and thence proceed to Zanzibar. He will be supported by the new Geographical Society of Marseilles, and its president, M. Rambaud, the large Zanzibar trader. News has lately been received in Berlin from the African traveller, Dr. G. A. Fischer, who has traversed since last autumn the tropical regions lying opposite the island of Zanzibar. Despite the hostility of the natives, he has succeeded in making a large number of scientific observations. and has gathered a large collection of zoological specimens, which are now on the way to Berlin. During the present month he starts on a journey up the river Tana.

CAPTAIN ELTON.—We have already referred to the great loss sustained by geography, by the death of this energetic traveller in Ugogo; he died of sunstroke. Mr. Cotterill and Captain Elton had reached this place from the north end of Lake Nyassa, the country traversed being described as very interesting and new to geography. They found the sources of the Ruaha, Usanga, and other affluents of the Lufigi, the Myembe tributary being specially worthy of notice. Mr. Cotterill's narrative will be looked for with interest, as well as Capt. Elton's diaries and map, which have been sent home. The latter, at the time of his death, was H.M. Consul in Portuguese East Africa, and had done work in various parts of the world. He had done good service in helping to clear up the history of African Copal, the produce of *Trachylobium Hornemannium*.

ANCIENT MAPS OF CENTRAL AFRICA.—M. Richard Cortambert, one of the librarians of the National Library in Paris, has discovered in that establishment a gilt globe, dated 1540, and showing apparently that the course of the Congo was known then to have almost the same direction as given to it by Mr. Stanley. There has also been discovered in the public library of Lyons a globe of 1701, on which are traced in detail the geography of the sources of the Nile and Congo. This globe is said to have been executed by the Fathers

Placide, of St. Amour, and Crispinien, of Toulon, and by the two Brothers Bonaventura and Gregory, all of the Order of St. Francis. Father Gregory, it is said, was the celebrated Lyonnaise geographer, Henry Marchand. In speaking of these discoveries at the Paris Geographical Society, M. R. Cortambert showed that there was nothing extraordinary in them. From the fifteenth century most of the maps make the Congo issue from a great mass of water in the interior of the African continent. No doubt all the information in these old maps was furnished by the Portuguese. M. Vivien de St. Martin is also of this opinion. The Portuguese traders were quite *au courant* with the geography of the interior of Africa, and all the maps, even that of Fra Mauro (15th century) represent the Nile issuing from lakes to the south of the equator, and give an idea of the course of the Congo, similar to that made known by Stanley. M. St. Martin reminded the Society, moreover, that Ptolemy himself had indicated three immense lakes in the centre of Africa from which issued the Nile and the Congo; only in his map these lakes are placed much too far south. Father Kircher, in his "Mundus Subterraneus," published at Amsterdam in 1653, gives a map showing four large lakes, from one of which, called Zaire, both the Nile and the Congo are made to issue. Kircher states that he obtained his information from the General Procurator of the Jesuits for these provinces, who lent him a manuscript of Father Païs. This manuscript may possibly be still preserved in the Jesuit College at Rome.

PARIS GEOGRAPHICAL SOCIETY.—Besides the medals to Mr. Stanley and M. St. Martin, the Paris Geographical Society will give medals to Dr. Harmand for his exploration of the Mekong and the coast of Anam, and to M. Ujfalvy for his travels in Turkestan. The *Bulletin* for December contains an important geographical and statistical article on Kashgar, compiled from various sources by M. J. B. Paquier, an itinerary on the Yang-tsze from Shung-shing to Yun-nan-fu, by M. Rocher, and a valuable summary of the geodetic work of the Russian Geographical Society in Asia, by Col. Chanoine.

NOTE ON THE DISCOVERY OF THE LIQUEFACTION OF AIR AND OF THE SO-CALLED PERMANENT GASES

IN the Notes on "Recent Science," in this month's *Nineteenth Century*, the writer, in an account of the results of the researches of M. Pictet and M. Cailletet on the condensation of the so-called permanent gases, draws attention to the long-neglected paper of Mr. Perkins "On the Compressibility of Water, Air, and other Fluids," an abstract of which, and apparently the only one with which the writer is acquainted, appeared in Thomson's *Annals of Philosophy*, *N. S.*, vol. vi., 1823. The paper was intended for the Royal Society, but, being mislaid, was not read at the appointed time. Either it or a second paper was, however, brought before the society on June 15, 1826, and appears in the *Philosophical Transactions* for that year. In this paper, as in the brief record in the *Annals*, Mr. Perkins announces that he had effected the liquefaction of atmospheric air, and other gases, by a pressure of upwards of 1,000 atmospheres, and fully describes the apparatus which he had employed, which is, in principle, very similar to that of M. Cailletet. He thus describes his results in the case of aëriform fluids:—

"In the course of my experiments on the compression of atmospheric air by the same apparatus which had been used for compressing water, I observed a curious fact which induced me to extend the experiment, viz., that of the air beginning to disappear at a pressure of 500 atmospheres, evidently by partial liquefaction, which is indicated by the quicksilver not settling down to a level with its surface. At an increased pressure of 600 atmospheres, the quicksilver was suspended about $\frac{1}{8}$th of the volume up the tube or gasometer; at 800 atmospheres it remained about $\frac{1}{4}$ up the tube; at 1,000 atmospheres, $\frac{3}{8}$ up the tube; and small globules of liquid began to form about the top of it; at 1,200 atmospheres the quicksilver remained $\frac{3}{4}$ up the tube, and a beautiful transparent liquid was seen on the surface of the quicksilver, in quantity about $\frac{1}{2000}$ part of the column of air. On another occasion a second tube was charged with 'carburetted hydrogen' and subjected to pressure; it began to liquefy at about 40 atmospheres, and at 1,200 atmospheres the whole was liquefied."

Mr. Perkins goes on to say: "These instances of apparent condensation of gaseous fluids were first observed in January, 1822, but for want of chemical knowledge requisite to ascertain the exact nature of the liquids produced, I did not pursue the inquiry further; and as the subject has been taken up by those who are eminently qualified for the investigation, I need not regret my inability to make full advantage of the power I had the means of applying."

Mr. Perkins's observations seem to have attracted little attention at the time they were published, and have since been, apparently, almost forgotten. Although they do not in the least detract from the great merit of M. Cailletet's work, they undoubtedly have their place in the history of this subject of the liquefaction of the gases.

It may be worth while to point out that the statement that all the gases known to the chemist have now been liquefied is not strictly true. The most recently-discovered of these—phosphorus pentafluoride—has not yet been seen in the liquid state, although there is not the least reason for believing that it will constitute an exception to the general law.

T. E. THORPE

HELMHOLTZ'S VOWEL THEORY AND THE PHONOGRAPH

THE following experiments with the phonograph are of interest as throwing light on the nature of vowel sounds:—

Let a set of vowel sounds, as A, E, I, O, U (pronounced in Italian fashion) be spoken to the phonograph in any pitch, and with the barrel of the instrument turned at a definite rate. Then let the phonograph be made to speak them, first at the same rate, and then at a much higher or lower speed. The pitch is, of course, altered, but the vowel sounds retain their quality when the barrel of the phonograph is turned at very different rates. We have made this experiment at speeds varying from about three to one, and we can detect no alteration in the quality of the sounds.

According to Helmholtz, the characteristic quality of each vowel is given by the prominence of a constituent note or notes, of definite or approximately definite absolute pitch, in the sounds uttered. Now obviously, the absolute pitches of the constituents of the vowel-sounds in the above experiment were all altered in the same proportion, so that the absolute pitch of the prominent notes varied greatly; but yet the vowel quality was unchanged. This experiment, therefore, appears to give results in contradiction of Helmholtz's theory as we understand it.

At the same time we have found, in the course of experiments, of which a full account will shortly be communicated to the Royal Society of Edinburgh, that if a scale be sung to the phonograph with one vowel sound, such as O, the wave-form of the marks on the tinfoil does not remain unchanged at all pitches. We have not yet had time to analyse the curves so obtained into their harmonic constituents. Such an analysis will show whether the changes we have observed in the wave-form as the pitch rises, are due to a change in the relation of the amplitudes of the constituents present, or only to a variation of phase.

FLEEMING JENKIN
J. A. EWING

Edinburgh, March 11

ELECTRICAL ANALOGIES WITH NATURAL PHENOMENA [1]

II.

POLAR AURORÆ.—The experiments of De la Rive have already shown the connection of polar auroræ with terrestrial magnetism; but they do not explain all the circumstances which accompany auroræ. In M. Planté's experiments the electric current, in presence of aqueous vapour, yields a series of phenomena altogether analogous to the various phases of polar auroræ.

If the positive electrode of the secondary battery is brought into contact with the sides of a vessel of salt water, we observe, according to the distance of the film from the liquid, either a corona formed of luminous particles arranged in a circle around the electrode (Fig. 8), an arc bordered with a fringe of brilliant rays (Fig. 9), or a sinuous line which rapidly folds and refolds on itself (Fig. 10). This undulatory movement, in particular, forms a complete analogy with what has been compared in auroræ to the undulations of a serpent, or to those of drapery agitated by the wind. The rustling noise accompanying the experiments is analogous to that sometimes accompanying auroræ; it is caused by the luminous electric discharge penetrating the moisture. As in auroræ, magnetic perturbations are produced by bringing a needle near the circuit, the deviation increasing with the development of the arch. Auroræ are produced by positive electricity; the negative electrode produces nothing similar.

FIGS. 8, 9, 10.—Coronas and luminous arcs.

Globular Lightning.—To study the effects produced on distilled water, M. Planté increased the tension of the current, combining twenty secondary batteries, composed each of forty couples, and forming a total of 800 secondary couples, whose current of discharge was nearly equal to that of 1,200 Bunsen elements.

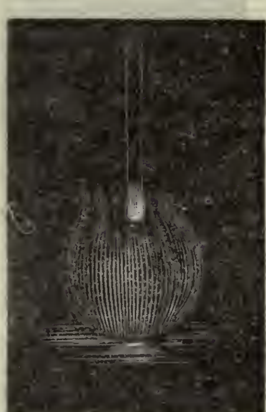


FIG. 11.—Flame produced over distilled water by an electric current of high tension.

When the current of this combination of batteries is made to act on distilled water, he finds, first, in much greater intensity, the effects already observed by Grove, by means of 500 elements of his nitric acid pile. The positive electrode being inserted in the distilled water, he obtains, by approaching the negative platinum wire to the surface of the water, and immediately raising it, a yellow flame, almost spherical, of about two centimetres in diameter (Fig. 11). The platinum wire, two millimetres in diameter, melts; the flame is formed by the rarefied incandescent air, by the vapour of the metal of the electrode, and by the elements of the vapour of water; spectral analysis shows clearly the presence of hydrogen.

If, to avoid the fusion of the metal, we diminish the intensity of the current by interposing a column of water in the circuit, the spark appears under the very compact form of a small globe of fire from eight to ten millimetres in diameter (Fig. 12). On raising the electrode a little more, this globe takes an ovoid form; luminous blue points, whose number varies continually, arranged in concentric circles, appear at the surface of the water (Fig. 13). Rays of the same colour soon issue from the centre and join these points (Fig. 14). At intervals the rays take a gyratory movement, now in one direction, now in another, describing spirals (Figs. 15 and 16). Sometimes the points and the rays disappear all on one side, and varied curves, formed by the movement of those which remain, are figured on the surface of the liquid. Finally, when the speed of the gyratory movement increases, all the rays vanish, and only blue concentric rings are seen (Fig. 17). The rings are found to be the last term of these transformations which are very curious to follow with the naked eye or with a telescope, and constitute a veritable electric kaleidoscope.

The production of these figures is explained by the great mobility of the arcs or luminous threads which compose the ovoid light, formed between the water and the electrode. On examining with care this particular form of spark, he finds that it is, in reality, a sort of voltaic brush discharge, analogous to the brush discharge of static electricity, but more dense on account of the greater quantity of electricity in play. These luminous threads being in a state of continual agitation, the points at which they encounter the surface of the liquid are constantly displaced, and form the rays observed. Their gyratory movement

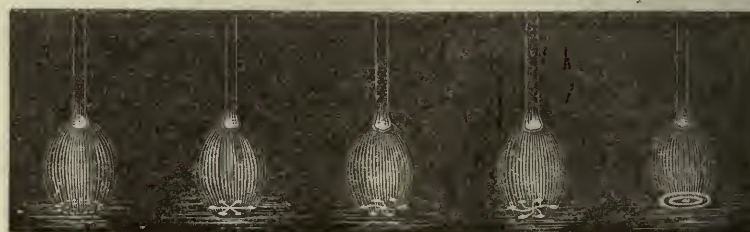
[1] Continued from p. 229.

proceeds from the reaction due to the flowing of the electric flux. As to the rings, they are formed in a visible manner, under the eye of the observer, by the more and more rapid movement of the blue points, and by the persistence of the impression upon the retina.

When the metallic electrode is positive and the distilled water negative, the spark still assumes externally an ovoid form; but the middle is traversed by a cone of violet light. When we employ two metallic electrodes we obtain a luminous spheroid, the interior of which is traversed by a brilliant line. This appearance corresponds to the spark, and the aureole of the spark seen in the discharges of an induction coil; only here the aureole occupies more space, in consequence still of the greater quantity of electricity. In fact, if we much increase the length of the column of water interposed, we do not obtain more than an arc or a straight line.

M. Planté therefore thinks globular lightning may result from an abundant flow of electricity in the dynamic state, in which quantity is joined to tension. The particular

FIG. 12.—Globular spark produced over distilled water by an electric current of high tension.



FIGS. 13 to 17.—Ovoid sparks and luminous figures produced over distilled water by an electric current of high tension.

case where globes of lightning present slow movements or times of stoppage, is explained by the movement or the repose of the column of moist air strongly electrified and invisible, which serves as electrode. To imitate this effect it is sufficient, in one of the preceding experiments, to make the electrode oscillate, it being previously suspended under the form of a long pendulum above a basin full of water or a metallic surface, and to mask by a screen its lower extremity. We then see a little ball of fire move above the water or the conducting surface, and thus reproduce all the appearances of the natural phenomenon.

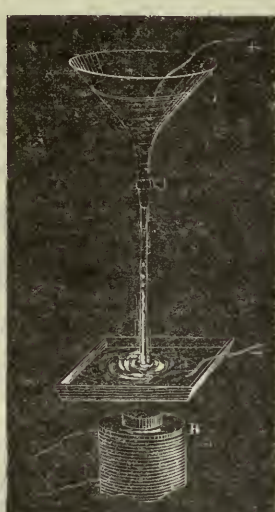


FIG. 18.—Experiment reproducing the effect of waterspouts.

Waterspouts.—Cause a narrow stream of salt-water to flow from a funnel provided with a cock communicating with the positive pole of a battery of 400 secondary couples; the liquid is received in a basin containing the negative wire and below which is an electro-magnet (Fig. 18). As soon as the voltaic circuit is closed the stream appears furrowed with bright lines at its upper part and traversed by a luminous thread at its lower part. Sparks, illuminated aqueous globules, play with a rustling noise at its extremity, vapour is disengaged, and the liquid which surrounds the bottom of the stream takes a gyratory movement in the opposite direction to that of the hands of a watch if the pole of the electro-magnet is north, and in the same direction as the hands if this pole is south. The movement is rendered visible by light bodies spread over the surface of the water. If we contract the stream so as to avoid all solution of continuity at its lower part, the electric and luminous signs disappear almost entirely. The liquid is, nevertheless, heated, as is shown by a light vapour, and the gyratory movement is yet more pronounced and rapid. On extending the stream anew the electric manifestations re-appear as before.

This experiment reproduces the principal effects of waterspouts, the rustling which proceeds from them, the

FIG. 19.—Electric bore.

mist which is formed around them, the flashes of light which furrow them, the globes of fire which sometimes appear at their extremities—in such a way that, according to M. Planté, these meteors may be compared to electrodes of liquid or of vapour, from which escape to the earth or the sea the powerful electric currents of storm-clouds; and if no thunder follows it is because the conductor accompanies them to the ground, and there is in this case no proper electric discharge, no more than under the preceding conditions.

The very formation of waterspouts, or the descent of these cloudy appendages towards the ground, has been connected by Brisson and Peltier with an electrostatic attraction between the clouds and the earth. We may add to this very natural attractive force an action of transport,

of which dynamic electricity presents numerous examples, and which tends to facilitate the flow of water from an electrified cloud. The agitation of the liquid, the boiling of the waters at the point where these meteors encounter the surface of the sea, are explained not only by the descending movement itself, but also by the action of the electric current, which may repel or raise liquid masses like a breeze or an impetuous wind. If we support, in

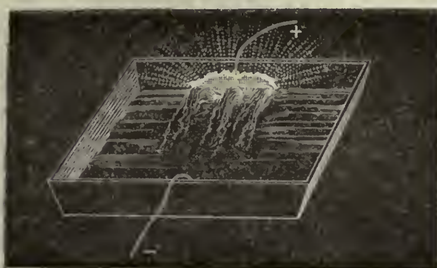


FIG. 20.—Electric bore or formation of liquid waves by the flow of a powerful current of dynamic electricity.

fact, the positive electrode against the sides of the vessel of salt-water communicating with the negative pole, we observe, besides the luminous streaks and jets abounding in vapour, a violent whirling of the liquid forming a sort of electric bore, which raises the water to the height of $1\frac{1}{2}$ centimetre above its level (Fig. 19). When the current meets at certain points inequalities of resistance, it is divided and gives rise to several aqueous hillocks, as seen in Fig. 20.

ON COMPASS ADJUSTMENT IN IRON SHIPS AND ON NAVIGATIONAL SOUNDINGS[1]

IV.—*On a Navigational Sounding Machine.*

THE machine before you is designed for the purpose of obtaining soundings from a ship running at full speed in water of any depth not exceeding 100 or 150 fathoms. The difficulties to be overcome are twofold: first, to get the lead or sinker to the bottom; and, secondly, to get sure evidence as to the depth to which it has gone down. For practical navigation a third difficulty must also be met, and that is to bring the sinker up again; for, although in deep-sea surveys in water of more than 3,000 fathoms' depth it is advisable, even when pianoforte wire is used, to leave the thirty or forty pounds' sinker at the bottom, and bring back only the wire with attached instruments, it would never do in practical navigation to throw away a sinker every time a cast is taken, and the loss of a sinker, whether with or without any portion of the line, ought to be a rare occurrence in many casts. The first and third of these difficulties seem insuperable—at all events they have not hitherto been overcome—with hemp rope for the sounding-line; except for very moderate depths, and for speeds much under the full speed of a modern fast steamer. It may indeed be said to be a practical impossibility to take a sounding in twenty fathoms from a ship running at sixteen knots with the best and best-managed ordinary deep-sea lead. Taking advantage of the great strength and the small and smooth area for resistance to motion through the water, presented by pianoforte wire, I have succeeded in overcoming all these difficulties; and with such a sounding machine as that before you the White Star liner *Britannic* (Messrs. Ismay, Imrie, and Co., Liverpool), now takes soundings regularly, running at sixteen knots over the Banks of Newfoundland and in the English and Irish Channels in depths sometimes as much as 130 fathoms. In this ship, perhaps the fastest ocean-going steamer in existence, the sounding machine was carefully tried for several voyages in the hands of Capt. Thompson, who succeeded perfectly in using it to advantage; and under him it was finally introduced into the service of the White Star Line.

[1] Report of paper read to the Royal United Service Institution, February 4, by Sir Wm. Thomson, LL.D., F.R.S., P.R.S.E., Professor of Natural Philosophy in the University of Glasgow, and Fellow of St. Peter's College, Cambridge. Revised by the Author. [The Council of the R.U.S.I. have kindly permitted us to publish Sir W. Thomson's paper in advance, and have granted us the use of the illustrations.—ED.] Continued from p. 354.

The steel wire which I use weighs nearly $1\frac{1}{2}$ lbs. per 100 fathoms, and bears when fresh, from 230 to 240 lbs. without breaking; its circumference is only ·03 of an inch. By carefully keeping it always, when out of use, under lime water in the galvanised iron tank prepared for the purpose, which you see before you, it is preserved quite free from rust, and, accidents excepted, this sounding line might outlive the iron plates and frames of the ship. If the sinker gets jammed in a cleft of rock at the bottom, or against the side of a boulder, the wire is inevitably lost. Such an accident must obviously be very rare indeed, and there does not seem to be any other kind of accident which is altogether inevitable by care in the use of the instrument. The main care in respect to avoidance of breakage of the wire may be stated in three words—beware of kinks. A certain amount of what I may call internal molecular wear and tear will probably occur through the wire bending round the iron guard rod which you see in the after part of the instrument, when, in hauling in, the wire does not lead fair aft in the plane of the wheel, as is often the case even with very careful steering of the ship, but from all we know of the elastic properties of metals, it seems that thousands of casts might be taken with the same wire before it would be sensibly weakened by internal molecular friction. Practice has altogether confirmed these theoretical anticipations so far as one year of experience can go. My sounding machine has been in regular use in charge of Captains Munro and Hedderwick in the Anchor liners *Anchoria* and *Devonia* (Messrs. Henderson Brothers, Glasgow) for eleven months and seven months respectively, and in neither ship has a fathom o wire been lost hitherto, though soundings have been taken at all hours of day and night, at full speed, in depths sometimes as great as 120 fathoms. No break not explicable by a kink in the wire has hitherto taken place in any ship provided with the sounding machine. That it will bear much rough usage is well illustrated by one incident which happened in a cast taken from the *Devonia* running at thirteen knots. The sinker in falling from the wheel into the water accidentally fell between the rudder chain and the ship, and fifty fathoms or so had gone out before it was noticed that the wire was running down vertically from the wheel instead of nearly horizontally as it ought to have been by that time. The handles were immediately applied to the sounding wheel, and it was turned round to haul in without reducing the speed of the ship. Though the wire was bent almost at right angles round the chain until it was nearly all in, it was all got safely on board, as was also the cod-line with attached depth gauge, and the sinker at the end of it.

When soundings are being taken every hour or more frequently (as in the case of a ship feeling her way up channel from the 100 fathom line when the position is not known with sufficient certainty by sights and chronometers) the sounding wheel should be kept on its bearings in position; with the cod-line, depth gauge, and sinker, all bent on and ready for use. But in all other cases the wheel should be kept in its tank under lime water, and the cod-line with sinker and depth gauge attached should be kept at hand in a convenient place near the stand of the machine, which should be always fixed in position ready for use. With such arrangements, and methodical practice, as part of regular naval drill in the use of the sounding machine, one minute of time should suffice to take the sounding wheel out of its tank, place it on its bearings, adjust the brake cord, bend on the cod-line, and be quite ready for a cast. When the machine is to be shown to an inspecting officer the wheel ought to be in its tank of lime water when he asks to see a cast. It should be carefully noticed that the ring at the end of the wire is securely lashed by small cord to the hole provided for it in the ring of the wheel whenever the cod-line is unbent from the ring. If the wire and ring are allowed at any time to knock about slack on the wheel when the wheel is being moved to be set up for use or to be replaced under the lime-water there is a liability to some part of the wire getting a turn which may be pulled into a kink. One accident, at least, has happened in this way: the sinker dropped off carrying the cod-line and ring with it just as it was being let down from the taffrail for a cast. If the sinker had weighed 400 lbs.[1] it could not have broken the double wire next the ring without a kink.

A description of the machine and rules for its use are given in the accompanying printed paper of instructions, to which I have only now to add a few words regarding the depth gauge. Erichsen's self-registering sounding lead (patented in 1836), depending on the compression of air, might be used with my machine, but the simpler form before you is preferable as being

[1] It weighs 22 lbs.

surer. It too depends on the compression of air, but in it the extent to which the air has been compressed is marked directly on the interior of a straight glass tube by the chemical action of sea-water on a preparation of chromate of silver with which the tube is lined internally. Between the salt of the sea-water and the chromate of silver a double decomposition takes place. The chlorine leaves the sodium of the common salt and combines with the silver, while the chromic acid and oxygen leave the silver and combine with the sodium. Thus chloride of silver, white and insoluble, remains on the glass in place of the orange-coloured chromate of silver lining as far up as the water has been forced into the tube, and the chromate of sodium dissolved in the water is expelled as the air expands when the tube is brought to the surface.

My navigational sounding machine was brought into practical use for the first time in the steamship *Palm*, belonging to Messrs. Charles Horsfall and Co., Liverpool, in a voyage to Odessa and back about a year ago, in command of Capt. E. Leighton. I cannot illustrate the use of the machine better than by reading to you an extract from a letter I received last April from Capt. Leighton, describing his experience of it in this first trial :—

"During the voyage in the *Palm* steamship, which has just come to an end, I took frequent opportunities of testing the sounding machine when I had a chance of cross-bearings to verify the depths as shown by chart, and always found it most accurate. For instance, going up through the Archipelago and just after clearing the Zea Channel, I got a good position by bearings, chart showing seventy-nine and seventy-six fathoms, two casts of your glass gave seventy-eight and seventy-five fathoms. In the Bosphorus also it gave capital results in thirty to forty fathoms water.

"The first real use I made of the machine was in the Black Sea during a fog which obscured everything. Wishing to make sure of my position I put the ship's head for the land and kept the machine at work. After running in to thirty fathoms at full speed I slowed down and went in to twelve fathoms, then hauled out to a convenient depth and put her on the course up the coast. When it became clear I found myself in a proper position, and no time had been lost by stopping to sound.

"How many shipmasters let hours go by without obtaining soundings either because of the delay or on account of the danger of rounding-to in heavy weather to get them, when, if they were provided with your sounding-machine, they could have their minds set at ease by having timely warning of danger, or by knowing that they were in a good position!"

I had myself very satisfactory experience of the usefulness of the sounding machine in coming up Channel running before a gale of south-west wind in thick weather, on the 6th and 7th of last August, on returning from Madeira in my yacht—a small sailing schooner of 126 tons. About 5 A.M. on the 6th I took two casts and found ninety-eight fathoms (sand and red spots) and 101 fathoms (sand and small shells). The mean with a correction of 2½ fathoms to reduce to low water, according to the state of the tide at Ushant at the time, was ninety-seven fathoms. Thenceforward I took a sounding every hour but one till eight in the evening. By writing these soundings on the edge of a piece of paper at distances equal according to the scale of the chart to the distances run in the intervals, with the edge of the paper always parallel to the course, according to the method of Sir James Anderson and Capt. Moriarty, I had fixed accurately the line along which the vessel had sailed, and the point of it which had been reached, with only a verification by a noon latitude. At 6 o'clock next morning, by the soundings and course, with proper allowance for the flood-tide, I must have been about thirteen miles magnetic south of the Start, but nothing of the land was to be seen through the haze and rain; and with the assistance of about ten more casts of the lead (by which I was saved from passing south of St. Catherine's) I made the Needles Lighthouse right ahead, at a distance of about three miles, at 2 P.M., having had just a glimpse of the high cliffs east of Portland, but no other sight of land since leaving Madeira and Porto Santo. In the course of the 280 miles from the point where I struck the 100 fathom line to the Needles, I took about thirty casts in depths of 100 fathoms to nineteen fathoms without once rounding-to or reducing speed; during some of the casts the speed was ten knots, and the average rate of the last 220 miles was a little over nine knots.

It is a pleasure to me to be able to add that the sounding machine has also been successfully used in the Royal Navy, Admiral Beauchamp Seymour and Capt. Lord Walter Kerr having kindly taken it on board H.M.S. *Minotaur* for trial last summer. Lord Walter Kerr wrote, on his return from Vigo, regarding it as follows :—

"The sounding machine is most serviceable. We have been using it constantly when running up Channel, from the time of crossing the line of soundings to the time of reaching Plymouth, and though running before a gale of wind with a heavy sea, at the rate of ten knots, we were able to get soundings as if the ship had been at anchor. We were able to signal to the squadron each sounding as it was obtained; thus, in thick weather, verifying our position by soundings without having to round the ships to."

THE ANALOGIES OF PLANT AND ANIMAL LIFE[1]

LET us begin our inquiry into the analogies of plant and animal life by comparing the egg of an animal with the seed of a plant. Let it be the ripe seed of a common plant, and the egg of a bird. Both seed and egg may be said to consist of the young creature and a supply of food which is stored up for its use, and is gradually exhausted as the young creature develops. Every one who has tried when a boy to blow a late bird's egg must have been painfully alive to the fact of its containing a young animal, and the egg we eat for breakfast may serve to remind us of the store of food which we diverted from its proper course of nourishing a young chicken.

Here is a diagram representing a section through the seed of a poppy, in which the young plant may be seen lying in its store of food containing a supply of carbohydrates and nitrogenous matter, which is consumed as the yolk of the egg is consumed by the young chicken. Other seeds, such as a bean, an acorn, or an almond, seem at first sight to consist of nothing but the young plant, and to have no store of food. The two halves into which a pea split are the two first leaves or cotyledons of the young plant, the embryo stem and root being represented by the little projecting mass lying between the two halves at one end of the seed. Here the store of food is laid up in the body of the young plant just as many young animals carry with them a store of food in the shape of the masses of fat with which they are cushioned; the two leaves which seem so gigantic compared with the rest of the plant are filled with nutriment, and perform the same function of supplying food for the growth of the seedling, which is performed by the mass of nutrient material in which the embryo of the poppy seed is embedded. Recent researches have shown that embryo plants are possessed of powers which even in the present day it seems almost ludicrous to ascribe to them. I mean powers of digestion. Gorup-Besanez,[2] a distinguished German chemist, found that in the germinating seed of a vetch a ferment exists similar to the ferment in the pancreatic secretion of animals—a secretion having the power of reducing both nitrogenous bodies and starch to a condition in which they can be utilised and absorbed by the tissues, so that the embryo plant behaves exactly as if it were a minute animal digesting and absorbing the store of food with which it is supplied. The power of digesting starch possessed by the embryo plant has been brilliantly demonstrated by van Tieghem,[3] who found that the embryo removed from the seed of the Marvel of Peru (*Mirabilis jalapa*) was distinctly nourished if placed in an artificial seed made of starch paste. He found that the starch paste was actually corroded by the young plant, proving that a digestive ferment had been at work.

This wonderful experiment is of special interest as proving that the digestive ferment is a product of the young plant itself, just as the digestive juice of an animal is a secretion from its stomach. It is indeed a striking thought that whether we grind up a grain of wheat to flour and eat it ourselves as bread, or whether we let the seed germinate, in which case the young plant eats it, the process is identically the same.

The power of storing up food in a fixed condition and utilising it when required is a most important function both in animal and plant physiology. And just as this utilisation is seen in the seed to be brought about by a ferment—by a digestive process—so probably wherever the transference or utilisation of food stores occurs it is effected by ferments. If this be so it would seem that the processes of

[1] A Lecture delivered at the London Institution on March 11 by Francis Darwin, M.B.

[2] *Deutsch. chem. Gesellsch.*, 1874; *Botanische Zeitung*, 1875, p. 565.

[3] *An. Sc. Nat.*, 1873, xvii. p. 205.

digestion proper, as they occur in the stomach and intestines of animals and on the leaves of carnivorous plants, I say it is probable that these processes are only a specialisation[1] of a widely spread power, which may exist in the simplest protoplasmic ancestor of animals and plants. In this case we shall have no right to consider the existence of carnivorous plants anything strange or bizarre; we should not consider it, as seems sometimes to be done, an eccentric and unaccountable assumption of animal properties by plants; but rather the appearance of a function which we have quite as much right to expect in plants as in animals. Not that this view makes the fact of vegetable digestion any less wonderful, but rather more interesting as probably binding together by community of descent a wide class of physiological functions. Let us now pass on to consider the analogies of plants and animals in a more advanced stage of life.

Great differences exist among animals as to the degree of development attained before the young ones enter the world. A young kangaroo is born in a comparatively early stage of development, and is merely capable of passive existence in its mother's pouch, while a young calf or lamb soon leads an active existence. Or compare a human child which passes through so prolonged a condition of helplessness, with a young chicken which runs about and picks up grain directly it is out of its shell. As analogous cases among plants, we may take the mangrove and the tobacco plant. The ripe seed of the mangrove is not scattered abroad, but remains attached to the capsule still hanging on the mother plant. In this state the seeds germinate and the roots grow out and down to the sea-level, and the plant is not deserted by its mother until it has got well established in the mud. It is due to the young mangrove to say that the conditions of life against which it has to make a start are very hard on it. The most intrepid seedling might well cling to its parent on finding that it was expected to germinate on soft mud daily flooded by the tide. Perhaps the same excuse may be offered for the helplessness of babies—the more complicated the conditions of life, the greater dependence must there be of offspring on parent.

Now compare a young tobacco plant with the mangrove. All the help the seedling tobacco receives from its parent is a very small supply of food; this it uses up in forming its first pair of leaves; it has then nothing left by way of reserve, but must depend on its own exertions for subsistence. By its own exertions I mean its power of manufacturing starch (which is the great article of food required by plants) from the carbonic acid in the air. In this respect it is like a caterpillar which is formed from the contents of the egg, but has to get its own living as soon as it is born.

In many cases there is a certain degree of independence in young creatures, which are nevertheless largely dependent on their parents' help. Thus, young chickens, though able to feed themselves, depend on their mother for warmth and guidance. A somewhat parallel case may be found among plants. It has been shown that the large store of reserve material in a bean is not all needed for the development of the seedling. It has been proved that well-formed and flourishing seedlings are produced, even when a large part of the cotyledons has been removed. In fact, the store of food in the bean has been said to play a double part in the economy of the plant,[2] first, as giving absolutely necessary formative material, and secondly, as protecting the young plant in the struggle with other plants, by supplying it with food till it is well established and able to make its own food. This view was fully established by my father,[3] who sowed various kinds of seed among grass in order to observe the struggle; he found that peas and beans were able to make a vigorous start in growth, while many other young plants were killed off as soon as they germinated.

The young bean is thus indirectly protected by its mother from death, which the severe competition entails on less fortunate seedlings. This kind of protection can only in a certain general sense be compared with the protection given by parent to offspring. Nevertheless, a more strictly parallel case may be found among animals. Certain fishes retain the yolk bag, still containing a supply of food, and swim about leading an independent life, carrying this store with them. Among plants, a good case of a retention of a store of food occurs[4] in the oak. Young trees possessing woody stems and several leaves may still have an acorn underground with an unexhausted store of food.

In comparing the lives of plants and animals, one is struck with the different relation which the welfare of the race bears to the welfare of the individual. In plants it is far more obvious that the aim and object of existence is the perpetuation of the species. The striking and varied development of the reproductive organs in plants is one factor in this difference. Roughly speaking, plants strike us most by their flowers and seed—that is by organs serving the interest of the race. Animals are most striking on account of their movements, which are chiefly connected with the wants of the individual. If a child wants to know whether a stick *is* a stick or a caterpillar, he touches it, and if it walks off, classes it in the animal kingdom. Of course, I do not mean that the power of movement is a mark of distinction between animals and plants, but it certainly is a power which is well developed in most animals, and badly developed in most plants. It is the absence of locomotive powers (as opposed to the absence of simple movements) that especially characterises most plants. One sees the meaning of this if one inquires into mode of life of stationary and of locomotive animals. Stationary animals either inhabit the water, or else are parasitic in habits, and live on tissues of plants or animals. In either case the absence of locomotion has the same meaning. Many aquatic animals derive their food from the minute organic particles floating in the water, so that even though they lead a stationary life, food will be brought to them by the currents in the water. Parasitic animals obtain their food directly from the juices or sap of their host, so that they do not need to move about as other animals do in search of food. In the same way plants live like parasites on the earth, penetrating it with their roots, and sucking out its juices; and their food—carbonic acid—is brought to them by the currents of the air, so that like both an aquatic and a parasitic animal, they have no need of locomotion as far as concerns the obtaining of food.

In the case of many young animals their powers of locomotion would be useless unless the eggs were deposited by the mother in a proper place; one cannot imagine anything more forlorn than a caterpillar reared from an egg laid anywhere by chance, and expected to find its proper plant. The necessity of finding proper places to lay her eggs implies the necessity of locomotion on the part of the mother. This need of locomotion is, of course, equally a need to the plant, but it is supplied in a distributed way. The seeds themselves become locomotive; they either acquire plumes to fly on the wind like the seeds of dandelions or they become burrs and cling to passing animals, or are distributed in other ways. Various and strange are the means of transportal adopted by seeds; for instance, the acorn seems to distribute itself by deliberately trading on the carelessness of creatures which are usually considered its superiors in intelligence. Good evidence exists that young oaks which grow scattered in large number over a wide extent of wild heathy land have sprung from the acorns accidentally dropped by passing rooks. In all these cases the young plant has to trust to chance as to what kind of soil it will be deposited in, and this of course accounts for the enormous number of seeds produced by plants. Some seeds are more fortunate in possessing a kind of mechanical choice or power of selecting suitable places to grow in. Many years ago my father described a plumed seed which, when damped, poured out a sticky substance capable of gluing the seed firmly to whatever touched it. Imagine such a plant blown by the wind over some sandy waste; nothing tends to stay its course till it happens to pass by a region where the soil is damper; then it sends out its sticky anchors, and thus comes to rest just where it has a chance of germinating favourably. Again, some seeds have a certain amount of locomotive power independent of such external agencies as wind or passing animals. I mean a power of burying themselves in the ground; the seeds of grasses are the best known of these self-burying seeds; and among them the feather-grass, or *Stipa pennata*, is the most conspicuous. These seeds possess a strong, sharp point, armed with a plume or tuft of recurved hairs, which act like the barbs of an arrow and prevent the seed from coming out again when it has once penetrated the soil. This arrow-like point is fixed at the lower end of a strong awn, which has the remarkable property of twisting when dried and untwisting when wetted. Thus the mere alternations of damp nights and dry days cause the arrow-like point to rotate, and by another contrivance, which it would take too long to go into, the point is pressed against the surface of the ground and actually bores its way into it. Fritz Müller

[1] See Morren, "La Digestion Végétale," Gand, 1876; and Pfeffer, "Landwirth. Jahrb.," 1877.

[2] Haberlandt, "Schutzeinrichtungen in der Entwickelungen der Keimpflanzen," 1877, p. 29. The idea is quoted as originally given by Sachs, *Vienna Acad.*, xxxvii., 1859.

[3] See "Origin of Species," 6th edition, p. 60.

[4] Haberlandt, p. 12.

described in a letter to me how these twisting grass seeds bury themselves in the extremely hard and dry soil of Brazil, and are thus no doubt enabled to germinate. Unfortunately these boring grass-seeds do not always confine themselves to penetrating the soil, but exercise their powers on both men and animals. I have received accounts from India and from Italy of the way in which the sharp-pointed seeds work their way through thick trousers into the legs of unfortunate sportsmen. But the most extraordinary case is that of certain grasses which work their way into sheep. They often penetrate the skin deeply and in large numbers, inflicting great tortures and often causing death by emaciation. Mr. Hinde, of Toronto, has given me the details of this plague to sheep-farmers as it occurs in Buenos Ayres. Another observer has described it in Australia.[1] He states that not unfrequently the seeds are found actually piercing the heart, liver, and kidneys of sheep which have died from the effects. I believe that the northern part of Queensland has been actually given up as a sheep country because of the presence of this grass.

Another use to which locomotion is applied by animals is that of finding a mate at the proper season. The curious imitation of the courtship of animals which is found in Vallismeria is well known. The stalk grows with extreme rapidity up through the water till the female flower reaches the surface, and there awaits the approach of the male flower, which breaks loose and floats down the stream to meet her. But most plants have not even this amount of locomotive power, and are therefore compelled to employ either the wind or insects as go-betweens. Fortunately for the beauty and sweetness of our woods and fields, insect fertilisation is the commonest means adopted; and all the bright flowers and sweet smells of flowers are nothing but allurements held out to insects to entice them to carry the fertilising pollen from one flower to another. It is curious to find a plant adopting a new mode of conveying its pollen when the old one fails. Thus, a wild cabbage-like plant which grows in Kerguelen's Land is now fertilised by the wind, that is, it produces dry dust-like pollen, which is easily carried by the wind. Now this cabbage is the only species in the enormous order of the Cruciferæ which is not fertilised by insects; so that we may be certain that a change has taken place for which some sufficient reason must exist. And the reason of the change is no doubt that the insects in Kerguelen's Land are wingless, and are therefore bad distributors of pollen. And to go one step further back, the reason why the insects are wingless is to be found in the prevalent high winds. Those insects which attempt to fly get blown out to sea, and only those are preserved which are gradually giving up the habit of flying. Thus the pollen of the cabbage has to learn to fly, because the insects will not fly for it.

In considering the analogies between plants and animals, one cannot merely compare those functions which are strictly and physiologically similar in the two kingdoms. One rather sets to work and studies the needs which arise in either a plant or an animal, and then discovers in what way the same need is supplied in the other kingdom. There is no connection between a plant having bright flowers and an animal's power of walking about, yet they may, as we have seen, play the same part in the economy of the two lives.

In the life of animals the first needs that arise are supplied by certain instinctive movements. The young chicken only escapes from its egg by some such movements. Mr. W. Marshall has also shown that the chrysalides of certain moths possess instinctive movements by which they escape from the cocoon or outer case. In one case a sharp spike is developed, sticking out from the side of the chrysalis, and as the latter rotates the spike saws the cocoon all round, so that the top lifts off like a lid. Again, in young chickens Spalding has shown the existence of an instinctive power of obtaining food, and instinctive recognition of the hen by sound only. This was proved by a newly-hatched chicken, which had never heard or seen its mother, running towards a cask under which a clucking hen was hidden. The powers of growth which exist in young seedlings would certainly be called instinctive if they existed in animals, and they are quite as indispensable as those just mentioned in supplying the wants which first arise.

These two instincts are the power of directing the growth in relation to the force of gravity, and in relation to light; the first being called geotropism, the second heliotropism. As soon as the young root emerges from the seed-coats, it turns abruptly downwards, perceiving like the chick in what direction the earth, its mother, lies. Thus the young plant fixes itself firmly in the ground as quickly as possible, and is enabled to begin to make arrangements for its water supply. At the same time the young stem grows upwards, and thus raises itself as much as possible over its neighbours. The power of directing itself vertically upwards is also a necessity to the plant, because without it no massive growth like that of a tree would be possible. It would be like a child trying to build a wooden house with bricks that did not stand straight. Thus, both the young stem and the young root have an instinctive knowledge as to where the centre of the earth is—one growing towards the point, the other directly away from it. This fact is so familiar to us, that we fail to think of it as wonderful; it seems a matter of course, like a stone falling or a cork floating on water. Yet we cannot even generalise the fact so far as to say it is the nature of all stems to grow up, and all roots to grow down, for some stems, such as the runner of a strawberry, have a strong wish to grow down instead of up, and side roots that spring from the main ones, though their method of growth is identical with that of the main roots, have no wish to grow downwards. We can find no structural reason at all why a root should grow down and a stem up. But we can see that if a plant took to burying its leaves and rearing its roots into the air, it would have a bad chance in the struggle for life. It is, in fact, the needs of existence which have impressed these modes of growth on the different organs of the plant in accordance with their various requirements. On the other hand, the plant is not absolutely tied down by geotropism, it is not bound to grow *always* in a vertical line, but is ready to be turned from its course if some other direction can be shown to be more advantageous. Thus Sachs[1] planted peas in a little sieve, and as the roots emerged underneath, they were enticed from the vertical by an oblique damp surface. This power of giving up the line of growth for the sake of a more advantageous position, must be of great service to roots, by enabling them to choose out damp places in the earth which a blind adherence to rule would have caused them to pass by.

The other tendency, which may be also compared to an instinct, is the power possessed by the growing parts of plants of perceiving the position of the chief source of light. This tendency of course interferes with the geotropic tendency, for if the tip of a growing shoot bends towards the light it deviates from its vertical course. This contest between two instincts is well shown by placing a pot of seedlings close to a lamp or a window, in which case the heliotropic beats the geotropic tendency and the young plants curve strongly to the light; now if the pot is removed to a dark room the geotropic tendency reasserts itself, and the seedlings become once more upright. One might fancy from this that the darkness of night would be always undoing any good gained by heliotropic growth in the day. An imaginary case in the life of a seedling will show that it is not so. A seedling germinates under a pile of sticks: having few competitors it makes a good start, but in consequence of the darkness it begins to starve as soon as it has exhausted the supply of food given it by its mother plant stored up in the seed from which it sprang. It starves because it is dark under the pile of sticks, and without light it cannot decompose the carbonic acid of the air and make starch; carbonic acid may be said to be the raw material from which a plant makes its food—but without the help of light the plant is powerless to make food—it starves in the midst of plenty. So that the power of knowing where the light is and of moving towards it may be just as necessary to prevent a young plant starving as the power of knowing a grain of corn when it sees one and of snapping it up are to a young chicken. Luckily for our imaginary plant a ray of light streams in between two sticks—if the plant insisted on growing straight up in obedience to the geotropic instinct it would lose its chance of life. Fortunately the other light-seeking instinct wins the day and the plant thrusts its summit between the sticks and reaches the light. And now it is clear that when the plant has once got between the sticks the tendency to straighten again in the night will not be able to undo the advantage gained in the day by heliotropism. Besides the tendency to seek the light, there is in some plants another exactly opposite tendency to grow away from it. Just as in the case of geotropism no reason can be given why two organs should be affected in exactly an opposite manner by the same cause; no difference of structure can be perceived and no difference in manner of growth can be found between a tendril which grows away from the light and one which grows towards it. The convenience of the plant seems to dictate the result. Thus the virginian creeper climbs by forming little sticky feet at

[1] C. Prentice, *Journal of Botany*, 1872, p. 22.

[1] "Text Book of Botany," Eng. Tr. p. 764.

the end of its tendrils, and as it climbs up a support each new tendril is enabled by its power of seeking for darkness rather than light to find out little dark crannies in which to place its feet. On the other hand a bryony climbs by seizing anything it can get hold of, and as each tendril reaches out towards the light the whole plant will tend to be dragged towards the lighter side of the bush or hedge on which it clambers.

It looks as if the case might be put thus: Given the fact that light produces some kind of movement, the convenience of the plant shall decide whether it be towards the light or away from it; or in other words, grant the plant the power of knowing where the centre of the earth is, and grant it the power of knowing where the light comes from, then the plant itself can decide what course of growth is most advantageous.[1]

(*To be continued.*)

[1] I have spoken as if the existence of positive and negative helio- and geotropism could be simply explained by considering the convenience of the plant. But in details many difficulties arise; for instance, some roots are heliotropic. (Sachs' "Text Book," p. 755.)

NOTES

The subscription for M. Leverrier's statue is progressing favourably. A sum of 4,200 francs has been already collected. The subscribers up to the present moment number thirty-five, almost all of them belonging to the French Institute. M. Cohen, of Antwerp, sent 1,000 francs. Other large sums are expected soon from different parts.

It is stated that M. Faye has declined to stand for the direction of the Paris Observatory, unless it is agreed to retain at the observatory the International Meteorological Office. It is very probable that the long spoken of Meteorological Institute will now be established; at all events a solution of the pending question will soon be adopted by the government.

M. Dumas announced to the Paris Academy of Sciences at its sitting on March 4 that an anonymous donor offers a prize of 6,000 francs to be awarded in 1880 to the person who makes the most useful application of M. Pasteur's researches to the healing art.

A committee has been formed at Königsberg to erect a fitting monument upon the grave of the great philosopher Immanuel Kant. The City authorities have headed the list of subscriptions with the sum of 4,000 marks (200*l.*).

In a report by M. Daubrée to the Paris Academy of Sciences it is strongly recommended that measures should be taken to preserve the many boulders which are scattered over France, and many of which are disappearing under the pick-axe of the builder. The Academy has appointed a commission for the purpose, which will have delegates in the principal districts of the country. Similar measures have been taken in Switzerland since 1866, and our Scotch geologists deserve praise for their zeal on behalf of the preservation of the boulders of their country, and for their excellent periodical reports on the subject.

Gen. Duff, in a letter to the Earl of Derby, dated Gothenburg, January 4, reports that great shoals of herrings of the large kind which disappeared from this coast in the year 1809 have now made their appearance again north of Gothenburg. The first appearance of the herring took place at Christmas, when whales were seen following the shoals toward the coast. Preparations were made by the merchants of Gothenburg to make good use of this godsend. It would appear from the history and traditions of Sweden that, after an interval of seventy years, there are some grounds for supposing that the shoals of herrings may be expected to visit the coast regularly for fifty or sixty years to come, as has been the case during earlier periods. The Swedish Government have appointed Professors Sars and Smitt to inquire into the various questions raised by this sudden appearance of the herring shoals off this part of the Swedish coast, the more important of these questions being the alleged disappearance of the shoals from the coast of Norway, whither, it is said, they have betaken themselves since 1808, and the bearing of the inquiry on the future of the fishery.

The first National Entomological Exhibition commenced on Thursday at the Royal Aquarium, Westminster, and is thoroughly creditable to all concerned. There are altogether about 250 exhibitors contributing between eight and nine hundred cases, with an average of at least 300 insects per case; and the whole of the specimens shown, with very few exceptions, have been collected by ladies and gentlemen and artisans, in their leisure hours.

We regret to learn the death of Joachim Monteiro, at Delagoa Bay. He was an active and enterprising naturalist, whose work on Angola will give him an enduring place in the literature of African travel, no less than his services in procuring and sending to this country a great part of the fine series of specimens from which *Welwitschia* was originally described.

The death is announced of Sulhiz Kurz, the Curator of the Herbarium of the Calcutta Botanic Garden. Possessed of an extensive knowledge of Indian botany, he had recently completed the preparation of a Flora of British Burma for the Indian Forest Department. He died at Penang on his way to the islands of the Malayan Archipelago, for the purpose of botanical exploration.

A letter has been written by the Municipal Council of Paris to the director of the Meteorological Service of the Observatory asking that the publication of weather telegrams and prognostications be made in Paris as well as in provincial towns.

A great prehistoric burial ground has recently been discovered at Cremmen (in the district of East-Havelland, Prussia), not far from Berlin. Numerous urns and ash-jars of varied form, all containing ashes and bones of burnt human remains, have been found. The urns are mostly round in shape, and stood some $2\frac{1}{2}$ to 3 feet below the surface upon a large slab of stone; they were surrounded by round stones, and each was covered with a flat stone lid. The antiquities will all be deposited in the Provincial Museum of Berlin.

An International Agricultural Exhibition will take place at Hamburg on June 13, and will last 5 days; and another exhibition of this nature will be held at Prague on May 15, 16, and 17.

The Royal Society for Agriculture and Botany of Ghent, will hold its Horticultural International Exhibition on March 31 next. These exhibitions are quinquennial, and last for seven days. The coming one promises to be unusually brilliant, to judge from the copious list of names of exhibitors.

We are glad to see that a beginning has been made in the formation of a local museum at Tenby, the proposal for which we referred to some time since. The magnificent geological collection of the late Mr. Smith, of Gumfreston, has been purchased. The Corporation of Tenby has given the old National School-rooms on the Castle Hill, and after some slight alterations have been made they will be admirably well adapted for a museum. In addition to the geological specimens there will be a valuable collection of British shells, and one of Pembrokeshire birds and eggs; also a library of scientific books. It would be idle to speak of the advantage this institution is likely to confer on the town, and on all the residents in South-west Wales, where nothing worthy of the name of a museum at present exists. About 300*l.* is still required before the museum can be opened. The trustees ought to have no difficulty in raising this moderate sum in the district concerned; perhaps some of our readers might like to contribute. The hon. secretary is Mr. Edward Laws, Tenby.

THE *Spectator* learns from a private letter that the telephone has been adopted by the Chinese, the telegraph being useless, as they have no alphabet. Five hundred miles, it is stated, have already been spoken over in China. Mr. H. F. Stevens writes from Tabreez, Persia, that conversation and music were transmitted satisfactorily by telephone between that town and Tiflis, along the line of the Indo-European Telegraph Company.

AT a recent meeting of the East Kent Natural History Society, P of. Gulliver exhibited a very extensive series of drawings of raphides and other crystals found in the tissues of plants. Mr. Gulliver considers that sufficient attention has not hitherto been directed to the part played by these deposits of mineral salts in the vital economy of the plant, or, from the soluble condition in which they are presented, in the nutrition of the animals which feed on them.

Apropos of the note in NATURE (vol. xvii. p. 311) relating to a recent attempt to send certain fish to America, Mr. Carrington, of the Westminster Aquarium, writes that there are in the Royal Aquarium, Westminster, at least eighteen species of fish from American waters. In return he has exported a number of sea-water animals, including fish, molluscs, crustaceans, and zoophytes. At first a large proportion of specimens was lost during the voyage, but now there are seldom any lost, either on the homeward or outward voyages. In addition to the constant attention necessary at sea, Mr. Carrington finds the great secret of success is to have the animals subjected to confinement for some weeks before shipment.

MESSRS. LONGMAN have published an abridged edition of Dr. Pole's excellent "Life of Sir W. Fairbairn." The personal narrative has been retained entire, the scientific and technical portions being much abridged. The little work deserves, and no doubt will have, a wide circulation.

WE are pleased to see that the *Natural History Journal*, conducted by the Societies in Friends' Schools, whose appearance we noted a year ago, has reached the beginning of a second volume. Of the 170 contributors sixty-three, we learn, have been boys and girls. The journal is well conducted and, judging from the number before us, its contents are well calculated to interest its young readers in science.

IN NATURE, vol. xii. p. 514, W. W. Wood, writing from Manila, describes a species of Navicula (?) with a gelatinous ciliated envelope, which is there figured. Mr. Wood announced his intention of submitting his specimens to a competent diatomist, but three years have nearly elapsed, and no more has been said on the subject. It is one of such extreme importance as bearing on the ordinary motion of diatoms, that Mr. G. S. Boulger writes asking for an explanation.

MANY of our readers will be sorry to hear of the death, on Monday last, of the old hippopotamus, in the Gardens of the Zoological Society. He was obtained in the White Nile when only a few days old, in 1849, and has been in the Gardens since 1850. Prof. Garrod, F.R.S., will communicate the results of his examination of the body to an early meeting of the Zoological Society.

WITH reference to a note in NATURE (vol. xvii. p. 38) respecting the uninflammability of eucalyptus, Mr. A. Nicols writes that this must be a mistake, as in Australia the wood is extensively used as fuel. Acclimatisation of these really valuable trees, Mr. Nicols thinks, should be strongly supported. They yield timber of immense size and strength, durable alike in dry or wet situations, and more proof against the attacks of *termites* than many other woods, and some work up into beautiful furniture. They would probably thrive wherever the mean annual temperature is not below 60°, or, roughly speaking, over an area of about one-half of the habitable region of the earth.

THE provisional observatory at Meudon is in full operation, as we reported some months ago. Dr. Janssen has done such good work that the ministry will propose to restore the old palace which is now in ruins, and establish a splendid physical observatory in an admirable situation. A credit of 250,000 francs is to be asked for; this will include the purchase of a refractor om. 67 in. diameter.

THE governor of the French island, Reunion, in the Indian Ocean, reports that this colony was visited by a terrible cyclone on January 15, causing great losses of life and property.

UNDER the auspices of the Deutscher Fischerei-Verein, 2,000,000 salmon eggs are hatched annually, and distributed among the various rivers of the empire. Strong efforts are now being made to introduce extensively the grayling, which is comparatively rare in Germany.

IN the February session of the Berliner polytechnische Gesellschaft, Dr. F. Siemens, of Dresden, the inventor of the new compressed hard glass, gave an interesting exhibition of this new invention. The process has been brought to such perfection, that the hard glass is not only more easily, but more cheaply manufactured than the ordinary glass. The power of resistance varies from eight to ten times that of ordinary glass. The serious objection made to hard glass at the time of its discovery, that it often fell to pieces when entirely unexposed to pressure, has been successfully avoided. This property was found to result from over-hardening, and it is now possible to detect all articles which have acquired it, by the use of the polarisator, under which over-hardened glass shows a prevalence of violet tints. This condition is also detected by exposure to water at a certain temperature.

THE working out of the results obtained by the Transit of Venus expeditions sent out by the German Government, were expected to have been far enough advanced for publication in the year 1877. It has been found, however, that this task causes more difficulties and expenses than had been at first anticipated. The Imperial Chancellor's office has therefore demanded from the German Parliament an extra credit of 500*l.* to defray the additional costs.

A USEFUL invention has recently been made by Herr Weber, of Hummel-Radeck, near Lübben (Prussia). This gentleman has contrived to construct a very simple machine for levelling roads, which for working requires only two horses, a driver, and a labourer, and renders it possible to make such improvements in a road, in a short time, as could otherwise be accomplished only by fifty or sixty workmen. The machine works equally well upon gravel or clay soil, and its cost is only forty-five marks (shillings). The whole machine works much in the same way as an ordinary carpenter's plane does upon wood.

ON January 25, shortly after noon, the belfry of Toucy (Yonne) was struck by lightning, and set on fire. According to Dr. Roche (who describes the case), the wind was blowing from the north-west, and a dense, low cloud had begun to cover the ground with large hailstones. A few minutes after a single and prolonged thunder peal was heard, and the cross on the belfry was then seen to be surrounded by a luminous meteor. Persons in the houses near the church saw coming from the base of the belfry two fire-balls of about 0·30m. to 0·40m. diameter, and about 0·50m. apart. They rolled on the steps of the building about 20m. and disappeared. A woman in a room about 15m. from the belfry was carried to the end of the room; a young man who was passing was thrown on the ground, and several other persons were more or less shaken. Immediately after the thunder peal the hail ceased and was replaced by a snow-storm which lasted three-quarters of an hour. It was afterwards found that the belfry was fired at two points—one at the upper part of the north-west side, the other at the lower part of the south-east side, probably the points of entrance and exit of the electric fluid. Toucy stands in the middle of a narrow

valley, running north-west, and is rarely visited by thunderstorms, which pass nearly always to the right or left.

SOME researches on the magnetic properties of nickel were recently made by the well-known physicist M. H. Wild, and are now published in the *Bulletin* of the Imperial Academy of Sciences of St. Petersburg. M. Wild arrives at the following conclusions: (1) Pure nickel can become permanently magnetic to a considerable degree, thus differing materially from pure (soft) iron. The maximum qnantity of permanent magnetism which pure nickel can retain is, however, only between one half and one-third of that quantity which hard steel can permanently retain. (2) Magnetism is less permanent in nickel than in well-hardened steel, after the magnetising force has ceased to act; the slow loss of maguetism in course of time, as well as by heating and cooling, is comparatively greater in nickel than in steel; and this is the case even if the nickel has, like hard steel, by repeated heating and cooling, been brought to a certain state of permanent capacity. (3) The temperature coefficient of nickel magnets in the latter state is a little larger than that of properly hardened steel. (4) The temporary magnetism which pure nickel can retain is about double its permanent quantity, or about one half of the temporary magnetism which hard steel, and about one quarter of that which soft iron can retain. In its magnetic properties nickel is, therefore, thoroughly inferior to iron and steel.

THE question with regard to the existence of microscopic organisms in media containing no oxygen has been a fruitful subject of discussion for biologists of late, and some doubts have been thrown on the entire absence of this gas in the experiments cited by Pasteur and others. Prof. von Nägeli, in his work on "Die niederen Pilze," which has just appeared in Munich, presents some interesting figures in this connection. According to his calculations the larger bacteria weigh $\frac{1}{2500000000}$ milligramme. If we assume that they consume the same amount proportionally of oxygen daily as a man, viz., 1 per cent. of his weight, then a million bacteria would require in twenty-four hours $\frac{1}{20000}$ milligramme, or nearly $\frac{1}{30000}$ cubic centimetre of oxygen. These calculations, taken in connection with the well-known difficulties of entirely eliminating gases, will probably render a repetition of the best experiments necessary.

FROM recent experiments on the spread of gases through bodies, Dr. Wroblewsky (*Pogg. Ann.*) arrives at the following conclusion:—When a gas is absorbed it spreads in the absorbent body according to the same laws as those ruling the propagation of heat in a solid bar; and that whether the absorbent body be liquid or solid, or in a transition state between these two extremes." The only exceptions to this law are attributable to the action of gravity. It is known that the excretion of carbonic acid by an animal is increased by a violent muscular action, but it has been uncertain whether the CO_2 is a direct product of muscular action, *i.e.*, belongs to the substances which, through decomposition processes, are formed in greater measure during contraction of the muscles. To clear up this point, M. Sedgwick-Minot recently forced through the vascular system of detached muscles of dogs (the blood having been removed) a quantity of blood-serum saturated with oxygen, and determined the proportion of CO_2 in the serum in a series of cases in which the muscles were at rest, and in another series in which they were repeatedly stimulated to contraction. If the contraction of the muscle caused a greater formation of CO_2, the serum, after passage, must contain more CO_2 than if the muscle remained at rest. The experiments, however, gave equal quantities of CO_2 in the two cases, and the reason of the fact referred to at the outset is not determined.

THE additions to the Zoological Society's Gardens during the past week include two Macaque Monkeys (*Macacus cynomolgus*) from India, presented respectively by Capt. Pole Carew, and Mr. Henry Wright; a Green Monkey (*Cercopithecus callitrichus*), two Common Chameleons (*Chamæleon vulgaris*) from West Africa, presented by Mr. G. H. Garrett; a Herring Gull (*Larus argentatus*), European, presented by Mr. Capstick; two Undulated Grass Parrakeets (*Melopsittacus undulatus*) from Australia, presented by Mr. Hylton Jolliffe; an American Darter (*Plotus anhinga*) from South America, purchased; two Sambur Deer (*Cervus aristotelis*), an Isabelline Bear (*Ursus isabellinus*) from India, a Javan Adjutant (*Leptoptilus javanicus*) from Java, received in exchange.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—Mr. J. R. Terry, M.A., Fellow of Magdalen College, who was Fifth Wrangler at Cambridge in 1873, has accepted the senior mathematical mastership in Magdalen College School; and Mr. D. C. Robb, B.A., scholar of Worcester College, has been appointed to a science mastership (in physics) at the same time.

CAMBRIDGE.—The report of the Council of the Senate recommending the appointment of an assistant to Prof. Hughes has been confirmed upon the understanding that the person to be appointed be not permitted to take private pupils.

EDINBURGH.—A movement has originated in the University of Edinburgh to procure a portrait of Prof. Balfour, in recognition of his services to the University in having for thirty years acted as Dean of the Medical Faculty. This movement has been joined in by the Fellows of the Royal Society of Edinburgh, in recognition on their part of the services he has for many years rendered to the Society in the character of Secretary.

BIRKBECK INSTITUTION.—A course of six lectures on Electric Telephony will be delivered by Mr. W. J. Wilson, F.C.S., on Saturday evenings, at eight o'clock, commencing March 23. The entire proceeds will be given to the fund now being raised for the erection of a new building for the institution. The lectures will be very fully illustrated by experiments, diagrams, &c., and will form a complete exposition of the subject.

PARIS.—M. Pierre Picard is proposed as a successor to the late Claude Bernard in the chair of physiology at the Collége de France. He was for a long time assistant to the famous physiologist, and is himself the author of valuable researches on the constitution of the blood corpuscles. At present he is a professor in the Faculty of Medicine at Lyons.

ALGERIA.—M. Bardoux proposes to establish in Algeria three preparatory schools for medical and law students, one in each of the three provinces. At present Algiers alone is provided with a preparatory school of medicine. The means for obtaining superior instruction, which have been very limited up to the present in the colony, will be greatly enlarged.

BERLIN.—On the night of the 8th instant the professors and students of the Berlin University, assisted by civil and military dignitaries, held a grand "Commers," or beer-drinking revelry, in the time-honoured style of German academical life, in honour of the sixtieth anniversary of the birthday of Prof. A. Hofmann, the celebrated chemist. The proceedings began by the secretary of the committee reading a letter from the Chamberlain of the Crown Prince expressing the regret of his Imperial Highness at being prevented from attending the festivity. After this Prof. Helmholtz, the Rector of the Berlin University, formally congratulated Dr. Hofmann, who replied in a speech to the felicitations addressed to him by his Berlin colleagues and friends. Numerous other speakers, among them Privy-Councillor Jacob, Chief of the Patent Office, and Prof. Reuleaux, Rector of the Berlin Polytechnic Academy, then addressed the hero of the day. The official part of the festivity closed at 2 o'clock, after which came the singing of all the obligatory songs and the delivery of student speeches. Not a few congratulatory letters and telegrams reached Prof. Hofmann on the auspicious day from England, America, and France.

PRUSSIA.—The three agricultural institutes of Prussia are attended at present by 270 students, of whom 215 are from Prussia, 20 from other parts of Germany, and 35 from foreign countries.

GERMAN POLYTECHNIC CONGRESS.—At the recent inauguration of the new Polytechnic Institution of Brunswick, the assembled men of science considered the question of a general congress of lecturers at the German polytechnic schools. It is intended to hold the congress at Dresden, and a preliminary meeting of delegates will take place in the beginning of April, in order to fix the programme for the congress. Dresden has also been selected as the meeting-place for a congress of German engineers and architects, and it is supposed that the two meetings will be held simultaneously.

SAXONY.—An interesting example of the comparative sums devoted in Germany to various educational purposes is to be seen in the recently-issued Report of the Minister of Public Instruction for Saxony, a kingdom with 2,550,000 inhabitants. The whole number of educational establishments is 3,900, of scholars and students, 523,000, of instructors, 6,400. The salaries amount to 12,300,000 marks, and the total educational expenses are 18,000,000, of which 5,000,000 are contributed by the Government. The State devotes 766,000 marks to its 76 gymnasia and *Realschulen*, 1,354,000 to the general school system, and nearly as much, viz., 1,048,000 marks to the University of Leipzig with its 161 professors and 3,100 students, besides 893,000 marks for pensions. The total annual cost of the Leipzig University is 1,402,000 marks, or 70,100*l*.

SCIENTIFIC SERIALS

Annalen der Physik und Chemie, No. 1, 1878.—The universal compensator, by M. Beetz.—On the electromotive force and the internal resistance of some thermopiles, by M. Beetz.—The theory of stationary currents regarded from a quite general standpoint, by M. v. Bezold.—On a tangent multiplier and the electromotive force of the Grove element, by M. Riecke.—On the influence of density of a body on the amount of light absorbed by it, by M. Glan.—On the theory of the longitudinal-elliptical vibrations in the incompressible ether, by M. Ketteler.—On fluorescence, by M. Lommel.—On metallic reflection, by M. Wernicke.—On the volume-increase of liquids through absorption of gases, by Messrs. MacKenzie and Nichols.—Some observations on Crookes's radiometer, by M. Riecke.—Determination of the resonance-tones of the mouth-cavity by percussion, by M. Auerbach.—On the pitch of a tuning-fork in an incompressible liquid, by M. Auerbach.

Zeitschrift für wissenschaftliche Zoologie, vol. xxx., part I.—Rhizopod studies, by Emil Buck, 49 pp. 2 plates; dealing with the development of arcella, and a new genus parasitic on rotifers.—Revision of the genus analges (avian parasite), by G. Haller.—Contribution to the anatomy of asteridæ, by Hubert Ludwig, 4 plates, 63 pp., describing the water-vascular system, the blood system, the nervous and the generative apparatus, the body cavity.—Contribution to the natural history of the cestodes, by H. A. Pagenstecher, dealing with *Tænia critica* and *Cœnurus serialis*.

SOCIETIES AND ACADEMIES

LONDON

Linnean Society, February 21.—W. Carruthers, F.R.S., vice-president, in the chair.—Mr. Thos. Christy illustrated by diagram and made remarks on M. Ossenkep's new system of plant-propagation; and he also showed the recently imported fresh berries of the Liberian coffee of this year's crop.—Mr. Holmes exhibited a remarkable oak gall of *Aphilothrix sieboldii*, Hart., obtained at Willesboro, Leas, Ashford.—Mr. Thiselton Dyer likewise exhibited and made a few observations on the inflorescence and a drawing of the palm *Pthychosperma rupicola*, Thw., which had flowered for the first time in Europe at Kew.—A paper, notes on the Mahwa tree (*Bassia latifolia*), was read by Mr. E. Lockwood. This tree grows in abundance in India; a hundred thousand may be seen in the plains around Monghyr. Wild animals of all kinds greedily devour the flowers, of which one tree will yield several hundredweights. Besides being highly nutritious to man, it is an excellent fattening agent for cattle, &c. A strong-smelling spirit is obtained by distillation of the corolla, an essential oil from the fruit, and as an agent in soap-making the tree is invaluable. Thus, certain yield, unlimited supply, nourishing and chemical qualities, easy preservation, and its cheapness, all combine to render it a commercial product of no mean importance to our Indian empire.—The gist of a "Synopsis of the Hypoxidaceæ," by Mr. J. G. Baker, was given. This group differs in some respects from the Amaryllidaceæ, and offers a close alliance with the Bellosiceæ. Four genera, and between sixty and seventy species are known. The Cape is their head-quarters, but some are found in Tropical Africa and Angola, a very few in Abyssinia and the Mascarenes. None are found in Europe, Polynesia, North and Central Asia, nor Extra Tropical South America.—The Secretary read an abstract of a technical paper on the Schoepfieæ and Cervantesiiæ, distinct tribes of the Styraceæ, by John Miers, F.R.S.—Then followed a communication by Mr. Arthur G. Butler, on the butterflies in the collection of the British Museum, hitherto referred to the genus Euplœa of Fabricius.—Dr. Hance, of China, Mr. E. Milner, Dr. Geo. Shearer, and the Rev. R. Boog Watson were elected Fellows of the Society.

Chemical Society, February 21.—Dr. Gladstone, president, in the chair.—A lecture entitled "Laboratory Experiences on board the *Challenger*" was delivered by Mr. J. Y. Buchanan. After describing his laboratory, which measured 10 feet by 5 feet 8 inches and 6 feet high, and its fittings, the lecturer gave a detailed account of the means by which, after estimating the compressibilities of water and mercury, he was enabled to determine the depths and temperatures attained by the sounding line. The compressibility of distilled water was found to be 0·000049 per atmosphere, or 0·0009 per 100 fathoms; of sea-water, 0·00077 per 100 fathoms; and of mercury, 0·0000271 per 100 fathoms, or 0·0000015 per atmosphere. He then described the apparatus and methods by means of which the amounts of oxygen, nitrogen, and carbonic acid were determined. The most interesting results obtained were the following:—From the surface down to 300 fathoms the amount of oxygen continuously decreases; from 300 fathoms downwards, whatever be the depth, the amount increases. This anomalous result the lecturer stated to be due to the great abundance of animal life at the depth of 300 fathoms, the increase in the quantity of oxygen at greater depths being caused by its non-consumption, owing to the scarcity of life. The next part of the lecture dealt with the distribution of the sea-water as regards density, in depth and superficially. Two regions of maximum density exist north and south of the equator, corresponding to the tracts frequented by the trade winds. At 350 fathoms deep a great zone of water of low density is found. The densest water is found in the Atlantic. Light water is found in the neighbourhood of ice and in certain regions immediately after the cessation of the monsoons. The maxima of density lie in the north hemisphere to the south-west, in the south to the north-west of the maxima of barometric pressure. A hearty and unanimous vote of thanks was given to Mr. Buchanan for his interesting lecture, which was illustrated by many tables and diagrams.

Physical Society, February 16.—Prof. W. G. Adams, president, in the chair.—The following candidate was elected a Member of the Society: Mr. G. H. West, M.A.—Dr. Lodge read, for Mr. H. F. Morley, M.A., a paper on Grove's gas battery. After referring to the views of M. Gaugain and Mr. Grove himself with regard to the cause of the action of this apparatus, the author proceeded to describe an elaborate series of experiments he has recently made in order to ascertain the circumstances by which it is regulated. It would be impossible to give a clear account of them in a short space, but some of his conclusions are as follows:—The whole of the current is due to dissolved gas, and if n be the distance of the level of the liquid from the top of the plate in the H tube, and $E = \frac{C\ R}{1{,}000}$, C being given in galvanometric readings and R in ohms, he finds that, approximately, $(1 + n\,a)\ C = b + n\,e - (c + n\,d)\ E$, where a, b, c, d, and e are constants. The electromotive force is not constant, but rises with the resistance. The current is greater in proportion as the gas present in the elements is less; and, finally, the current appears to vary directly with the pressure.—Mr. S. C. Tisley then described the harmonograph, specially referring to its use for drawing pairs of curves for the stereoscope. This, the latest forms of his pendulum apparatus, is capable of giving a very great variety of curves, for, in addition to rectangular vibrations, parallel and elliptic motions can be combined by its means. In the older form of apparatus each pendulum moves on the other as a centre, whereas in the instrument described they are independent. One pendulum carries at its upper end a table which can be caused to rotate by clockwork if required. The whole is supported on a kind of gimbal joint formed of two pairs of knife edges at right angles, so arranged that vibration

can take place either on one or the other, or the two can be so combined as to give a circular motion; or again, the pendulum can be caused to vibrate in any given plane. The second pendulum vibrates in the plane in which the two hang, and carries at its upper end an arm terminating in a pencil over the table of the other pendulum. A very ingenious adjustment renders it possible to raise or lower the bob of the second-named pendulum during its motion. If two pens be attached, about $2\frac{1}{2}$ inches apart, instead of the single one usually employed, and two curves be traced, they are not precisely similar, and when viewed in a stereoscope they are found to give the well-known appearance of solidity to the figure. It was further shown that by gradually changing the relative motions of the pendulums it is possible to impart to the curve many of the forms observed in biaxial crystals in the polariscope.—Mr. F. J. M. Page then exhibited the action of the telephone on a capillary electrometer. The construction of Lippman's electrometer as modified by Marey was first explained, and the meniscus of the mercury in the capillary tube was thrown on the screen by the electric light. The delicacy of the instrument was shown by passing a current of $\frac{1}{1000}$th of a Daniell, which caused a distinct movement of the mercury. Resistance of 5,000 ohms and $\frac{1}{10}$th ohm gave approximately the same deflection; so that, in practice, the instrument may be considered to be independent of resistance, in addition to which it possesses the great advantage of portability, and its indications are almost instantaneous. To illustrate the use of the electrometer for physiological investigations, a frog's heart was connected by non-polarisable electrodes with the instrument; each beat of the heart caused a considerable movement of the mercury column. A telephone was now connected; on pressing in the iron plate the mercury moved, and on reversing the wires the movement was seen to be in the opposite direction. On singing to the telephone each note produced a movement, but the fundamental note of the plate as well as its octaves and fifths had the greatest effect. On speaking the mercury oscillated continually; some letters of the alphabet had scarcely any effect, and the *w* was especially curious, producing a double movement. Reversing the wires did not alter the character or *direction* of these movements. The same effect was observed when the telephone was in the primary and the electrometer in the secondary coil of a Du Bois Reymond's induction coil. In conclusion, Mr. Page showed the contractions produced in a frog's leg; on inserting under the sciatic nerve two platinum wires coupled with the binding screws of a telephone and talking to this instrument, violent contractions ensued. In the course of the discussion which followed, Prof. Graham Bell expressed himself as highly gratified at the results of Mr. Page's experiments. He has made very many attempts to ascertain the strength of the current produced by the human voice in vain, but considers the present method will in all probability give some most valuable results. He was quite unable to account for the fact that the motion of the mercury took place *from* the opening, but this seems to depend on conditions not yet determined.—Mr. Wilson then exhibited, for Prof. S. P. Thompson, a lantern slide galvanometer for showing the deflections of the needle to an audience. It consists of a coil of insulated copper wire wound on a flat bobbin, within which a needle is balanced on a horizontal axis; this needle carries a long needle of aluminium traversing a semicircular divided photographic scale, and as this is transparent the index can be projected on to the screen. The whole is inclosed between two glass plates.

Geological Society, February 20.—Henry Clifton Sorby, F.R.S., president, in the chair.—James W. Carrall, Tientsin, China, Edward Cleminshaw, Percy John Neate, Arthur Nicols, John Snell, and John Spencer were elected Fellows of the Society.—The following communications were read:—Notes on the physical geology of the Upper Punjàb, by A. B. Wynne, F.G.S. The author stated that crystalline rocks are rare in the accession parts of the Upper Punjàb district, and that when present they consist of syenite and gneiss. The Cambrian and Silurian formations are represented by more or less metamorphosed azoic slates in the Himalayan district, and in the Salt Range by a zone less than 200 feet thick, containing either *Obolus* or *Siphonotreta*, underlain by a thick unfossiliferous sandstone, beneath which is a deposit of gypseous marl and salt. Above the Silurian in the Salt Range, and conformable to it, comes the magnesian sandstone group and a group of unfossiliferous sandstones and clays; in the Himalaya these deposits are probably represented by an unfossiliferous siliceous dolomite, which rests unconformably upon the slates. There are no fossils indicative of rocks of Devonian age. The carboniferous rocks, are also conformably deposited on limestones, sandstones, and shales, the last sometimes carbonaceous. These deposits contain hæmatite in sockets, and the oldest known ammonites have been found in them. An infra-triassic group occurring in Lei Bau mountain consists of red shales, sandstones, and red quartzitic dolomites, overlain by lighter-coloured siliceous dolomites, which in their turn are covered by hæmatite, quartz breccia, sandstones, and shales. The author believes these to have been deposited by the same waters which subsequently laid down the trias, which is largely composed of limestones in the northern Himalayan area, and here and elsewhere includes dolomites, shales, and sandstanes. Numerous fossils occur in some of the beds, such as *Dicerocardium*, *Megalodon*, and *Nerinæa*. In the western part of the Salt Range conglomerates composed of great blocks are regarded by the author as evidence of proximity of land. The Jurassic deposits are local in their distribution, and consist of shales, sandstones, and limestones, containing abundant fossils, such as belemnites, ammonites, and saurians. A dark limestone contains also *Gryphea* and *Trigoniæ*. The cretaceous deposits, when present, are conformable to the carboniferous; they are variable in thickness and fossil contents, and are not recognisable near Attock between the Jurassic and nummulitic groups. Further east a group, supposed to be cretaceous, includes clays with boulders of crystalline rock, which the author regards as derived from land to the south. One of these boulders presented glacial striæ. The eocene rocks are generally limestones, and lie conformably upon the subjacent formations. The nummulitic series of the Salt Range includes gypseous and coaly shales. The salt beds sometimes attain a thickness of over 1,000 feet. The Miocene and Pliocene deposits are of immense thickness, and contain only fossils of terrestrial and fresh-water origin, so that the deposits were formed in lakes and inland seas. The tertiary epoch closed with the elevation of the Himalayas and Salt Range, which was followed by a long period of change, during which various deposits were produced, some including great quantities of erratics, which, however, the author believes were brought to their present position rather by floating ice than by the extension of glaciers.—Description and correlation of the Bournemouth beds; Part I., Upper or Marine Series, by J. Starkie Gardner, F.G.S. The author comes to the conclusion that the whole group is contemporaneous with the Bracklesham beds, and is not of Lower Bagshot age. Similar shore conditions probably extended into the London basin, and the beds mapped by the Survey as Lower Bagshot are probably of the same age as those at Boscombe, in which case nothing more than the Bracklesham is to be met with in the London basin. The similarity of the leaves, &c., from Bovey Tracey to those obtained by the author leads him to infer that the former also are of eocene, and not of miocene age. The author increases the thickness of the London clay at Alum Bay at the expense of the Bagshot beds, and diminishes that of the Bracklesham beds at Whitecliff Bay by transferring part of them to the Lower Bagshot.—Notes on certain modes of occurrence of gold in Australia, by Richard Daintree, F.G.S.—Notes on the geology of the Island of Mauritius and the adjacent islets, by W. H. T. Power, B.A. (Communicated by W. Whitaker, F.G.S.)

Entomological Society, February 6.—H. W. Bates, F.L.S., F.Z.S., president, in the chair.—Prof. J. O. Westwood, Mr. J. W. Douglas, and Mr. F. Smith, were nominated by the president as vice-presidents for the year.—Mr. Rich. S. Standon and Mr. T. W. Wonfor, were elected Members of the Society.—Mr. J. Jenner Weir exhibited the following spiders:—three species identified by Sir Sydney Saunders as *Atypus sulzeri*, taken at Lewes; a remarkable form from Madagascar, and a small species beaten out of trees in the New Forest, which in marking and coloration, resembled lichen.—Mr. McLachlan exhibited a small collection of dragon-flies in illustration of a paper he communicated entitled "Calopterygina collected by Mr. Buckley in Ecuador." The collection contained a fine series of a new species, *Euthore mirabilis*.—Mr. Meldola exhibited a remarkable specimen of *Leucania conigera*. The colour and markings of the fore-wings were reproduced in the lower half of the left hind-wing.—Mr. Meldola read extracts from a letter addressed to Mr. Chas. Darwin from Dr. Fritz Müller, St. Catharina, Brazil, containing some valuable observations on the discrimination exhibited by a number of butterflies for certain colours in flowers. Mr. Müller also described the odoriferous organ of a male sphinx-

moth which exhaled a strong musk-like odour, and called attention to a secondary sexual character observable in some species of Callidryas and other Pierinæ, in the serration of the costal margin of the anterior wing. This is confined to the males, though sometimes found in the females of Callidryas Philea, but in a far less degree.—Reference was made to a sphinx-moth, the proboscis of which, measuring 22 centimetres, had been forwarded by Mr. Müller and was exhibited at the meeting.—Mr. A. G. Butler stated that he had measured the probosces of all the Sphingidæ from Madagascar contained in the British Museum, and found that none of them exceeded 5 inches in length. He also stated that the Callidryades in the British Museum with serrated costal margins to the fore-wings, included the males of all the species of the genera Catopsilia, Phœbis, and Callydryas (true), with the addition of one or two other species. The President observed that in the genus Prioneris the serrated costal margin existed in both sexes.—The Secretary, on behalf of Capt. Elwes, exhibited some coloured illustrations of butterflies which had been taken by a new process of nature-printing.—Mr. G. C. Champion exhibited a specimen of the rare British beetle *Anthicus bimaculatus*, taken at New Brighton, and some specimens of the genus Cetonia, from the Mediterranean region.—Mr. J. W. May exhibited a specimen of *Carabus intricatus*, which he described as taken, for the first time, in the neighbourhood of London.—Mr. H. Goss called attention to the occurrence of sexual dimorphism in *Erebia medea*, exhibiting specimens of both forms of the female.—Sir John Lubbock read a paper on the colouring of British caterpillars. Accepting the principle laid down by Mr. Darwin and others, that dull-coloured, green, and smooth-skinned caterpillars are eaten by birds, &c., whilst spiny, hairy, and brightly-coloured species are rejected, the author stated that by the statistical method it was shown that no hairy caterpillars are green, whilst, on the other hand, a large majority of black and brightly-coloured species are hairy or otherwise protected.—Mr. Meldola read extracts from a recent communication by Dr. Fritz Müller in *Kosmos* on the subject.—The following papers were communicated by Mr. C. O. Waterhouse:—"Description of a new Dragon-fly (Gynacantha) from Borneo," "Description of a new Species of Chernetidæ (Pseudoscorpionidæ) from Spain," "On the Different Forms occurring in the Coleopterous Family *Lycidæ*, with Descriptions of New Genera and Species."

PARIS

Academy of Sciences, March 4.—M. Fizeau in the chair.—The following papers were read:—On the theory of the telephone, by M. Du Moncel. The theory of speech being transmitted by electro-magnetic action causing the plate of the receiving telephone to repeat the vibrations of the sending one, is, he thinks, untenable. The plate in the receiving instrument merely strengthens by reaction the magnetic vibrations of the bar, which seem to be due to contractions and dilatations of the magnetic molecules, through being successively magnetised and demagnetised. Induced currents probably owe their advantage for this work to their *instantaneity*. Their greater or less intensity is of small account.—The vibrations of matter and the waves of the ether in photo-chemical combinations, by M. Favé.—Report of Committee on the importance of preservation of certain erratic blocks situated on French territory, and on the work of MM. Falsan and Chautre, on ancient glaciers and the erratic region of the middle part of the Rhône valley, by M. Daubrée.—On the telluric etiology of cholera, by M. Decaisne. Cholera appears on all geological formations, but its development and propagation depend largely on the physical aggregation of the ground, its permeability for water and air, and the variable quantity of water it contains. The partisans of the telluric doctrine always suppose a specific infectious substance or cholera germ, which is propagated from place to place by human communications, not by the atmosphere.—Study of the resistance of the air in the torsion-balance, by MM. Cornu and Baille. Eliminating accidental perturbations, they have established these two laws: (1) The amplitudes or distances of two successive elongations decrease in geometrical progression; (2) The epochs of the elongations are in arithmetical progression. One theoretical consequence is that the resistance of the surrounding air to the movement of the lever is proportional to the first power of the angular velocity of the lever.—Influence of electricity on evaporation, by M. Mascart. Small basins containing water or moistened earth were placed under conductors (having the form of circular gratings), which were electrified by a Holtz machine driven by a water-motor, and were kept in a constant electric state. The evaporation was thus constantly increased, sometimes even doubled. Inequalities of temperature, however, veil the influence of electricity; the basins were inclosed in a large case, the air in which was regularly dried, and in winter the operation was performed in a kind of subsoil.—Observations on gallium, by MM. Lecoq de Boisbaudran and Jungfleisch. *Inter alia*, the authors exhibited anhydrous chloride, bromide, and iodide of the metal.—Discovery of a small planet at Clinton, New York, by Mr. Peters.—Theory of Vesta, by M. Perrotin.—On the employment of particular solutions of a differential equation of the first order and the first degree, in the investigation of the general integral, by M. Darboux.—On the fundamental points of the group of plane curves defined by a differential equation of the first algebraic order, by M. Fouret.—On the sumnatory formula of Maclaurin, by M. Callandreau.—On the elastic forces of vapours emitted by a mixture of two liquids, by M. Duclaux. A mode is indicated of calculating beforehand the boiling temperature of a liquid of known constitution.—Theory of the new direct-vision spectroscope, by M. Thollon.—On the combustion of gases, by M. Schützenberger. This relates to the propagation of combustion in eudiometers. The chief conditions affecting the phenomenon are: pressure of the gas, length of the gaseous column, composition of the mixture, and diameter of the tube.—On two allotropic varieties of magnetic oxide of iron, by M. Moissan. Sesquioxide of iron heated in an atmosphere of hydrogen or carbonic oxide to 350° or 440°, is transformed in a few hours into magnetic oxide; but this is very different in properties from the magnetic oxide got at a high temperature, by decomposing water with iron at a red heat or burning iron in oxygen, or decomposing sesquioxide at a lively red.—On the action of fluoride of boron on anethol; study of fluorhydrate of fluoride of boron, by M. Landolph.—New carbonated cupric liquor for determination of glucose, by M. Pellet.—On lactic fermentation, by M. Boutroux. He describes the form of the organism present and its mode of action.—Researches on the chemical composition and the functions of the leaves of plants, by M. Corenwinder. The predominance of azotised substances in young leaves indicates that it is these substances which exercise the respiratory function (absorbing oxygen and exhaling carbonic acid). Phosphorus too is in much less quantity in the older leaves, which again are rich in calcareous salts, and the chlorophyll in them retains and decomposes the CO_2 emanating from respiration.—Researches on the maturation of olives, by M. Roussile.—On the mineral water of Challes, in Savoy, by M. Willm.—On the frequency of glaucoma on the north coast of Africa, by M. Gayal.

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THURSDAY, MARCH 21, 1878

EASTERN EXCAVATIONS

Mycenæ. A Narrative of Researches and Discoveries at Mycenæ and Tiryns. By Dr. Henry Schliemann. (London: Murray, 1878.)

Troy and its Remains. A Narrative of Researches and Discoveries made on the Site of Ilium and the Trojan Plain. By Dr. Henry Schliemann. (London: Murray, 1875.)

Exhibition of Antiquities from Hissarlik at the South Kensington Museum. By Dr. Henry Schliemann.

Cyprus: its Ancient Cities, Tombs, and Temples. A Narrative of Researches and Excavations. By General Louis Palma di Cesnola. (London: Murray, 1877.)

TWO Eastern questions occupy the attention of Europe at the present time—one relating to the present, and, it is to be feared greatly, to the future; the other has reference to the past, and to the bridging over of that little-known protohistoric period which connects the civilisation of the far east, that is, Egypt and Assyria, with the culture of ancient Greece, to which we western Europeans are so much indebted. Different conditions of thought are engaged in the study of these two questions, yet both are connected, for the present crisis in the East represents the returning current of that same stream of culture which was flowing westward towards the dawn of our era. What Egypt and Assyria lent to Greece she passed on to Etruria and Rome, and the Romans carried to the shores of the Atlantic, there developing and fructifying, it has passed back eastward in a return wave, reviving the ancient monarchies in its path. Rome has regained its ancient landmarks. Germany has consolidated. Austria has been pushed, and is still pushing eastward. Greece is proclaiming the revival of its ancient nationality, and this will doubtless be followed in times to come by the resuscitation of Egypt and Palestine. The Turk, representing the last wave of the western flow, has been met and swamped by the returning ebb.

The time has been well chosen by our archæologists for an examination into the sites of those ancient cities whose history corresponds most closely to the period on which we are now entering; and to us English the parallel between the two eras has special interest. At a time when our fleets are massing in these seas in order to keep open our communication with the East, we are reminded that it was by means of a seafaring people that civilisation was spread over this region in ancient times. The comparison between ourselves and the Phœnicians has been often drawn; like causes produce like results. For the same reason that they peopled the shores and islands of the Mediterranean with their colonies, we have caused them to be studded with our military posts. What the Phœnicians did for the flow of civilisation in days of old, we, if we fulfil our functions rightly, shall do for its returning ebb at the present time. Other European nations are concerned in continental movements, but, like the Phœnicians, our path is by the sea. Syria, Cyprus, Crete, and Greece was the line they traversed, and this is the line which sooner or later we appear destined to occupy in the struggle to come.

It is not well to carry a simile too far, but one other parallel, as a natural outcome of the instincts of the two people, may be fairly drawn. It is said that in art, in modern times at least, we have no style of our own. Neither had they; devoted to navigation and commerce, their art, instead of being indigenous, was borrowed from the nations with whom they traded. This is well shown in the collection of antiquities from Cyprus, for the knowledge of which we are indebted to General di Cesnola, the American consul in that island. Cyprus was one of the first islands colonised by the Phœnicians. Three distinct styles of art are recognised in the Cypriote pottery, sculptures and glyptic representations, the Assyrian, the Egyptian, and the Greek. In the temple of Golgoi the objects belonging to these three different styles were found separately placed, the Egyptian by themselves, the Assyrian in like manner, and the Greek also together, showing in the opinion of the author that they were collected at different epochs, spreading over a long series of years. On the other hand a considerable number of the objects figured in General Cesnola's book distinctly include both the Assyrian and the Egyptian, for example, in the patera from Curium, figured in p. 329; the centre figure represents a winged warrior, probably a king, fighting with a lion, which is in true Assyrian style, whilst the outer circle of the same vessel is ornamented with figures that are as purely Egyptian. Probably between the eighth and tenth centuries B.C. both styles may have prevailed in Cyprus at different times, but it is evident that a period arose in which both styles as well as the Greek were united, and closely imitated, and this constitutes the chief characteristic of the Cypriote art.

Very different in this respect are some of the objects discovered by Dr. Schliemann in the royal tombs at Mycenæ, which, though rude and barbarous—more so, indeed, than the majority of the Cypriote antiquities—nevertheless show some attempt at realism. More especially may be noticed the bull's head, the bas reliefs, and some of the gold ornaments. In these we perceive an absence of that servile imitation of earlier styles which has been noticed as the characteristic of Cypriote art; and although falling far short of Hellenic greatness, there is a freedom from conventionality which left the artist at liberty to turn to nature as his instructor, and thus, with the aid of a little imagination, we may perhaps recognise potentially in these rude designs the germs of those qualities which made Greek art so famous in the times that followed.

The concentric circles of the Cypriote ornamentation are here replaced by a system of coil ornaments which resemble those in use during the bronze age of Europe rather than anything to be found in the countries immediately to the eastward. Notwithstanding this, however, the connection with Cyprus is apparent in many of the forms. The rude terra-cotta figures of men and animals correspond very closely with those found in Cyprus as well as Rhodes, and the long-nosed warriors drawn on the fragment of a painted vase (p. 133, "Mycenæ") might clearly claim family relationship with the lady figured on the Cypriote vase in Fig. 394 of General Cesnola's work. The mode of ornamenting the eyebrows by means of parallel incised lines is distinctly Cypriote. But perhaps the objects which most

clearly attest the connection between the two places are the golden diadems (p. 186 "Mycenæ") found on the heads of the bodies in the tombs. These consist of pointed oval plates of gold, sometimes highly ornamented and having at the points, small holes by which they were fastened round the head with a wire. The position of the graves in which similar diadems to these were found at Idalium in Cyprus proves distinctly that they were more recent than the graves of the Phœnician period which lay beneath them. Similar forms of golden diadems from Kouyunjik are in the British Museum. The golden diadems found at Idalium are shown by these associated remains to belong to a more advanced period of art than the larger and more massive ones discovered in the royal tombs in the Agora at Mycenæ, the former being probably of the Greco-Roman age. Nevertheless the identity of the forms ought not to escape attention when considering the relative antiquity of the finds; they were, as Dr. Schliemann truly remarks (p. 189), in very extensive use in early times, and an investigation into the origin of these peculiar brow ornaments will without doubt have an important bearing on the period of the interments with which they are associated. It is to be regretted that General Cesnola, although he mentions the finding of these diadems in p. 75 of his work gives no illustration of them, but a number of them were sold at Sotheby's some years ago, and the remarks here made are based upon observations made at the time of the sale.

Turning now to Hissarlik our attention is naturally drawn in the first place to the so-called owl-faced vases which form so large a proportion of the antiquities discovered by Dr. Schliemann there. No subject has been more frequently applied to the ornamentation of funereal and other vases than the representation of a human face, as examples of which we may call to mind the rude jars representing Besa or Typhon in the Egyptian department of the British Museum, or our own Bellarmin jugs of the sixteenth century. Such representations are usually at first realistic, and expressive of the best endeavour of the designer, but in process of time the forms suffer degradation in the hands of inexpert or hasty workmen; the transmutation of form observable on British coins affords a well-known illustration of the gradual changes produced by means of imperfect copies, and similar degradation is often seen in the tribal and other ornaments and badges of modern savages. On the pottery found in the Peruvian graves a human face is of frequent occurrence. Some of these figures of faces are equal to the best productions of Cyprus or Mycenæ, whilst in others the features are so much dwarfed and distorted that little more than a line for the eyebrows and another for the nose remains to denote the intention of the potter, the other features having disappeared in those examples in which nothing more than a rude symbolism has been aimed at. An examination of the large collection of vases from Hissarlik, now exhibited by Dr. Schliemann at South Kensington is sufficient to show that this has been the true history of the γλαυκῶπις, or "owl-faced Goddess Minerva." In some of these vases all the features of the human face are present; in others some of them disappear or become conventionalised; the mouth is no longer represented, and the nose shrinks into a small beak-like projection beneath the eyebrows. Yet if the form of it is looked at carefully, it will be seen that it is still a nose, and in no case has it been the intention of the potter to represent a beak; its position is never that of an owl's beak beneath the line of the eyes. The eye of an owl is surrounded on all sides by a complete disc of feathers, but in no single instance has the lower and inner side of such a disc been represented on these vases; even in the most degraded examples the line which sweeps round the upper and outer portions of the eye is still seen to be an eyebrow, which is a feature that is entirely wanting in an owl. In many cases the ear has been retained, where the mouth has disappeared, and the ear is still distinctly human. It may be safely said that there is no example in the whole collection at South Kensington in which the form of an owl's face has been intentionally represented. In like manner the long upright projections on the sides of some of the vases, which, when associated with the symbolic features above spoken of, have been said to represent the wings of an owl, can be shown by a selected series to be nothing more than the handles of the pots developed and adapted to use in another form. Other handles, of which most of the pots are provided with three or four, have been dwarfed so as to dwindle into a mere reminiscence, marked by slightly raised lines on the sides of the vessels. Similar developments of handles may be seen in the specimens of terra-cotta lamps exhibited by the Palestine exploration committee at South Kensington. Then again, the small flat stone objects figured in page 36 of Dr. Schliemann's book, "Troy and its Remains," and supposed by him to be Athena idols, are clearly nothing more than symbolic vases. The lines denoting the face on these stone objects represent the face on the vases, the head, neck, and body of the vase and the horizontal lines across the neck marking the separation between the cover and body of the vase are all shown on these miniature models, which correspond to the stone models of vases which at a later period replaced those previously employed in Egyptian tombs, and it was no doubt by means of some such symbolism that these model vases at Hissarlik came to be introduced.

The peculiar "crown-shaped" covers found by Dr. Schliemann at Hissarlik, and figured at page 25, are of interest, and serve by their form to fix the position of the Hissarlik antiquities in point of sequence. These crown-like lids are survivals of the neck and handles of earlier forms whose history is to be traced in other parts of the Levant. The form of vase with two handles, one on either side joining the mouth and body of the vessel, of which a good example is represented on page 102 of General Cesnola's work, appears to have given rise to a shape with a closed or dummy neck, in which the form of the neck and handles are retained, but the real opening is in a funnel-shaped mouth adjoining the dummy neck. Dr. Schliemann found specimens of these altered vases in the tumulus at Sparta and also at Mycenæ. An illustration of one from the latter place is given at page 64 of his work on Mycenæ. They are common in Rhodes, examples of which may be seen in specimens from Ialysos, in the British Museum. They are also found in Attica, Cyprus, and in Egyptian tombs. The "crown-shaped" covers found at Hissarlik represent a further degradation of this form in which the neck has disappeared, the mouth

and handles only remaining. Three and four handles have been substituted, in some cases, for the double handle of the earlier vessels. The cover, with the dummy mouth and handles, of course occupies the position previously occupied by the true neck and handles on the top of the vase. As these crown-shaped covers are found in the lowest stratum, the "earliest city" discovered by Dr. Schliemann at Hissarlik, it follows, if the history of these forms has been correctly stated above, that the whole of the Hissarlik antiquities are of comparatively recent protohistoric date, though belonging, no doubt, to a people in a barbarous condition of culture, which accounts for the number of rude stone implements found from top to bottom throughout the excavations.

The so-called crest of the helmet of Athena (p. 283, Hissarlik), is a further degradation of these crown-shaped tops, and represents the dwarfed survival of one of the handles, the connecting links being represented by three specimens in the collection at South Kensington, where the vestiges of all three handles are shown in their proper places, and these were subsequently replaced by one, transferred for convenience' sake from the position formerly occupied by the three to the centre of the lid. In short, the history of every form may be traced by connecting links in the specimens exhibited at South Kensington, the whole collection forms a continuous sequence which, by judicious arrangement of connected forms, is capable of demonstration, and it is to be hoped that some such arrangement may be adopted before this interesting collection leaves the place. To apply the expression "Darwinism" to such a sequence of forms is no mere figure of speech, it expresses the truth as fully in its relation to savage art and ornament as to the forms of nature. Conservatism, acquired habits, and incapacity for improvement on the one hand, love of variety, economy of time and trouble, and imperfect copying on the other, combine to produce those slow and gradual changes which are characteristic of all barbarous art. Every object marks its own place in sequence by means of its form, and it is the recognition of this principle which supplies the place of written records in those prehistoric and protohistoric phases of culture with which we are dealing. Earlier forms are retained side by side with the more advanced ones and are applied to those objects and uses for which they appear fittest. If any evidence were wanting to disprove the absurd imputations that have been cast upon the genuineness of these antiquities, these connected varieties would alone suffice to prove that they were the work of a people in a very primitive condition of civilisation. Whatever difference of opinion may exist as to some of Dr. Schliemann's deductions no reasonable archæologist will be found to dispute the extraordinary merit of his discoveries. We are glad to hear that he is about to resume his excavations at Hissarlik. To the deep research and disinterested enthusiasm which has already placed him in the front rank of explorers, will now be added a large amount of archæological experience and knowledge of allied forms that he has acquired since his first excavations were conducted at this place, leading us to hope that his future discoveries will exceed them all in interest and importance.

PROFESSOR BELL'S "SELBORNE"

The Natural History and Antiquities of Selborne, in the County of Southampton. By the late Rev. Gilbert White, formerly Fellow of Oriel College, Oxford. Edited by Thomas Bell, F.R.S., F.L.S., F.G.S., &c., Professor of Zoology in King's College, London. 8vo. 2 vols. (London: Van Voorst, 1877.)

THE edition of this classic work for so many years expected from the hands of Prof. Bell, has at length appeared, and readers will regard it with much gratification and a little disappointment. The former feeling will arise from the large amount of new matter which it contains, and the latter from the conviction which cannot but force itself upon them that more was to be made of the whole than the editor seems to have been aware of. Yet Prof. Bell's long life—it is more than fifty years since he first won his spurs in the field of science—and his invaluable services in so many departments of zoology, render us very unwilling to say more than we are compelled in detraction of this, his latest labour, and the child of his old age. He writes now, as he always has written, pleasantly enough, but he fails to give us the notion that he has done the best he could with the materials placed at his disposal, and with his other unequalled opportunities. It is evident that his task grew upon him, and that a considerable portion must have been printed off before its extent was determined. This, indeed, is not an uncommon thing with young authors and editors; but Prof. Bell's literary experience, and the long time he is known to have had the present work in preparation, should have guarded him from an error of the kind. We might almost infer that when the memoir was written he had not mastered all the details of the deeply interesting correspondence which forms the bulk of his second volume, and certainly that he had not decided how many, and which, of the letters it contains should be given to the world. It is sufficient for us now to say that there is not one of them that could have been spared, for we must presently return to their consideration.

That any memoir of Gilbert White must, from the scarcity of facts relating to him, give a meagre account of that great and estimable naturalist, we are ready to admit, and that Prof. Bell's is at the same time far more copious than any other that has been published, will be obvious to all who are acquainted with the subject. But we cannot help regretting that the chief biographical facts have not been set forth in a clearer light than they appear, and proper as it is to tell us something of all the members of the family, we unfortunately find least is told us of those members of whom we should like to know most. Gilbert White had three brothers who were distinctly men of capacity above the average, beside two others much less, or hardly at all, distinguished. Of the former, Thomas, we are told, was successful in trade, and became a F.R.S., but in what trade or when he died we are left ignorant. Benjamin was the well-known publisher of natural history books—among others of Pennant's—for whom Prof. Bell has some hard words, not, perhaps, wholly undeserved, but it is very probable, to say the least, that had not Gilbert, through his brother, become acquainted with Pennant the "Natural History of Selborne" would never have

been written. The third remarkable brother was John, who was for a considerable time chaplain at Gibraltar, of which place he wrote a zoology that unfortunately was never printed, and of which the manuscript seems to have vanished, though Prof. Bell says the introduction to it is in his possession. Pity it is he has not given us this fragment, for some of the hints and suggestions that Gilbert was always imparting to John, his "most steady and communicative correspondent," must surely be therein contained, and it could not fail to have been a valuable addition to these volumes. In the next generation were "Jack," son of the aforesaid John, and a pupil of Gilbert's, who thought highly of him, and Samuel Barker, another nephew, an agreeable and evidently valued correspondent of his uncle's. It seems hardly possible but that diligent research would not have recovered more of these younger men than we find here recorded.

Of Gilbert himself we doubt not Prof. Bell has done all in his power to gather information, and in some respects he has been successful. Born at Selborne in 1720, he went to school at Basingstoke, and to Oxford in 1739. There he graduated B.A. in 1743, and the following year was elected to a Fellowship at Oriel, which he enjoyed till his death. Taking orders he successively held two curacies in Hampshire, one at Selborne till 1752, when he filled the office of Proctor (*Junior* Proctor, Prof. Bell is careful to tell us) for a year. Then he took another Hampshire curacy for a couple of years, at the end of which time he came once more to live at Selborne, which remained his home till his death. In 1757 he accepted the living of Moreton-Pinkney, in Northamptonshire, but the preferment must have been small, as it did not incapacitate him from holding his Fellowship, and, according to the custom of the times, residence was not required of him. The following year his father died, but he did not come into the family property at Selborne—"The Wakes," now possessed by Prof. Bell—until the death of an uncle in 1763. He seems to have made Pennant's acquaintance about 1767, or perhaps a little earlier. In 1768 we find him writing to Banks, and the following year began his correspondence with Barrington, who, in 1774 and 1775, communicated to the Royal Society those ever-memorable monographs of the British *Hirundines*, which first made known White's powers of observation and felicity of expression. In 1774 he refused no fewer than three college livings, for he was doubtless in easy circumstances, and once more accepted the curacy of his birth-place. At the age of sixty-nine his single book was published, and he survived its appearance just four years. Another event in his life must be noted here —his attachment to the sister of his college friend Tom Mulso. What hindered their union does not appear, but in 1760 the lady was married to Mr. Chapone, and was subsequently the authoress of several well-known works, and a celebrated "blue-stocking." We have to thank Prof. Bell for collecting most of these facts and dates now for the first time published, but they are not very easily gathered from his memoir.

Of course we have no occasion here to review the letters to Pennant and Barrington which formed the original "Natural History of Selborne." Their place in literature and science is assured. It were impertinent to speak of their merits, or to indicate their few—very few —defects. Being the results of the personal experience of their author they will hold their ground for all time. Never before, perhaps, was there so careful an observer, and since, we know of but one other so accurate. That other has no doubt surpassed his predecessor in the ingenuity of his induction and the versatility of its application, but it is no detriment to Gilbert White that he should be ranked as an observer second only to Mr. Darwin. Numerous editors have tried their hand in annotating this ever-popular work, and many more will make the same attempt. Prof. Bell is chary—too chary, perhaps, of his comments—but if he errs he errs on the safe side, and readers who have been disgusted with the inanity or the flippancy of the notes to some recent editions, will rejoice that in him they have an editor whose remarks, if they be but few, are always to the point, and never in bad taste.

Now we ought to consider the new letters, but the length of this article warns us that we must be brief in what we say of them. They remove the present edition from comparison with any other, and we have sincerely to thank Prof. Bell for having shown us, by printing them, that White was even more than had formerly appeared. Every grace of style, every power of thought—in a word, every good quality which was foreshadowed in the famous epistles to Pennant and Barrington is doubled, or more than doubled in intensity in the letters now given to the public—letters, too, which were never prepared by their writer for publication. We have him before us as the instigator to good works, the sage adviser in matters literary and scientific, the self-denier, the man of affectionate relations, the man of high aspirations, yet humble; simple, yet full of humour; a recluse, yet a man of the world in the best sense. We long to subjoin extracts from them, but want of space renders that impossible. Our readers will read and judge for themselves. It must suffice to say that there are more than one hundred from Gilbert's pen, of which scarcely a dozen have ever been printed before, in addition to a most interesting correspondence between John White and Linnæus on the zoology of Gibraltar, and letters from various members of the family which faithfully reflect, as it were, in a remarkable manner Gilbert's own nature, besides a few—too few, unfortunately—addressed to him by men like Lightfoot, Skinner, Montagu, and Marsham. For the sake of these we readily forgive all the shortcomings of the present volume—even the want of a table of contents and of a good index.

OUR BOOK SHELF

Proceedings of the London Mathematical Society, vol. viii. (November, 1876, to November, 1877), 321 pp. (Messrs. Hodgson.)

THIS goodly-sized volume bears testimony to the activity of its members, and contains twenty-nine papers, published *in extenso*. We may specially refer to one or two. The "Pure" side of the subject of mathematics, as usual, is the favoured one, and furnishes memoirs by Prof. Cayley on the condition for the existence of a surface cutting at right angles a given set of lines, on a general differential equation, geometrical illustration of a theorem relating to an irrational function of an imaginary variable, on the circular relation of Möbius

and on the linear transformation of the integral $\int \frac{du}{\sqrt{U}}$.—Prof. Clifford has an excellent paper on the canonical form and dissection of a Riemann's surface. Prof. H. J. S. Smith contributes the conditions of perpendicularity in a parallelepipedal system, and a very interesting presidential address on the present state and prospects of some branches of pure mathematics. Mr. Spottiswoode writes on curves having four-point contact with a triply infinite pencil of curves, and Prof. Wolstenholme gives an easy method of finding the invariant equation expressing any poristic relation between two conics.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Trajectories of Shot

I HOPE you will be able to afford me space for a few remarks on the following extract from a paper on the Trajectories of Shot, by Mr. W. D. Niven, which appeared in the *Proceedings* of the Royal Society for 1877.

Mr. Niven arranges his paper under three heads, calling them the first, second, and third methods. The third method is the one he favours, while he endeavours to dispose of the other two in the following terms :—

"§ 11. It will be observed that the two foregoing methods each open with the same equation (a). Now there is a serious difficulty in the use of that equation. Suppose, for example, we were to integrate over an arc of 1°, we should have to use the mean value of k between its values corresponding to the velocities at the beginning and end of the arc. But we do not know the latter of these velocities ; it is the very thing we have to find. The first steps in our work must be to *guess* at it. The practised calculator can, from his experience, make a very good estimate. Having made his estimate, he determines k. He uses this value of k in equation (a), and if he gets the velocity he *guessed* at, he concludes that he *guessed* rightly, and that he has got the velocity at the end of the arc. If the equation (a) does not agree with him, he makes *another guess*, and *so on till he comes right*."

The case would be indeed hopeless, if all this was quite correct. But I have to inform Mr. Niven that, in all proper cases v_β may be found *accurately* from equation (a), and without any "guessing" whatever. Taking Mr. Niven's own solitary example, I will *calculate* the value of v_β at the end of an arc, not of 1°, but of 3°, and compare my result with his own. The initial velocity, v_α, is here 1,400 f.s., and the corresponding value of the coefficient k_α, given in my table, is 104·0. Substitute this value for k in equation (a), given below, and v_β will be found 1291·7 f.s., a *first* approximation. Now *calculate* the mean value of k between velocities 1,400 and 1,290 f.s. by the help of the table, and it will be found to be equal to 106·3. Substitute this new value of k in equation (a), and v_β will be found 1289·8 f.s., a *second* approximation. We must stop here, because if we attempted to carry the approximation further, we should obtain the same value of k, and therefore of v_β, as in the second approximation. Mr. Niven finds $v_\beta = 1290$ f.s.

Of course in ordinary cases, a calculator, in making his first approximation to v_β, would commence by taking a value of k corresponding to a velocity *somewhat below* the initial velocity. In this way a better *first* approximate value of v_β would be found. Thus, again referring to Mr. Niven's own example, I will take a step over an arc of 6°, from $\alpha = +3°$ to $\beta = -3°$. The initial velocity is 1,400 f.s. I now go so far as to "guess" that the mean value of k will correspond to a velocity considerably below 1,400 f.s., and take $k = 107·9$, corresponding to a velocity 1,300 f.s. This gives $v_\beta = 1208·1$, a *first* approximation. The mean value of k between 1,400 and 1,210 f.s. is now found to be 107·2, which gives $v_\beta = 1209·0$ f.s. Mr. Niven obtains 1207·4 by stepping over two arcs of 3°. If any further adjustment was required, proportional parts might be used, seeing that a correction $\delta k = -0·7$ gives $\delta v_\beta = +1·8$.

Mr. Niven then proceeds to question the *accuracy* of what he is pleased to call the "process of guessing," as follows :—

"It seems to me, however, that this method of going to work, leaving out of account the loss of time, is open to objection in *the point of accuracy*. For, first there is no method of determining on what principle the mean value of k is found—what manner of mean it is. Again, let us suppose for an instant that the velocity at the end of the arc *guessed* at, and the value of k, are in agreement ; that is to say, let the equation

$$\left(\frac{1,000}{v_\beta}\right)^3 \sec^3\beta - \left(\frac{1,000}{v_\alpha}\right)^3 \sec^3\alpha = \frac{d^2}{w}\,\frac{k}{g}(P_\alpha - P_\beta) - \text{(a)}$$

hold for the values of v_β and k used by the calculator. It by no means follows that he has hit on the right value of v_β and k. For if he is dealing with a part of the tables in which $\frac{dk}{dv}$ happens to be nearly equal to $-3\frac{wg}{d^2}\,\frac{\sec^3\beta}{P_\alpha - P_\beta}\,\frac{(1,000)^3}{v^4}$, it is obvious that there are ever so many pairs of values of v_β and k which will stand the test of satisfying the above equation. Now an examination of Mr. Bashforth's tables for ogival-headed shot shows that the value of k diminishes as v increases from 1,200 feet upwards, so that $\frac{dk}{dv}$ is negative for a considerable range of values of v which are common in practice. It is not at all unlikely, therefore, that the value for $\frac{dk}{dv}$ just stated may often be very nearly true ; in which the case the *process of guessing* becomes extremely dangerous."

I here observe that Mr. Niven is not entitled to assume that, because two quantities have the *same sign*, they will therefore be probably often nearly of *equal value*. Without discussing the value of his test of danger, I have to state that my tabular value of $\frac{\delta k_\beta}{\delta v_\beta}$, for velocities above 1,200 f.s., lies between 0 and $-0·09$. I have calculated the numerical values of Mr. Niven's expression for $\frac{dk}{dv}$, for shot fired from various guns, from the Martini-Henry rifle up to the 80-ton gun, and have always obtained a numerical result so far outside the limits of the tabular value, that, for the present, I conclude that Mr. Niven's condition (whatever may be its value) *is never nearly satisfied in any practical example.* But when a practical case is produced where "ever so many pairs of values of v_β and k," *differing sensibly*, "stand the test of satisfying the above equation" (a), it shall receive my best attention.

It is well known that the problem of calculating the trajectory of a shot, like so many other practical problems, does not admit of a direct and complete solution. So that all solutions, being approximations, are more or less erroneous. But I feel perfect confidence in the results given by my methods of calculation, because, the smaller the arcs taken at each step, and the nearer the *calculated* will approach to the *actual* trajectory. But methods of approximation require to be used with judgment. For instance with the heaviest shot in use, we may take steps of 5° for velocities above 1,100 f.s. ; while for small arm bullets arcs of half a degree will be quite large enough. In any case of real difficulty the remedy will be to divide the trajectory into smaller arcs.

From what I have said it appears that my method of finding the trajectories of shot, *when properly applied*, is neither a "process of guessing" nor yet "dangerous."

Minting Vicarage, March 8 F. BASHFORTH

Australian Monotremata

I AM surprised to find that "P. L. S." (vol. xvi. p. 439), was not aware that the Echidna *Tachyglossus hystrix*, is found in N. Queensland. For the benefit of your readers I may mention that the Australian Museum possesses a fine specimen of *T. hystrix* from Cape York. Mr. Armit, of Georgetown, Mr. Robt. Johnstone, and others, have frequently found them in various parts of Queensland. One specimen from Cape York was obtained there by our taxidermist, J. A. Thorpe, in 1867.

The Platypus (*Ornithorhynchus anatinus*) is also found in Queensland as far north as the Burdekin at least, perhaps further.

Tachyglossus, strictly speaking, has no pouch, but the *areola*

is sunk in the skin, and when the young are first born this depression, or miniature pouch, is large enough to hold them; when about a month or so old, their hinder parts may be seen sticking out; when two or three months old, only the head, and afterwards, as they become larger, only the snout is hidden. the marsupial bones, which are well developed, support the weight of the young one while sucking. The young does not leave the mother until at least one-third grown, and *even when fully the size* of the adult, the quills are only then beginning to show through the skin, which is black, and thinly covered with black hair.

The new species, *T. lawesii*, Ramsay, from Port Moresby, may be distinguished at once by the stiff flat bristles of the face and the more cylindrical form of its spines; *T. bruijnii* has a very long snout, nearly twice the length of any other species at present known. See *Proceedings* L. Soc. of N. S. W., Vol. ii., Pt. 1. Pl. 1. E. P. RAMSAY

Australian Museum, Sydney, January 25

P.S.—It may interest your readers to know that Messrs. Ramsay Bros., of Maryborough, Queensland, have a fine series of *eleven Ceratodus alive* in a large tank constructed for them. These fish have now lived and thriven well in confinement for over eighteen months. I was the first to send the Ceratodus in spirits to England, although I never got the credit of it; nor did any of those naturalists to whom I forwarded specimens through a friend at the Zoological Society, ever think it worth their while to acknowledge them. Had it been otherwise, living specimens would have found their way to England long since. It is a great mistake to suppose the Ceratodus is now common; they can only be obtained at certain seasons and in certain parts of the Rivers Mary and Burnet. The Osteoglossum (*Barramundi*), with which the Ceratodus (*Teebi ne*) is often confounded, is plentiful enough in the western waters of Queensland.

E. P. R.

Fetichism in Animals.—Discrimination of Insects

I HAVE frequently noticed the fetichism of dogs, and was therefore much interested by Mr. G. J. Romanes' letter of December 27, which I have but just seen. Our terrier—a very queer character and a great warrior—is abjectly superstitious. He will not come near a toy cow that lows and turns its head, but watches it at a distance with nose outstretched. A vibrating finger-glass terrifies him; indeed he has so many superstitions that we often make him very miserable by working on his fears. I feel sure he constantly tries to understand, but never gets further than the sense of "uncanny"-ness. Dogs vary greatly as to this.

A propos of the discriminating power of insects. I have seen humming-bird moths deceived by sight. They were seeking in an open loggia, ceiled with wood, some dark place in which to hide; the pine wood was studded with brown knots. Again and again the two moths flew from knot to knot, felt and rejected them. At last they reached the open work—holes which looked much like the knots—and in them they hid themselves.

I was much struck at the time, as it appeared to me to show they possessed some dim sense of colour, but no defining perception of surface. C. G. O'BRIEN

Cahirmoyle, Ardagh, Co. Limerick

Nitrification

IT seems right to direct attention to the fact that Bacteria were observed by Meusel to convert nitrates into nitrites; an abstract of which observations is to be found in the *Annals and Magazine of Natural History* for February, 1876; this abstract is copied from *Silliman's Journal* for January, 1876, where the reference to Meusel's paper will be found. This reference is *Ber. Berl. chem. Gesel.*, October, 1875.

No indication of their knowledge of these observations is to be found in Schloesing and Munk's paper in the *Comptes Rendus* (February, 1877) or in Mr. Warington's communication to NATURE, vol. xvii. p. 367. F. J. B.

Oxford, March 11

The Wasp and the Spider

MAY I suggest a possible explanation of the curious case of spider-hunting by a wasp cited by Mr. Cecil; had the prey so accurately tracked by the wasp been anything but a spider, it would, indeed, have seemed an almost conclusive instance of hunting by scent; but when one recollects the fine line usually left by spiders as they go, it is evident that sight or feeling may have been the sense exercised, and that the fatal clue may have been the guide to the wasp. E. HUBBARD

March 18

ENTOMOLOGY AT THE ROYAL AQUARIUM

AN aquarium is put to its legitimate use when it is made the home of natural history exhibitions, and any attempt to rescue one from the too dominant sway of the showman deserves every support at the hands of science. The Entomological Exhibition, the opening of which at the Royal Aquarium we noticed last week, is also quite a novelty, though it is the outcome in a particular branch of the idea that led to the Loan Exhibition of Scientific Apparatus at South Kensington; as in that case the exhibitors are induced by no hope of prizes, but merely from the love of their science to lend their treasures. One learns from such an exhibition as this how much genuine love for natural history exists amongst men whose daily lives are devoted to manual labour, and that there are those who live within sound of Bow Bells, who make as good a use of their more limited opportunities as Edward in Banffshire. Here is a Mr. Machin, compositor by trade, whose long day's work has not prevented him from collecting and rearing a magnificent series of crepuscular and nocturnal moths, shown in twenty beautifully-arranged cases and accurately named; and the collections of some others are scarcely less noticeable in this respect. But apart from the interest attaching to some of the exhibitors, the material brought together affords an opportunity both to the entomologist proper and to the general naturalist not often to be met with. The greater portion of the whole exhibition is perhaps inevitably taken up with British lepidoptera, but these are not, as might be feared, an endless multitude of specimens of no special interest beyond their rarity and beauty, but are made to teach as well as please. Lord Walsingham, for example, shows the larvæ, pupæ, and imagines of nearly 370 species with the plants on which they occur—so that we have their complete life-history so far as it can possibly be represented to us. This, perhaps, from its scientific character and the beautiful means of preservation adopted, is the most interesting to the general naturalist, but there are others more limited, but scarcely less instructive—as those shown by the Messrs. Adams, in which the usual parasites are included in the series with each insect. Other instructive collections are those which illustrate the varieties of a single species; such is the set of specimens of *Colias edusa*, exhibited by Mr. Harper, a grand series showing insensible passages between perfectly distinct colourings. The influence of climate on colour is well illustrated in the melanic northern varieties of several species of moths, which are usually of a lighter colour in the south of England, the two varieties being placed side by side in the Yorkshire collections, and the results of selective breeding in the same direction in the photographs, unfortunately not specimens, of the common gooseberry moth, varying from nearly white to almost entirely dark. The moths and butterflies of the fen districts, which are now becoming so scarce, are represented by a very large collection by Mr. Farn. But one of the most interesting objects is a large white close-set web, in appearance like a cloth—some eight feet by four feet, spun by the larvæ of a moth, *Ephestia elutella*, that feeds on chicory. It is a portion only of a larger web, six times the size, formed on the walls and ceiling of a chicory warehouse in York, by the incessant marching to and fro of the well-fed larvæ. The threads composing it are less than $\frac{1}{6000}$ inch in diameter, and as they are nearly contiguous and eight or ten deep, the portion exhibited represents about 4,000 miles of their wanderings. When twisted into a rope, it has been made to support a

weight of 56lbs. The foreign Lepidoptera also figure largely, and are naturally attractive from their beauty, and in General Ramsay's cases from Nepaul, for their rarity. This portion of the series, however, is chiefly valuable for the illustrations of protective mimicry which it affords. Admirable specimens of the leaf butterfly, *Kallima inachis*, with the varying tints of their under surfaces, are in Gen. Ramsay's collection, and Mr. Swanzy has a grand series specially arranged of Diademas and Papiliones mimicking—some in the females and some in both sexes—the nauseous smelling members of the Danaidæ and Acraidæ. Similar series are shown by Rev. J. A. Walker and Mr. Weir. The extraordinary differences between male and female in some butterflies is well illustrated by Mr. Briggs' collection of Lycænas.

The remaining orders are in some instances admirably illustrated, but by far fewer exhibitors. Dr. Powers' nearly complete collections of British coleoptera and British hemiptera, are among the best ever made; and Mr. Frederick Smith's hymenoptera, which supplied much of the material for the British Museum Catalogue, and Mr. Stevens' exhaustive collection of weevils, both the results of forty years' work, are here exhibited. A most instructive series of Grecian hymenoptera, with their galleries bored in briars, and some magnificent coleoptera from Ashantee, containing beautiful examples of *Goliathus Drurii*, complete the list of the more noteworthy objects. Some important orders are thus without special illustration here, but no doubt this will not be the last as well as the first of such exhibitions; and when it comes round to the insects again we may hope to see as complete sets of diptera or neuroptera as of other orders. It would be a great advantage to students if such exhibitions of limited classes could be periodically instituted by loan, and Mr. Carrington certainly deserves our thanks for the idea and its successful realisation.

THE GOVERNMENT RESEARCH FUND

THE following list of grants to be paid from the Government Fund of 4,000*l.* on the recommendation of the Royal Society, during the present year, in aid of scientific research, has been sent us for publication :—

Not Personal.

David Gill, 93, Wimpole Street, W.—To defray Expenses connected with a Determination of the Solar Parallax by Observation of the Diurnal Parallax of Mars £250

Rev. Dr. Haughton, Trinity College Dublin.—For Aid in the Numerical Reductions of the Tidal Observations made on board the *Discovery* and *Alert* in the late Arctic Expedition £75

Prof. Fleeming Jenkin, 3, Great Stuart Street, Edinburgh.—For Experimental Investigations on Friction £50

W. Chandler Roberts, Royal Mint, Tower Hill, E.—For Researches on Metals and Alloys in a Molten State passing through Capillary Tubes £25

J. Kerr, Free Church Training College, Glasgow.—For Continuation of Electro-Optic and Magneto-Optic Researches £50

J. Norman Lockyer, 16, Penywern Road, South Kensington, S. W.—For Continuation of Spectroscopic Researches £200

Dr. O. J. Lodge, University College, Gower Street, W.C.—For Investigations into the Effect of Light on the residual Charge of Dielectrics; on the Conductivity of Hot Glass, and other Transparent Conductors, on Electrolytic Conduction, and other Subjects £100

Thomas Stevenson, Hon. Sec. Scottish Meteorological Society, General Post Office Buildings, Edinburgh.—For Aid in carrying on a Simultaneous Series of Anemometrical Observations at different heights, and in sheltered and unsheltered situations £50

W. Galloway, Cardiff.—For further Investigation of the Explosive Properties of Mixtures of Fire Damp and Coal Dust with Air £100

Sir William Thomson, University College, Glasgow.—For Tidal Investigations £100

For Experiments in Magnetisation of different Qualities of Iron, Nickel, and Cobalt under varying Stresses and Temperatures £100

J. E. H. Gordon, Pixholme, Dorking.—For Continuation of Experimental Measurements of the Specific Inductive Capacity of Dielectrics £100

H. Tomlinson, 36, Burghley Road, Highgate Road.—For Researches on the Alteration of Thermal and Electrical Conductivity produced by Magnetism, and on the Alteration of Electrical Resistance produced in Wires by Stretching £100

Prof. H. Alleyne Nicholson, University of St. Andrew's; R. Etheridge, jun., Geological Survey Office, Edinburgh.—For Aid in examining the Fauna of the Silurian Deposits of the Girvan District, Ayrshire, and in publishing a Descriptive List of the same £75

R. McLachlan, 39, Limes Grove, Lewisham.—For Aid towards the Expense of Publication of a Revision and Synopsis of European Trichoptera £50

C. Callaway, Wellington, Shropshire.—For Aid in working out the so-called Eruptive Rocks of Shropshire, and in verifying certain points in Local Geology £25

H. T. Stainton, Mountsfield, Lewisham.—In Aid of the Publication Fund of the Zoological Record Association... ... £150

Dr. J. W. Dawson, McGill College, Montreal.—For Aid in excavating Erect Trees in the Coal Formation of Nova Scotia, in Beds where they are known to contain Reptilian and other Remains £50

Dr. R. H. Traquair, Museum of Science and Art, Edinburgh.—For Aid in preparing and publishing a Monograph on the Carboniferous Ganoid Fishes of Great Britain £75

W. Saville Kent, St. Helier's, Jersey.—To pay for Microscopical Apparatus for the Further Prosecution of Investigations into the Structure and Life-History of certain Lower Protozoa £50

Dr. W. A. Brailey, 38, King's Road, Brownswood Park, Green Lanes, N.—For Researches on the Causes determining the Tension of the Globe of the Eye in Man and Animals, and on the Physiological Influence on this Tension of such Substances as Atropia, Daturin, Eserine, and Pilocarpine £25

E. A. Schäfer, University College, Gower Street.—For Payment of an Assistant in Continuing his Histological and Embryological Investigations £50

H. Woodward, 117, Beaufort Street, Chelsea.—For Continuation of Work on the Fossil Crustacea, especially with reference to the Trilobita and other Extinct Forms, and their Publication in the Volumes of the Palæontographical Society £75

Prof. H. G. Seeley, 61, Adelaide Road, N.W.—For an Examination of the Structure, Affinities, and Classification of the Extinct Reptilia and Allied Animals £75

Dr. C. R. A. Wright, St. Mary's Hospital, Paddington.—For Continuation of Researches on Certain Points in Chemical Dynamics; on the Determination of Chemical Affinity in Terms of Electrical Magnitudes; and on some of the lesser-known Alkaloids £100

Prof. C. Schorlemmer, Owens College, Manchester.—For Continuation of Researches into (1) The Normal Paraffins. (2) Suberone. (3) Aurin £100

Prof. E. J. Mills, 234, East George Street, Glasgow.—For a Research on Standard Industrial Curves £100

Personal.

J. Allan Broun, 9, Abercorn Place, St. John's Wood, N.W.,—For Continuation of Correction of the Errors in the published Observations of the Colonial Magnetic Observatories £150

Dr. J. P. Joule, 12, Wardle Road, Sale, near Manchester.—For an Exhaustive Inquiry into the Change which takes place in the Freezing and Boiling Points of Mercurial Thermometers by long Exposure to those Temperatures £200

Prof. W. K. Parker, 36, Claverton Street, S.W.—For Assistance in Continuation of Researches on the Morphology of the Vertebrate Skeleton and the Relations of the Nervous to the Skeletal Structures chiefly in the Head £300

Prof. A. H. Garrod, 10, Harley Street, W.—For Aid towards Publication of the Second Fasciculus of an Exhaustive Treatise on the Anatomy of Birds £100

Rev. J. F. Blake, 11, Gauden Road, Clapham, S.W.—For Aid in continuing the Publication of a Synopsis of British Fossil Cephalopoda £100

Dr. W. A. Brailey, 38, King's Road, Brownswood Park, Green Lanes, N.—For Researches on the Causes determining the

Tension of the Globe of the Eye in Man and Animals, and on the Physiological Influence on this Tension of such Substances as Atropia, Daturin, Eserine, and Pilocarpine £25

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Prof. Schorlemmer, Owens College, Manchester.—For Continuation of Researches into (1) the Normal Paraffins, (2) Suberone, (3) Aurin £150

W. N. Hartley, King's College, Strand.—For Investigation of the Fluid Contents of Mineral Cavities; of the Properties of the Phosphate of Cerium; of Methods of Estimating the Carbonic Acid in small Samples of Air; and of Photographic Spectra £150

Dr. Armstrong, Lewisham Village, S.E.—For Continuation of Researches into the Phenol Series... ... ... £250

THE SOURCES OF LIGHT[1]

WHEN the sun rises in the morning, the darkness of the night seems to fade away, and, wherever we look, without or within, all the air and space about us appears to be full of light. When evening comes again, the daylight disappears, and the moon and the stars give us another light. In the house we start the lamps, and they give us another light. Out-of-doors, in the dusky meadows, we see the fire-flies darting about, and giving out pale sparkles of yellow light as they fly. We look to the north in the night and see the aurora, or we watch the lightnings flash from cloud to cloud, and again we see more light.

This light from sun and moon, the stars, the fire, the clouds, the sky, is well worth studying. It will give us a number of the most beautiful and interesting experiments, and by the aid of a lamp, or the light of the sun, we can learn much that is both strange and curious, and perhaps exhibit to our friends a number of charming pictures, groups of colours, magical reflections, spectres, and shadows. All light comes from bodies on the earth or in the air, or from bodies outside of the atmosphere; and these bodies we call the sources of light. Light from sources outside of the atmosphere we call celestial light, and the sources of this light are stars, comets, and nebulæ. The nebulæ appear like flakes and clouds of light in the sky, and the comets appear only at rare intervals, as wandering stars that shine for a little while in the sky and then disappear. The stars are scattered widely apart through the vast spaces of the universe, and they give out their light both day and night. The brightest of these stars is the sun. When it shines upon us, the other stars appear to be lost in the brighter light of this greater star, and we cannot see them. At night, when the sun is hid, these other stars appear. We look up into the sky and see thousands of them, fixed points of light, each a sun, but so far away that they seem mere spots and points of light. Besides these stars are others, called the planets, that move round the sun. These give no light of their own, and we can only see them by the reflected light of the great star in the centre of our solar system. Among these stars are the Moon, Venus, Mars, Jupiter, and many others. We might call celestial light starlight; but the light from the great star, the sun, is so much brighter than the light of the others, that we call the light it gives us sunlight, and the light from the other suns we call starlight. For convenience, we also call the reflected light from the planets starlight, and the light from our nearest planet we call moonlight.

Terrestrial light includes all the light given out by things on the earth, or in the air that surrounds the earth. The most common light we call firelight, or the light that that comes from combustion. When we light a lamp or candle, we start a curious chemical action that gives out light and heat. The result of this action is fire, and the light that comes from the flame is firelight. When a thunder-storm rises, we see the lightning leap from the clouds, and give out flashes of intensely bright light. Sometimes, at night, the northern sky is full of red or yellow light, darting up in dancing streamers, or resting in pale clouds in the dark sky. You have seen the tiny sparkles of light that spring from the cat's back when you stroke her fur in the dark, or have seen the sparks that leap from an electrical-machine. All these—the aurora, the lightning, and the electric sparks—are the same, and we call such light electric light.

Sometimes, in the night, we see shooting-stars flash across the sky. These are not stars, but masses of matter that, flying through space about the earth, strike our atmosphere and suddenly blaze with light. The friction with the air as they dart through it is so great that these masses glow with white heat, and give out brilliant light. Two smooth white flint pebbles, or two lumps of white sugar, if rubbed quickly together, will give out light, and this light we call the light from mechanical action.

Sailors upon the ocean sometimes see, at night, pale-yellow gleams of light in the water. A fire-fly or glow-worm imprisoned under a glass will show, in the dark, bright spots of light on his body. A piece of salted fish or chip of decayed wood will sometimes give a pale, cold light in the night; and certain chemicals, like Bologna phosphorus and compounds of sulphur, lime, strontium, and barium, if placed in the sunlight in glass vessels and then taken into the dark, will give out dull-coloured lights. All these—the drops of fire in the sea, the glow-worm, the bit of decayed wood, and these chemicals—are sources of the light called phosphorescence.

These are the sources of light—the stars, the fire, electricity, friction, and phosphorescent substances. We can study the light from all of them, but the light from the sun or a lamp will be the most convenient. The light of the sun is the brightest and the cheapest light we can find, and is the best for our experiments. A good lamp is the next best thing, and in experimenting we will use either the sun or a lamp, as happens to be most easy and convenient.

The Heliostat.

In looking out of doors in the daytime we find that the sunlight fills all the air, and extends as far as we can see. It shines in at the window and fills the room. Even on a cloudy day, and in rooms where the sunshine cannot enter, the light fills everything, and is all about us on every side. Now, in studying light we do not wish a great quantity. We want only a slender beam, and we must bring it into a dark room, where we can see it and walk about it and examine it on every side, bend it, split it up into several beams, make it pass through glass or water, and do anything else that will illustrate the laws that govern it.

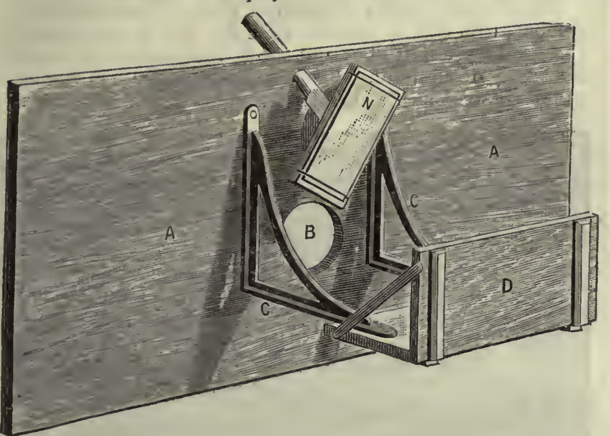
Choose a bright, sunny day, and go into a room having windows through which the sun shines. Close the shutters, curtains and blinds, at all the windows save one. At this window draw the curtain down till it nearly closes the window, and then cover this open space with a strip of thick wrapping-paper. Cut a hole in this paper about the size of a five-cent piece, and at once you will have a slender beam of sunlight entering the hole in the paper and falling on the floor. Close the upper part of the window with a thick shawl or blanket, and, when the room is perfectly dark, our slender beam of light will stand out clear, sharp, and bright.

As soon as we begin to study this beam of light, we find two little matters that may give us trouble. The sun does not stand still in the sky, and our beam of light

[1] From a forthcoming volume of the "Nature Series"—"Light: a Series of Simple, Entertaining, and Inexpensive Experiments in the Phenomena of Light, for the Use of Students of Every Age," by Alfred M. Mayer and Charles Barnard.

keeps moving. Besides this, the beam is not level, and it is not in a convenient place. We want a horizontal beam of light, and some means of keeping it in one place all day. An instrument that will enable us to do this, and that can be adjusted to the position of the sun in the sky at all seasons of the year and every hour of the day, may be readily made, and will cost only a small sum of money.

We give several drawings giving different views of such an instrument and some of its separate parts. It is called a heliostat, and we shall find it of the utmost value in our experimenting in light, heat, sound, electricity, and other branches of physical science.



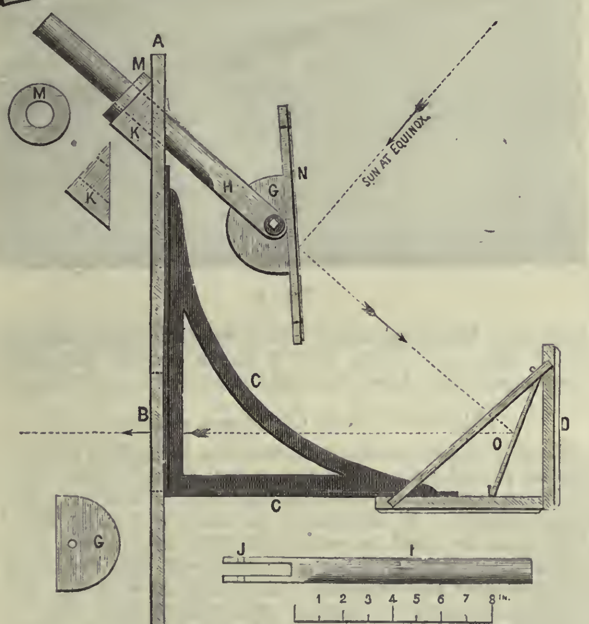


Fig. 1.

The first drawing represents a front-view of the heliostat. The second drawing gives an end-view, and we can now make one by simply following these few directions: The part marked A in the two drawings is a piece of pine board, 23 inches (58·4 centimetres) wide and two or more feet long, or as long as the window where it is to be used is wide. Any boy who can use plane and saw can make this piece of work out of common inch-board, and, if you have no pieces so wide as that, it can be made of two or more pieces fastened together with cleats; but, in this case, all the cracks must be close and tight. In the middle of this board, cut a round hole 5 inches (12·7 centimetres) in diameter, with its centre 8 inches from the bottom of the board. In the first drawing this hole can be seen at B, and in the second drawing it is shown by dotted lines at B. On one side of the board screw two iron brackets, using brackets measuring 14 inches (35·5 centimetres) by 12 inches (30·5 centimetres). These brackets are placed one on each side of the hole in the board, and are placed 14 inches (35·5 centimetres) apart, and with the short arm of the bracket against the board. In the first drawing the two brackets are shown, and in the second drawing one is shown in profile, and they are marked C in both drawings. On the end of the brackets is placed a flat piece of board, $6\frac{1}{2}$ inches (16·5 centimetres) wide and 14 inches (35·5 centimetres) long, or long enough to reach from one bracket to the other. This board may be screwed up to the brackets, and thus make a shelf. Care must be taken in fastening this shelf to the brackets to place it so that the outside edge of the shelf will be 16 inches (40·6 centimetres) from the large board. On the outside edge of this shelf another board, 7 inches (17·8 centimetres) wide, is placed upright, and secured with screws and small strips of wood at the ends, as in the drawing. This shelf, with the wooden back, is marked D in the drawings.

These things make the fixed parts of the heliostat, and we have next to make the movable parts, or the machinery whereby it can be adjusted to the movement of the sun in the heavens. First, get out a flat piece of board $10\frac{1}{2}$ inches (26·7 centimetres) long, $6\frac{1}{4}$ inches (16 centimetres) wide, and $\frac{1}{2}$ inch (12 millimetres) thick. Then make a flat, half-round piece, shaped like the figure marked G. This piece must be $\frac{1}{4}$ inch (7 millimetres) thick, $5\frac{1}{2}$ inches (14 centimetres) along the straight side, and with the circular part with a radius of 3 inches (7·6 centimetres). A hole, $\frac{1}{2}$ inch (12 millimetres) in diameter, is made in this, as represented in the drawing, and then the half-round piece must be screwed to the flat piece of wood we just cut out. In the part marked N in Fig. 1 you will see these two pieces fastened together. The piece marked I is the most difficult piece of all. It should be made of ash or some hard wood. One end is square, and has a deep slot cut in it; the rest is round, and may be $1\frac{1}{4}$ inch (32 millimetres in diameter. The square part must be large enough to slip over the half-circular piece, G, as is shown at H. A hole, $\frac{1}{2}$ inch (12 millimetres) in diameter, is cut in the two ends, as marked by dotted lines at J, and through these holes an iron bolt and nut are fitted, so as to hold the circular piece, G, and yet allow it to turn freely in every direction. A hole, $1\frac{1}{4}$ inch (32 millimetres) in diameter, is cut through the triangular piece of wood K, as shown by the dotted lines, and then this block is securely fastened to the back of the large board, as shown in the second drawing. An opening of the same diameter, and having the same direction, is also cut through the board, and the movable piece, marked I, is put through this hole, as in the drawing. Finally, we want a wooden washer, $3\frac{1}{2}$ inches (8·7 centimetres) wide, as represented at M. This we slip over the long wooden handle, as shown in the second drawing, and this washer rests on the block K, the top of which is $3\frac{1}{2}$ inches square. This makes all the movable parts of the heliostat, and, when we have put in the mirrors, the instrument is finished and ready for use. We must have two mirrors, one 6 inches (15·2 centimetres) square and one 10 inches (25·4 centimetres) long and 6 inches (15·2 centimetres) wide. These may be made of common looking-glass: but plate-glass with silvered back is far better, and costs only a little more.

Any carpenter can make this instrument, and the cost will be about as follows: Wood, 50 cents; labour, $1.75; glass, $1; iron nut, 5 cents; brackets, 50 cents—total, $3.80. When finished, the instrument should have a coat of shellac-varnish, and, when this is done, the mirrors may be put in place, and fastened on with very heavy bands of rubber. This will enable us to take the glasses

off when the instrument is not in use, and if the elastic bands or rings are very strong, they will answer perfectly. The long mirror is to go on the movable piece at N, and the small mirror stands on the shelf, facing the opening in the board, at O. This mirror stands at the angle shown in the next drawing (Fig. 2), and the other mirror is adjusted to the sun at its various positions in the sky at different seasons of the year.

Here is a diagram showing the position of the handle of the heliostat, and the mirror for different seasons and in different parts of the country. The handle must be placed on a line parallel with the axis of the earth, and the four dotted lines give its position when the heliostat is to be used in Boston, New York, Washington, and New Orleans. This also causes the block of wood marked K to have a slightly different shape, so that the hole through it will be in the middle. The dotted line marked "At Equinox" shows the path of the light from the sun, and

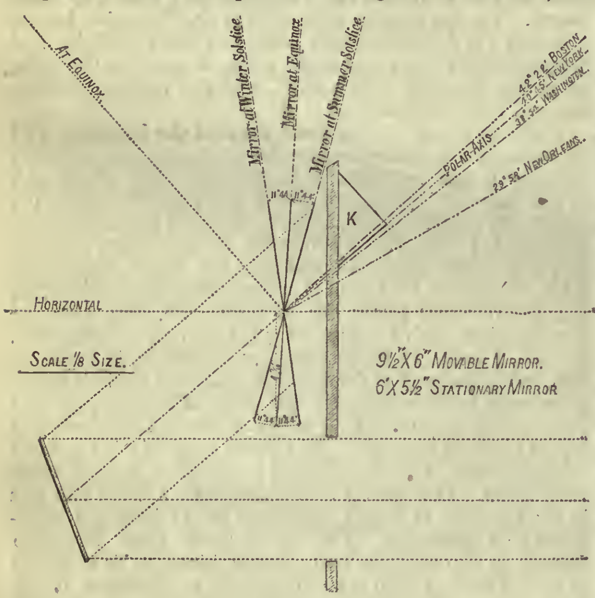


Fig. 2.

the three dotted lines show the paths of the reflected light as it passes from one mirror to the other. The position of the movable mirror is also shown in the positions it has at the summer and winter solstices.

First Experiment with the Heliostat.

Choose a bright sunny day, and take the heliostat into a room having a window facing the south. Raise the sash and place the instrument in the window, and fasten it there so that it will be firm and steady. Before closing the window down upon it, move the larger mirror on its axis till it reflects a beam of light into the small mirror. Then turn the handle to the right or left, and a round, horizontal beam of light will enter the room. When this is done, close all the windows, so as to make the room as dark as possible. To do this, shawls or blankets or enamelled cloth will be found useful inside the curtains and shutters. Then get a piece of cardboard, about 6 inches (15·2 centimetres) square, and lay a five-cent piece in the centre, and, with a knife, cut a hole in the card just the size of the coin. Then fasten this, with pins or tacks, over the opening in the heliostat.

We have now a slender beam of light in a dark room. Walk about and study it from different sides. See how straight this slender bar of light is; it bends to neither the left nor the right, but extends across the room in an absolutely straight line. As the sun moves, turn the handle of the heliostat to keep the light in place.

Here (Fig. 3) is a picture of a dark room, in the window of which is the heliostat. In the centre of the piece of cardboard is the small hole where the light enters the room. A boy is holding one end of a long piece of linen thread just at the bottom of the hole in the card, and another boy has drawn the thread out straight and tight, so that it just touches the beam of light throughout its length.

Were you to try this experiment, you would see that the thread would suddenly be lighted up throughout its whole length, and would shine in the dark room like silver. Then if the boy allows the thread to become slack and loose, or if he lowers it even a very little, it will disappear in the darkness. If he raises and lowers it quickly, it will seem to appear and disappear as if by magic.

This is a very pretty experiment; but we must not stop to look at its merely curious effects. Try it over several times, and see if it does not show you something about the beam of sunlight. Plainly, if the thread is lighted up its whole length when it is straight, then the beam of

Fig. 3.

light must be straight also. Here we discover something about light; we learn that it has a certain property. Our experiment shows that light moves in straight lines.

Experiment with Cards and a Lamp.

Here (Fig. 4) is a picture representing three little wooden blocks placed in a row upon a flat, smooth table, and fastened to them are three postal-cards, so that they will stand upright. At the end of the table is a small lamp. This is all we need to perform another experiment, that will show us the same thing we observed with the beam of light from the heliostat. To make these things, get a piece of wood 10 inches (25·4 centimetres) long, 3 inches (76 millimetres) wide, and 1½ inch (37 millimetres) thick, and saw it into 5 pieces, each 2½ inches (64 millimetres) long. Next make three slips of pine, 4 inches (10 centimetres) long, 3 inches (76 millimetres) wide, and ⅛ inch (4 millimetres) thick. Having made these, get three postal-cards, and lay them flat on a board, one over the other. Just here we need a tool for making small holes and doing other work in these experiments; and we push, with a pair of pliers, a cambric needle into the end of a wooden penholder or other slender stick, putting the eye-end into the wood, and thus making a needle-pointed awl. Measure off one-half inch from one end of the top postal-card, and with the awl punch a hole through them all, just half-way from each side. Lift the cards up, and with a sharp penknife pare off the rough edges of the

holes, and then run the needle through each, so as to make the holes clean and even.

Take one of these cards and one of the wooden slips, and put the card squarely on one of the wooden blocks and place the slip over it, and tack them both down to the block. This will give us the cards and blocks as shown in the picture. When each card is thus fastened to a block, we shall have two blocks left. These we can lay aside, as we shall need them in another experiment.

Now light the lamp, and place one block on the table, quite near the lamp. Look at the lamp carefully, and see that the flame is just on a level with the hole in the card. If it is too high or too low, place some books under it, or put the lamp on a pile of books on a chair near the table. Take a chair and sit at the opposite end of the table, and place another card before you. Now look, through the hole in this card, at the first card before the lamp. If the table is level, you will see a tiny star or point of light shining through the holes in the two cards. Without moving the eye, draw the third card into line between the others, and in a moment you will see the yellow star shining through all three cards.

Next take a piece of thread and stretch it against the sides of the three cards, just as they stand, and immediately you see that they are exactly in line. The holes in the cards we know are at the same distance from the edges of the cards, and our experiment proves that the beam of light that passed through all the holes must be straight,

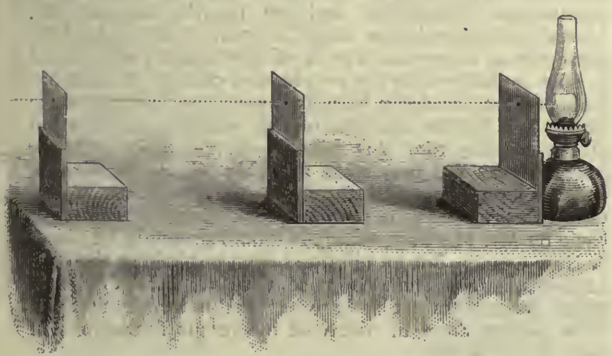


Fig 4.

or we could not have seen it. The cards are in a straight line, and the beam of light must also be straight. This experiment, like the first, shows us that there is a law or rule governing the movement of light, and that law is, that light moves in straight lines.

Move the lamp as near to the edge of the table as possible, and then bring one of the cards close to the lamp chimney. Then change your seat, and repeat this experiment several times in different directions. Each time you will see exactly the same thing, no matter in what direction the light moves from the lamp. The lamp may be moved from one side of the table to the other, and in every direction we shall find the light moving in exactly straight lines from the source of light. This is true whether the source be the sun, a lamp, or a star. One can walk all about the lamp and see it from every side, and we can place our three cards in any direction, north or south, up or down, east or west, or in any and every direction, and every time it will give the same result.

Thus we have found out the law by which light moves, viz., it moves in straight lines in all directions from the source of light.

Knowing this, you can readily think of a number of things in which these laws are made useful. A farmer planting an orchard, an astronomer fixing the positions of stars, a sailor steering his ship by night, employs this law: the first, to arrange his trees in straight lines; the second, to measure out vast angles in the sky; and the third, to lay the courses of his ship in safety. Each employs these laws with certainty and safety, because they are fixed and never change.

OUR ASTRONOMICAL COLUMN

Double Stars.—Vol. xliii. of the *Memoirs of the Royal Astronomical Society* contains two series of micrometrical measures of double stars. The first, by Mr. Knott, includes measures taken near Cuckfield, Sussex, between the years 1860 and 1873, with a refractor by Alvan Clark, having an aperture of $7\frac{1}{3}$ inches, one of the instruments formerly in the possession of the Rev. W. R. Dawes. Measures of most of the well-known binaries will be found in this series, as also of a number of objects not so frequently under observation. Amongst the latter is the suspected variable, U Tauri, which has been observed on several occasions since November, 1863; D'Arrest first pointed out that this star, supposed to be variable by Mr. Baxendell, is really double; it is included in Schönfeld's last catalogue of suspicious objects with the query, "welche Componente veränderlich?" Mr. Knott's observations throw no light on this point, as he appears to have failed to notice any certain traces of change. A note referring to a star near β Leonis deserves attention. Smyth, in his *Cycle of Celestial Objects*, gives a measure, or, as it should perhaps be termed, an estimation of the position of a companion to this bright star, which he calls an eighth magnitude, and *dull red*, position 114°, distance 298″. At the epoch 1864·38 there was no star of such magnitude in this place, but Mr. Knott measured one which by the method of limiting apertures was found to be 11·6m., position 115°·4, distance 303″·5. The inference, especially in presence of Smyth's judgment of the colour of his companion, must be that we have here a new variable star. The *Durchmusterung* has nothing in this position.

The other series of double star measures to which we have referred emanates from the Temple Observatory, Rugby, and forms the second catalogue issued by Mr. Wilson and Mr. Seabroke. The previous catalogue was printed in the preceding volume of the *Memoirs*, and contains some introductory remarks that are wanting in the present one. The selection of objects and the instrumental means appear to be the same; the stars are found either in the Dorpat Catalogue or in the Pulkowa Catalogue of 1850. Amongst them may be noted O.Σ. 298, the first measures of which by the discoverer gave, for 1846·49, position 183°·8, distance 1″·19, while the Temple Observatory measures, 1873·48, assign for the position 232°, with an estimated distance, 0″·45, and the intermediate measures by Baron Dembowski, in 1866, confirm the change in angle and distance. A great change is remarked in Σ 651; at the epoch 1829·67 we have, position 101° 8, distance 10″·82, whereas the Rugby measures give for 1875·18, position 59°·3, distance 16″·26. In this case it is probable that the alteration is caused by proper motion of one of the components: thus the measures may be reconciled, if we suppose an annual motion of the principal star of about 0″·243 in the direction 17°·9. Of 32 Orionis it is remarked "not divided, perhaps binary," and the angle for 1874·1 is 198°·5; between 1830 and 1853 the distance appears to have been about one second without any decided change in the position, which by a mean of Struve, Dawes, and Jacob was 203° 6; the star seems to require further attention. Of 33 Pegasi, another object measured at Rugby, Struve remarks "comes in cœlo prorsus quiescit," or in other words the change in angle and distance noted between his measures in 1829 and 1851, is due to the proper motion of the principal star, which, according to Mädler, amounts to 34″·0 in the century, in the direction 93°·5. Mr. Wilson's measures of O.Σ. 311 confirm the marked diminution of distance

mentioned by Dembowski in *A.N.* 1823—proper motion of one component is no doubt here also the cause of change.

These catalogues of double-star measures made at the Temple Observatory are meritorious productions from an institution not exclusively devoted to a regular course of observations, but also occupied in endeavouring experimentally to interest the youths of the school in astronomical science, with the hope that some, to use the words of the last Annual Report of the Royal Astronomical Society, "may hereafter join that band of amateurs to whom is owing much of what is most characteristic of English astronomy."

While referring to this Report it occurs to us to mention an article by O. Struve on the Baron Dembowski's long series of measures of double stars which is not noticed in the address of the President of the Society, on the occasion of the richly-merited award of the gold medal to the Italian astronomer. It is published in vol. viii. of the *Vierteljahrsschrift der astronomischen Gesellschaft.* After a general outline of the Baron's work, there is given an index to the volumes and numbers of the *Astronomische Nachrichten*, in which his measures have appeared, and which, though not entering into much detail, is useful in their present scattered state. Is it too much to hope that eventually the results of the indefatigable Gallarate observer, may be presented in a collective form, at least as regards their annual means?

SCHMIDT'S LUNAR CHART.—It is understood that this great work, which has been engraved at the expense of the Prussian Government, will, with accompanying letterpress description, be ready for issue in the course of a few weeks. We believe Prof. Auwers, of Berlin, is superintending its publication.

TEMPEL'S COMET OF SHORT PERIOD (1873 II.)—It is probable that the period of revolution of this comet, determined by Mr. W. E. Plummer, from observations extending from July 3 to October 20, will not be found to require very material correction; according to his orbit, the comet cannot attain the distance of Jupiter in its aphelion, and as at the last passage through this point, the planet was distant from it 7·70 (the earth's mean distance = 1) perturbations during the actual revolution are likely to be small. Assuming, then, with Mr. Plummer, that the revolution occupies 1,850 days, the comet may again arrive at perihelion about July 19·5 in the present year. Reducing the perihelion and node to 1878·0, we have the following expressions for the comet's heliocentric co-ordinates referred to the equator:—

$$x = r\,[9{\cdot}99212]\sin(v + 36^\circ\,51'{\cdot}8)$$
$$y = r\,[9{\cdot}98170]\sin(v + 310\;\;7{\cdot}9)$$
$$z = r\,[9{\cdot}53313]\sin(v + 274\;53{\cdot}1)$$

Combining the co-ordinates thus found with the X, Y, Z of the *Nautical Almanac*, and taking July 19·5 for the time of perihelion passage, the following apparent track results:—

12h. G.M.T.

	Right Ascension.	North Polar Distance.	Distance from Earth.
	° ′	° ′	
April 20	281 1	95 56	1·025
May 20	297 53	93 50	0·708
June 19	316 7	95 12	0·488
July 19	334 42	104 56	0·376
August 18	347 22	118 45	0·397

The comet would be nearest to the earth on July 29, and brightest about that date. With such a course it should be well observed. Though, possibly, observations may have been made later than October 20 in 1873, so far as we know none such have been published. Mr. Plummer's elements will be found in the *Monthly Notices* R.A.S. for December, 1873.

[Since the above was written, we learn that Herr Schulhof is engaged upon this comet, with the view to providing an ephemeris for the approaching appearance.]

GEOGRAPHICAL NOTES

AMERICAN LONGITUDES.—The United States Hydrographic Office is continuing the work of establishing secondary meridians of longitude by the electric telegraph. Lieut.-Commander F. M. Green, U.S.N., with the same officers who have been engaged in similar work in the West Indies for some time past, has commenced the determination of South American meridians by measuring from the Royal Observatory at Lisbon through the cables of the Brazilian Submarine Company to Madeira and St. Vincent. The measurement will be continued by way of Pernambuco to Bahia, Rio de Janeiro, Buenos Ayres, and Valparaiso. The longitude of the Royal Observatory at Lisbon will shortly be determined with great exactitude by electrical measurement from London and Paris. The expedition has met with the most gratifying and cordial assistance from the officers of the Portuguese Government and the authorities of the telegraph companies.

NEW GUINEA.—The statement that gold has been discovered in New Guinea by Mr. Goldie, a plant-collector sent out by Messrs. Williams and Co., has caused considerable excitement in the Australian Colonies, but a letter in yesterday's *Times*, from the Rev. W. G. Lawes, who has just returned from a three years' residence on the south-east coast, ought to make would-be gold-hunters cautious. As yet the metal has been found in almost infinitesimal quantities, and we heartily support Mr. Lawes' recommendation that Government ought to take some means to prevent a rush of adventurers who would be sure to demoralise the people, and change to hostility their present decidedly friendly disposition towards white men. It is for the interests of the scientific exploration of the country that this friendly disposition should be maintained. We may state that Mr. C. S. Wilkinson, Government Geological Surveyor of New South Wales, inferred two years ago, from the rock specimens brought back by Mr. Macleay, that gold would probably be found in New Guinea, but he refrained from publishing the fact, he states, fearing it might cause a rush. Mr. Wilkinson states that gold is not likely to be found more plentifully in New Guinea than in the vast auriferous formations of New South Wales.

AFRICAN EXPLORATION.—Dr. Emin Effendi, who in 1876 travelled with Gordon Pasha to the Somerset River, sends from Mruli to Dr. Petermann, a sketch dated November last, of his second journey from Magungo on the Albert Nyanza, across Kirota and Masindi to Mruli in August last, and from Mruli to Mpara-Njamoga, in the south of Masindi, and back to Mruli (in September and October). Sir Samuel Baker, it will be remembered, found Kaba Rega, the lord of Ungoro, utterly intractable; but Dr. Emin Effendi spent a month alone with him, showing the impossibility of anticipating the chances of such travels. In November Dr. Effendi was to go from Mruli to Uganda and Karague, and thence, according to Gordon Pasha's desire, to reach, if possible, Lake Akanyaru, the Mfumbiri Mountains, and Ruanda.

ARCTIC EXPLORATION.—The U.S. Senate has passed the Bill for allowing the *Pandora*, which has been chartered by Mr. James Gordon Bennett for an Arctic Expedition, to sail under the American flag, and for permitting United States naval officers to be detailed for service on board that vessel during the proposed expedition.

PETERMANN'S MITTHEILUNGEN.—As a sequel to a former paper on the distribution of the sedimentary formations of Europe, Petermann's *Mittheilungen* for March contains another on Europe during the two glacial periods, accompanied by a map. The paper on the distribution of palms is concluded, and the first instalment of a summary of exploration of the Ogové given, accompanied by a map.

The April part will contain a long paper, with map, by Prof. Hertzberg, on the Ethnology of the Balkan Peninsula in the fourteenth and fifteenth centuries, and the conclusion of the paper on Prof. Nordenskjöld's proposed expedition from Norway to Behring's Straits. There is also the itinerary (with map) of a journey between Ozaka, Kioto, Nara, and Ominesanjo, in Nippon, Japan, by Dr. Knipping.

AMERICAN GEOGRAPHICAL SOCIETY.—In the *Bulletin* of the Society, No. 5 (1876-7) will be found a pretty full account of the work of the American Palestine Exploration Society, by Dr. Merrell, and a paper on a trip up the Magdalena, and among the Andes, by Mr. J. A. Bennett, U.S. Consul at Bagotá. At the meeting of the society on February 27, the president, Chief-Justice Daly, gave his annual address, summing up in an interesting and complete manner the geographical work of the past year.

BERLIN GEOGRAPHICAL SOCIETY.—The fiftieth anniversary of the foundation of this Society will be celebrated in the Kaisersaal of the Flora. The Crown Prince of Germany, several ministers, and numerous foreign guests, are expected to be present at the festival, which will begin on April 31. The last three numbers for 1877 of the *Verhandlungen* of this Society contain some papers which may interest geographers and ethnologists. Among these (in No. 8) are a paper by Prof. Virchow on "The Anthropology of America," and in the same number a paper on "The Hygiene of the Tropics," by Herr Falkenstein; in No. 10 a paper by Dr. Hildebrandt on his travels in East Africa, in his attempt to reach Mounts Kenia and Kilima-Njaro, to which we have already referred.

SUMATRA.—The Dutch Geographical Society has recently received important news from the Expedition in Sumatra. MM. van Hasselt and Veth report that in the course of their exploration of the southern highlands of Padang, they ascended the Peak of Indrapura, the highest mountain in Sumatra. From the summit of this volcano they had an extensive view over the land and lakes of Korintji. The travellers also report that of late they had met with less enmity on the part of the independent chiefs than at the outset of their expedition.

NOTES

DURING the field operations of one of the parties connected with the U.S. Geological Survey of the Territories, in charge of Prof. F. V. Hayden, portions of south-western Colorado, north-western New Mexico, and north-eastern Arizona, were traversed, embracing that broken-up country occupied in remote times by a race of people who were known as the cliff-dwellers. This subject is well known to readers in general, but we must recur to it again so as to be able to reach the importance of the discovery to be described. In one of the cañons, known as the Chaco, Mr. H. W. Jackson made detailed investigations and measurements of the immense ruined buildings. In one of the arroyas or dry water-courses, the sectional view of the alluvial deposit was exposed to a depth of about sixteen feet. Fourteen feet beneath the surface, a layer of pottery and *débris* came to view. This may not seem strange, as, in a comparatively narrow valley, dirt and gravel to the depth of fourteen feet might be deposited in a short term of years. But ten feet above this layer the foundation walls of ancient buildings were visible, built upon another layer of gravel and sand. These were in time covered with the alluvium upon which now stand the famous ruins, of which no history is extant, and of the builders of which no history will ever be known. How many ages have passed since the lower or first bed was the surface upon which moved the numerous hordes, of which all evidence at present is hidden behind the veil of the dark past? Now, a skull comes to view upon the layer of pottery, which is beneath two eras of occupation and semi-civilisation. This skull, in its contour, is unique. Its closest relations are the ancient Mexicans, Peruvians, Caribs, and Natchez. There is an extraordinary flattening of the upper posterior portion of the head (posterior parietal), which is evident in those figured in Morton's *Crania Americana*. The contents of the skull as found, consists of sand, which is now as hard as ordinary agglutinated sandstone, and has, in nearly all portions, the appearance of liminite. The skull will be described and figured by Dr. W. J. Hoffmann, of the U.S. Survey, and it affords another strong link in the chain of facts and hypotheses of the cliff-dwellers and the ancient Mexicans being more nearly related than is generally admitted or supposed.

MR. PARK HARRISON telegraphs to us from Worthing that he has just (yesterday) exhumed, at Cissbury, a contracted skeleton, sixteen feet deep, lying in the centre of the pit, over which the cist was found last autumn. The work will be continued on Saturday and next week.

A SCRUTINY took place on the 18th instant at the Academy of Sciences for the nomination of a successor to M. Leverrier as member of the section of astronomy. The successful candidate was M. Tisserand, the Director of Toulouse Observatory, who took thirty-two votes out of fifty-five, against M. Wolf. M. Tisserand was the second astronomer of the Japan Mission for the Transit of Venus, which was led by M. Janssen.

As we have already stated, a subscription list has been opened in France for the foundation of a memorial to Claude Bernard. A small sub-committee has been formed to obtain subscriptions in this country, consisting of Sir James Paget, Dr. J. Burdon Sanderson, Prof. Humphry, Dr. Michael Foster, Mr. Ernest Hart, Mr. Romanes, and Prof. Gerald Yeo, King's College, to the latter of whom, as honorary secretary of the Physiological Society, subscriptions may be sent.

PORTER AND COATES of Philadelphia are about to bring out a new and cheap edition of Wilson and Bonaparte's "American Ornithology," three volumes in one, together with 103 new plates.

THE report of Major Feilden, the naturalist of the Arctic Expedition, is now nearly completed, and will shortly make its appearance as a Parliamentary Paper, together with some interesting additional remarks by Sir George Nares.

GENERAL DE NANSOUTY published in the beginning of March a letter stating that a sum of 20,000 francs was required to complete the Pic-du-Midi Observatory, of which he is director. Three days after the publication of his letter in the *XIXme Siècle*, an inhabitant of Calais sent him 5,000 francs, and five days later he was presented with a sum of 15,000 francs by M. Bischofsheim, the eminent Parisian banker, whose generosities to science we have so often to record.

Brownea grandiceps is producing its fine *Rhododendron*-like heads of flowers in No. 1 house at Kew.

KING HUMBERT of Italy has granted four annual prizes of 5,000 lire each (about 190*l.*) for the best productions in art, science, and literature. The Academia dei Lincei, at Rome, is charged with the annual award and distribution of these prizes.

A COMPETITIVE trial of German and Swiss chronometers took place recently at the Deutsche Seewarte at Hamburg, by order of the German Admiralty. The best instrument was furnished by Herr Bröcking, and its performance is said to be superior to that of any chronometer examined at Greenwich during the last three years.

MAJOR-GENERAL SIR HENRY RAWLINSON, K.C.B., F.R.S., and Sir John Lubbock, M.P., F.R.S., have been appointed trustees of the British Museum in the place of the late Right Hon. Sir David Dundas and the late Sir William Stirling Maxwell.

The death is announced of Dr. Joseph Henry Corbett, of Dublin. The deceased was formerly Professor of Anatomy and Physiology, and an Examiner in the Queen's University in Ireland.

We understand that the herbarium of the late eminent botanist, Alexander Braun, has been purchased by the German Government for the sum of 21,000 marks.

The cryptogamic herbarium of the late Italian botanist, G. De Notaris, has been acquired by the Italian Minister of Public Instruction for the Botanic Garden at Rome.

We are happy to state that a decree has established in Lyons, in Bordeaux, and in Besançon observatories for astronomical, meteorological, and horological purposes. For the two former towns, and especially for Lyons, this decree is merely an acknowledgment and regulation of former efforts, but the merit of this measure is not lessened by that consideration, as it puts an end to all local opposition.

Easter being very late this year, the meeting of the delegates of the French learned societies will take place in the last days of April, only three or four days before the opening of the International Exhibition.

At a meeting at the Mansion House last week an influential committee was formed to promote the holding of a great agricultural exhibition in London next year, under the auspices of the Royal Agricultural Society of England. Hyde Park was proposed as the place for holding the show.

A shock of earthquake is reported to have been felt at Debenham, a few miles from Ipswich, on Saturday morning.

Though the cultivation in India of the best quinine-yielding species of Cinchona (*C. officinalis*) has not proved a success, it is satisfactory to know that one species at least thrives most abundantly in the Sikkim plantations. From a paper read at the last meeting of the Pharmaceutical Society by Mr. Wood, the Government Quinologist in India, it seems that out of a total of about three million trees, comprising four or five species of Cinchona it is estimated that there are as many as 2,500,000 belonging to the species *succirubra*. It is from this bark that the now well-known "Cinchona febrifuge" is prepared. This substance, according to many well known medical practitioners in India, possesses to so very nearly the same extent the anti-periodic properties of quinine that it may be safely substituted for the latter in the treatment of ordinary fevers and ague. 5,000 lbs. of this febrifuge, we are told, has already been made and issued, and it is now being made at the rate of 4,000 lbs. a year; the demand, however, is so rapidly overtaking this scale of production that a further extension will shortly be necessary. For use it appears in the form of a fine white powder, which, however, becomes in a short time of a pale buff tint. It does not agglutinate even in the Indian climate. It is freely soluble in weak acids and is readily taken up by lemon-juice, which constitutes a pleasant vehicle for its administration.

The Pharmaceutical Society of Great Britain has just issued an excellent catalogue of the fine collections of Materia Medica and chemical products in their museum in Bloomsbury Square. The catalogue is the work of the Society's Curator, Mr. E. M. Holmes, F.L.S., and includes a great deal of information regarding the several products mentioned. The alphabetical classification of the plants according to their genera in each order and the numerous references to figures in English, American, and foreign works will make this book valuable not only to students of the collection it illustrates, but also for handy reference on the subject generally.

Those who are interested in the subject of railway brakes will obtain much instruction and pleasure by a visit to the offices of the Westinghouse Brake Company, at St. Stephen's Palace Chambers, Westminster, where the Company's Automatic Brake may be seen at work. By an ingenious arrangement the brake-power sufficient for a train of ten carriages is represented. At one view the whole of the apparatus that would be brought into play to bring such a train to a stop is seen. A steam-engine compresses the air and distributes it through all the tubes and the ten reservoirs extending over the whole length of the train, and which, by simply turning a handle, acts upon the brakes, one of which is ready to clasp each wheel of the train. The brake can be applied by engine-driver or guard in little more than five seconds, and its action is so powerful that a train going at forty miles an hour can be brought to a dead stop in something like fifteen seconds and within a distance of about 500 yards. The essential principle of this system is the admission of compressed air into a cylinder attached underneath a carriage, and containing the ends of two pistons acting by leverage upon the brakes; the compressed air is stored in pipes attached to the cylinder, and is thus ready for instantaneous admission, which is effected by producing a reduction of pressure, and thus opening a set of valves that admit the air into the cylinder. The air thus admitted acts upon the pistons by pushing them out and causing the brakes to clasp the wheels and instantly stop their revolution. The distinctive feature of the automatic brake is that in case of the train breaking into one or more parts or in case of its meeting with any obstruction or leaving the rails, the brakes are at once applied automatically, and thus the risk of disaster is immensely diminished. Our examination of the apparatus has convinced us of its perfect efficiency, which we find is testified to by all the railway companies that have used it; and any one who has recently travelled north by the Midland Railway must admit that it would be difficult to improve upon a system that can bring a long train going at full speed to a stop within a few seconds. The brake can be applied with any strength, and thus is of great service in going down inclines and taking sharp curves. On the apparatus at St. Stephen's Chambers is a nozzle from which the compressed air may be allowed to escape, and with which some curious phenomena with a hollow elastic ball are shown. The ball is placed within the current of escaping air, and if the tap is kept upright the ball is sustained as if by a jet of water, but with little or no revolving motion. If the tap be brought to an angle of say thirty or forty degrees from the perpendicular, the ball is still sustained by the current, receding and advancing in the line of the tap and revolving rapidly outwards in the direction of the current, so rapidly as to produce a most marked flattening at the poles or sides at right-angles to the direction of motion. Ultimately it becomes almost a disc. Gradually the axis of rotation changes till it is at right-angles to its original position, when the speed of rotation diminishes and the ball gradually comes to rest. Again it begins to spin upon its new axis, going through the same changes again and again so long as it is kept within the action of the jet. In concluson we may say the brakes are comparatively simple in construction; it is almost impossible to put them out of order, and they may be effectually handled by ordinary railway officials.

The method of coincidences has recently been applied by M. Szathmari, to determine the velocity of sound in free air, as follows:—A pendulum, whose rate was accurately known, closed, at each passage through the vertical position, a battery circuit, the line of which was 220 m. long, and included two electric bells. When both bells are placed before the observer, he hears them simultaneously. If one be moved a little way off this simultaneity ceases; and if the bell be moved still further a point is reached, at which both bells are heard simultaneously again. The distance is that through which the sound moves in the interval between two successive ringings of the bells. The pendulum, in the present case, had a period of 0·2961 seconds; the distances at which the sounds of the two bells were heard at

once were directly measured, and the average value (from thirty measurements) was 99·25 m. From this the velocity of sound in free air = 335·19 m. Reducing the value to that for dry air at zero the number obtained is 331·57 m. This lies about midway between Regnault's value (330·7) and that of Moll and Van Beck (332·26).

AT a recent meeting of the Berlin Geographical Society, Prof. Karsten, of Kiel, read an interesting account of the activity of the Commission established in Schleswig-Holstein, which has for its object the exact and minute investigation of the climatological, physical, and chemical conditions of the Baltic and the German Ocean, as well as of the influence which these conditions exercise upon organic life. The commission has established a large number of stations for making observations of the currents existing in these seas, in order to obtain data for the understanding of the general laws governing marine currents. With regard to animal life, the commission has up to the present confined its labours to the most important inhabitant of the two seas, the common herring, and it has succeeded in determining with certainty the few zoological varieties of this fish, as well as in finding its spawning places, and as a result, the artificial cultivation of herrings has already been set on foot. The commission will now devote its attention to other species of fish.

A GERMAN Viticultural Society has just been formed at Cassel. For the present the Society intends to take up two important matters, viz., (1) discovering the best method for the destruction of phylloxera, and (2) the suppression of the secret manufacture of wines by artificial means.

IN NATURE (vol. xvii. p. 372) an account is given of the difficulty met with in Australia in getting bees to work after a few years. A correspondent calls attention to the fact that a similar difficulty occurred in California, where it has been obviated by a systematic abstraction of the honey as the bees collected it. If this were tried in Australia it might possibly meet the difficulty.

IN a recent communication to the Belgian Academy on digestion in insects, M. Plateau, after a careful examination of forty individuals of various types retires from his former position that the digestive juices (in the normal state) are *never acid*. In insects which feed wholly or partly on animal matters, they are slightly acid. He will not, however, concede a constant acidity for all insects (which some naturalists affirm); and in reply to the objection based on the characteristic acidity of the gastric juice of vertebrates, he contends that the digestive liquid in articulata, insecta, myriapoda, arachnida, and crustacea is not analogous to that juice, but rather to the pancreatic juice; the acidity is an accessory character and not the sign of a physiological property. The ferment present is evidently something quite different from the gastric pepsine of vertebrates. Thus, a very little hydrochloric acid, so far from promoting its action, retards or arrests it.

A NEW method, said to be more accurate in its results than that of Helmholtz, for determining the tones of the mouth-cavity which correspond to the vowels, is recommended by M. Auerbach in a recent number of the *Annalen der Physik*. It is based on percussion. Having made a long inspiration, you bring the mouth into the position corresponding to the particular vowel, and then strike the larynx after the manner of physicians, *i.e.*, place the middle finger of one hand firmly on it, and strike it with that of the other hand. A comparatively distinct tone is then heard, which varies with the position of the mouth, but for a given position is always the same. The effects are perceived more distinctly if the ears are previously stopped with wax. M. Auerbach describes results of observation by this method.

MR. A. W. BENNETT (Lecturer on Botany, St. Thomas's Hospital, London, S.E.) requests us to state that he is engaged on an introductory handbook of Cryptogamic Botany, to be published in the International Scientific Series, and that he will be extremely glad of any recent original memoirs, English or Foreign, bearing on any branch of the subject which the authors may incline to send him.

AN International Congress of Botany and Horticulture will be held in Paris on August 16 and following days, under the auspices of the Botanical Society and the Central Horticultural Society of France, in the rooms of the latter Society, 84, rue de Grenelle. A programme of subjects, botanical and horticultural, is announced, on which papers are especially invited, as well as the exhibition of illustrative specimens, collections, and apparatus. One of these subjects is the establishing and fitting up of botanical laboratories. The attendance and co-operation of foreign botanists are cordially invited.

IN the year 1877 no less than 8,000 new publications appeared in Italy. Amongst these there were 5,743 new books (1876 : 4,323), 1,880 pamphlets (1876 : 1,524), and 194 new journals (1876 : 256).

THE additions to the Zoological Society's Gardens during the past week include two Common Marmosets (*Hapale jacchus*) from South-East Brazil, presented by Mr. R. Donaldson; a Three-striped Paradoxure (*Paradoxurus trivirgatus*) from India, presented by Capt. Dalrymple; a Secretary Vulture (*Serpentarius reptilivorus*) from South Africa, presented by Messrs. W. Rigg and J. Curtis; a Green Glossy Starling (*Lamprocolius chalybeus*) from North-East Africa, a White-eared Bulbul (*Pycnonotus leucotis*) from India, a Californian Quail (*Callipepla californica*) from California, presented by Mrs. Arabin, F.Z.S.; a Common Kestrel (*Tinnunculus alaudarius*), European, presented by Mr. A. Blumenthal; a Lion (*Felis leo*) from Africa, a Variegated Sheldrake (*Tadorna variegata*) from New Zealand, received in exchange; two Common Swans (*Cygnus olor*), European, deposited; three Black Swans (*Cygnus atratus*), bred in the Gardens; a Zebu (*Bos indicus*), two Common Badgers (*Meles taxus*), born in the Gardens.

THE ANALOGIES OF PLANT AND ANIMAL LIFE[1]

II.

WE may find a kind of analogy for these cases of contradictory action—for they really strike one as contradictory.

The chameleon and the frog are both affected in a peculiar manner by light; they both change colour in accordance with variations in the intensity of the light. Moreover, the change of colour is produced by the same mechanism in the two cases; by a kind of contraction and expansion of certain coloured cells in their skin. But the curious fact is that chameleons[2] become darker in sunshine, while frogs[3] become pale in sunshine and darker in darkness. No doubt both these changes are in some way serviceable to the frog and the chameleon, and we may suppose that the whole phenomenon is really analogous to the opposite effects of light which occur in plants.

To quit the paths of science for those of another region of "Wonderland," it has been pointed out by Mr. Lewis Carrol that dogs wag their tails when they are pleased, whereas cats do so when angry. Seriously the principle is the same—given that emotion produces disturbance of the tail, it will depend on the surrounding circumstances in which the creatures live as to whether a given emotion shall produce a wagging or a rigid tail.

Let us once more consider what needs will arise in the life of an animal, and then see how the same needs are supplied by plants. An animal needs to be alert to changes going on in the world around it; it needs delicate sense-organs to perceive the approach of enemies or the whereabouts of its food. In fact it is evident that to prosper in the varying conditions of life an animal must be sensitive to these changes. By sensitiveness one

[1] A Lecture delivered at the London Institution on March 11 by Francis Darwin, M.B. Continued from p. 391.

[2] Brücke, *Wien. Denkschrift*, 1851; v. Bedriaga, "Die Entstehung der Farben bei den Eidechsen," 1874.

[3] Lister, Cutaneous Pigmentary System of the Frog. (*Phil. Trans.*, 1858; v. Wittich, Müller's *Archiv*, 1854.

means that an animal must be capable of being affected by changes which, considered as mere physical agents, are insignificant. A fly living in the same room with an active-minded boy will depend for its safety on its power of rapidly appreciating the approaching shadow of the boy's hand. Now the changes produced in the arrangement of forces in the universe are not perceptibly affected by this shadow—it is utterly insignificant—yet what a violent effect it has on the fly. It is because the nervous system of the fly possesses the property of magnifying external changes so that apparently slight disturbance causes large results.

This power of being strongly affected by apparently slight changes is a very important character of living matter. The processes which occur within the fly have been likened to the explosion of a pistol, the force used in moving the trigger being utterly insignificant when compared with the result produced. I do not mean that this exploding power is a distinguishing mark of living matter, but it certainly is a well marked feature. Besides the power of magnifying or intensifying external changes, which we have described as the exploding power of irritable tissue, there is another, the power possessed by nerves of transmitting a stimulus wave from one part to another. We will first look for this transmitting power as it exists in plants.

The leaf of the sundew, or Drosera, consists of a shallow, slightly saucer-shaped disc covered over with short glands, and fringed all round with projecting tentacles which also terminate in glands. The glands secrete a sticky fluid, which hangs in drops on them, hence the name of sundew, because the leaves seem to be covered with dew in sunshine, when other plants are dry. Insects are caught by the sticky secretion, and are also embraced and held fast by the outer tentacles, which possess the power of moving. When the insect has been killed by being drowned in the sticky secretion, it is digested by the acid juice poured out by the glands and subsequently absorbed.

The external or movable tentacles may be made to bend inwards, either by insects alighting on the centre of the disc of the leaf, or on the sticky glands of the tentacles themselves. In the first case, when an insect is caught on the middle of the leaf, and the external tentacles bend in and surround it, we have a true transmission of stimulus, a message sent, like a message is sent along a nerve. The insect may be struggling to free itself, and will probably succeed in doing so, unless the external tentacles give their help. The external tentacles can be made to bend not only by insects or other objects placed on the centre of the leaf, but also by anything placed on the gland at the end of the tentacle itself. In this case the meaning of the movement is equally obvious. If a gnat or fly lights on one of the external glands, it will probably escape, unless carried to the centre of the leaf, where it will be also held by the small sticky glands. Here also there is a true transmission of stimulus. The message has to be sent from the gland at the top to the place where the tentacle bends; a message is sent from the gland to the bending part of the tentacle, just as a message goes through nerve tissue from our skins to our muscle.

In this case the tentacle always carries the fly it has caught to the actual centre of the leaf. But if a fly has been caught by the disc of the leaf, and not qulte in the centre, then the messages are sent in accordance with the position of the fly, and all those tentacles within reach move to the point of irritation with marvellous precision. This transmission of messages is all the more wonderful, because, as far as our powers of observation go, there is no special structure to convey the stimulus. It is true that waves of stimulation do travel with special facility along the fibro-vascular bundles, or what are usually called the veins of the leaf. But in this case, where tentacles converge to a given point in the disc of the leaf, this mode of transmission is impossible, because the veins are few in number, and could not cause so nice an adaptation of movements. Moreover, stimuli can travel across a leaf of Drosera after the vascular bundles have been cut through.[1] So that we have the wonderful fact of a wave of stimulation travelling with great accuracy transversely through a number of cells with absolutely no structure like nerve-fibre to guide the course in which the stimulus-wave shall flow.

One other curious phenomenon may be alluded to as showing the extraordinary power of stimulus-transmission. If a piece of meat is placed on an external tentacle, the gland on which it rests sends forth an acid secretion; and if a piece of meat is placed on the centre of the leaf, the tentacles, as before said, bend in and ultimately touch it; but if the external glands are tested with litmus paper before they reach the meat in the centre, they will be found to be covered with acid secretion, proving that not only had a message been sent to the moving part of the tentacle, but also to the secreting cells in the gland.

[1] See Batalin, "Flora," 1877, who has correctly pointed out the importance of the fibro-vascular bundles as conveying stimulus-waves.

One might find a parallel to this in the action of the human salivary glands. The gland nerves may be excited either by the stimulus of food placed in the mouth, or by the voluntary action of the muscles of mastication. Here the saliva is poured out, although there is no food to act on, just as the Drosera-gland secretes during the movement of the tentacle before there is anything for its secretion to digest.

Having briefly considered the transmission of stimulus-waves as shown in Drosera I will pass on to consider what manifestations may be found of the other general property of nerve tissue, the property which I have called exploding power. It is chiefly manifested in Drosera by the extreme sensitiveness of the glands on the external tentacles. It is found not to be necessary to place meat or insects on the gland, but that bits of glass, wood, paper, or anything will excite them. Smaller and smaller atoms were tried and still the glands were found to be sensitive to their presence.[1] At last a minute piece of a human hair, about one-hundredth of an inch in length, and weighing just over $\frac{1}{80000}$ of a grain, was placed on the gland of a tentacle and it caused unmistakable movement. The case is yet more wonderful than it sounds, because the piece of hair must be partly supported by the thick drop of secretion on the gland, so that it is probably no exaggeration to say that the gland can perceive a weight of one-millionth of a grain. This degree of sensitiveness is truly astonishing, it seems to us more like the sense of smell than that of touch, for to our most delicate tactile organ, the tongue, such atoms are quite imperceptible.

The power which Drosera has of perceiving the presence of ammonia is perhaps still more astonishing. A solution of phosphate of ammonia in pure distilled water in the proportion of one part to over two million of water, caused inflection of tentacles.[2] One may form an idea of this result by making a solution of a single grain of the phosphate and thirty gallons of distilled water, and then finding out that it is not pure water. Considering the water-supply which we at present enjoy, we may well be grateful that our senses are duller than those of a sundew.

As examples of simple sensitiveness these facts are sufficiently striking, but the powers of discriminating between different kinds of stimuli are equally curious. The tentacles having proved so extraordinarily sensitive to light bodies resting on them, one would expect that the slightest touch would make them bend. But it is not so; a single rapid touch, though it may be violent enough to bend the whole tentacle, does not cause inflection. The meaning of this is clear, for in windy weather the glands must be often touched by waving blades of grass, and it would be a useless labour to the plant if it had to bend and unbend its tentacles every time it was touched. It is not excited except by prolonged pressures or quickly repeated touches. This is also quite intelligible, for when an insect is caught on the sticky secretion of the gland it will give a somewhat prolonged pressure, or a number of kicks to the sensitive gland, unless indeed it flies away after a single struggle, and in that case the tentacle will be also saved from uselessly bending.

In another carnivorous plant, Dionæa, the specialisation of sensitiveness is exactly the reverse; thick and comparatively heavy bits of hair can be cautiously placed on the sensitive organs without causing any movement, but the delicate blow received from a cotton thread swinging against the hair causes the leaf to close.[3] Dionæa catches its prey by snapping on it like a rat-trap—there is no sticky secretion to retain the insect as in Drosera till the slowly moving tentacles can close on it. Its only chance of catching an insect is to close instantly on the slightest touch. The specialisation of sensitiveness in Dionæa is therefore just what it requires to perfect its method of capture.

In describing the sensitiveness of Drosera and Dionæa I wish rather to insist on a wide and general similarity to the action of nerves. There may be said to be an analogy between the specialisation of extreme sensitiveness in Drosera and Dionæa and the nervous tissues of animals, because these properties play the same part in the economy of the plant that is supplied through some kind of nerve machinery in the higher animals. Closer analogies could be pointed out. There are, for instance, the

[1] "Insectivorous Plants," p. 32.
[2] "Insectivorous Plants," p. 170.
[3] "Insectivorous Plants," p. 289.

well-known researches of Dr. Burdon Sanderson, in which he compares the electrical disturbances which occur in the leaf of Dionæa to those which take place in nerve and muscle. Again Mr. Romanes has, in a recent lecture in this place, compared the peculiar sensitiveness of Drosera to repeated touches with the phenomenon known in animal physiology as the summation of stimuli. But I have merely sought to show that we find in Drosera a power of conduction of stimuli, an extreme sensitiveness to minute disturbances, and a power of discriminating between different kinds of stimuli which we are accustomed to associate with nervous action. To establish this analogy I believe that the examples already mentioned may suffice.

We will now inquire whether among plants anything similar to memory or habit, as it exists among animals, may be found.

The most fruitful ground for this inquiry will be the phenomenon known as the sleep of plants. The sleep of plants consists in the leaves taking up one position by day and another at night; the two positions for night and day following each other alternately. The common sensitive plant (Mimosa) is a good example of a sleeping plant. The leaf consists of a main stalk from which two or more secondary stalks branch off; and on these secondary stalks are borne a series of leaflets growing in pairs. The most marked character of the night or sleeping position is that these leaflets, instead of being spread out flat as they are in the day, rise up and meet together, touching each other by their upper surfaces. At the same time the secondary stalks approach each other and ultimately bring the rows of closed-up leaflets (two rows on each stalk) into contact. Besides this well-marked change the main stalk alters its position. In the afternoon it sinks rapidly, and in the evening it begins to rise, and goes on rising all night, and does not begin to sink until daylight. From that time it sinks again till evening, when it again rises, and so on for every day and night. In reality the movement is more complicated, but the essential features are as I have described them.

In comparing the sleep of plants with anything that occurs in animal physiology, we must first give up the idea of there being any resemblance between this phenomenon and the sleep of animals. In animals, sleep is not necessarily connected with the alternation of light and darkness, with day and night. We can imagine an animal which by always keeping its nutrition at an equal level with its waste would require no period of rest. The heart which beats day and night shows us that continuous work may go on side by side with continuous nutrition.[1] Mr. Herbert Spencer has suggested that since most animals are unable to lead a life of even ordinary activity during the night because of the darkness, therefore it answers best to lead an extremely active life in day when they can see, and recover the waste of tissue by complete rest at night. On the other hand, certain animals find it more to their profit to sleep in the day and rest at night. But there is nothing of this kind in plants; their sleep movements are not connected with resting. Although the leaflets close up, yet the main stalk is at work all the night through.[2] Moreover, owing to the closing up of the secondary stalks of the leaf, the length of the whole organ is increased, and therefore the work done by the main stalk is also increased. So that, far from resting at night, the main stalk is actually doing more work than in the day. Besides this, instead of being more or less insensible, as a sleeping animal is, the primary petiole of the Mimosa remains fully sensitive at night, and displays the same property which it shows by day, viz., that of falling suddenly through a large angle on its irritable joint being touched. Besides these points of difference, there is the important distinction that the movements of sleeping plants are strictly governed by light and darkness without any reference to other circumstances.

In Norway,[3] in the region of continual day, the sensitive plant remains continually in the daylight position—although no animals probably remain continually awake.

There is one—but only a fanciful resemblance—between the sleeping plants and animals, namely, that both have the power of dreaming. I have been sitting quietly in the hot-house at night waiting to make an observation at a given hour, when suddenly the leaf of a sensitive plant has been seen to drop rapidly to its fullest extent and slowly rise to its old position. Now in this action the plant is behaving exactly as if it had been touched on its sensitive joint; thus some internal process produces the same impression on the plant as a real external stimulus. In the same

[1] Leaving out of the question the repose during diastole.
[2] In Mimosa at least.
[3] Schübler, quoted by Pfeffer ("Die periodische Bewegungen der Blattorgane," 1875, p. 36).

way a dog dreaming by the fire will yelp and move his legs as if he were hunting a real instead of an imaginary rabbit.[1]

I said that in the regions of perpetual light the sensitive plant remains constantly in the day position. We might fairly expect, therefore, that we should be able to produce the same effect by artificial light constantly maintained. This experiment has, in fact, been made by A. de Candolle,[2] Pfeffer, and others with perfect success. But before the leaves come to rest a remarkable thing takes place. In spite of the continuous illumination, the sleeping movements are executed for a few days exactly as if the plant were still exposed to the alternation of day and night. The plant wakes in the morning at the right time and goes to sleep in the evening; the only difference between these movements and those of a plant under ordinary circumstances is that under constant illumination the movements become gradually smaller and smaller, until at last they cease altogether. When the plant has been brought to rest it can be made to sleep and wake by artificial alternations of darkness and light. This fact seems to me extremely remarkable, and one which, in the domain of animal physiology, can only be paralleled by facts connected with habit. The following case is given me by a friend and is probably a common experience with many people:—Having to be at work at a certain time every day, he has to get up at an early hour, and wakes with great regularity at the proper time. When he goes away for his holiday he continues for a time waking at the proper hour to go to work, but at last the body breaks through the habit, and learns to accommodate itself to holiday hours.

It seems to me that this case may fairly be likened to that of the sensitive plant in constant illumination. There is the same continuance of the periodic movement on the first removal of a stimulus, and the same gradual loss of periodicity consequent on the *continued* absence of the stimulus.

From this kind of habitual action there is but a small step to those actions in which we say that memory comes into play. Dr. Carpenter[3] relates the case of a boy who, in consequence of an injury to his brain, never acquired the power of speech or of recognising in any way the minds of other people. In spite of this mental incapacity he had an extraordinary sense of order or regularity. Thus although he disliked personal interference, his hair having been one day cut at ten minutes past eleven, the next day and every following day he presented himself at ten minutes past eleven, as if by fate, and brought comb, towel, and scissors, and it was necessary to cut a snip of hair before he would be satisfied. Yet he had no knowledge whatever of clocks or watches, and was no less minutely punctual when placed beyond the reach of these aids.

It is hard to say whether this boy actually remembered at ten minutes past eleven that now was the time to have his hair cut, or whether it was an unconscious impulse that made him do so. But whether we call it habit or memory, there is the same knowledge of the lapse of time, the internal chronometry, as Dr. Carpenter calls it, which exists in the sensitive plant, and the same tendency to perform an action because it has been done previously. There is, in fact, hardly any distinction between habit and memory; if a man neglects to wind up his watch at night, he says that he forgot it, and this implies that memory normally impels him to wind it; but how little memory has to do with the process is proved by the fact that we have often to examine our watches again to see that they are wound up. It is the old problem of conscious and unconscious action. If a friend, in order to test our powers of self-control,[4] moves his hand rapidly near the face, we cannot help winking, though we know he will not hurt us; and when we are breaking through a hedge or thicket, we close our eyes voluntarily to keep twigs out. Here are two actions performed with the same object by the same muscles under command of the same nerves, yet one is said to be directed by the will and the other by instinct, and a great distinction is drawn between them. It seems to me that the presence of what Mr. Lewes calls "thought consciousness" is not the crucial point, and that if it is allowed that the sensitive plant is subject to habit (and this cannot be denied), it must, in fact, possess the germ of what, as it occurs in man, forms the groundwork of all mental physiology.

I am far from wishing to make a paradoxical or exaggerated statement of this resemblance between the periodic movements of plants and memory of the human mind. But the groundwork

[1] This curious phenomenon was first observed by Millardet, who describes it as of rare occurrence. (Millardet, *loc. cit.*, p. 29.)
[2] Quoted by Pfeffer ("Periodische Bewegungen," p. 31).
[3] "Mental Physiology," p. 349.
[4] See "Physiology of Common Life," vol. ii. p. 200.

of both phenomena seems to be the repetition of a series of acts, or the recalling of a series of impressions, in a certain order at a certain time, because they have been repeated in that order and at that time on many previous occasions.

I will mention one more fact in connection with the movements of Mimosa, in which the formation of habit is illustrated. Every one knows that a noise regularly repeated ceases to disturb us; that one becomes habituated to it, and almost ceases to hear it. A boy fast asleep inside an iron boiler while riveting is going on, is an example of this power of habituation. The same thing occurs with the Sensitive Plant. A single violent shake causes the main stalk to drop, and the leaflets to shut up; in a minute or two the leaf recovers, and will again react on being disturbed. In order to test the power of habituation, I fastened one end of a thread to the leaf of a sensitive plant, and the other to the pendulum of a metronome, and placed the plant just at such a distance from the instrument that it received a pull at every beat. The first shock caused the leaf to shut up, but after a few repetitions it became accustomed to it, and I had the curious sight of a highly-sensitive plant unaffected by a series of blows. In nature this power no doubt enables the plant to withstand the constant shaking of the wind.

In spite of the amount of time which has been spent on the study of sensitive and sleeping plants, no satisfactory explanation of the use which the movements are to the plant has ever been given. In the case of the carnivorous plants, we saw that the movements of plants may be offensive, and like the movements of animals in securing its prey. In the case of certain flowers which we will now consider, the movements are *de*fensive, like the closing of a sea anemone. I shall describe these movements with a view to showing the existence of periodicity or habit, and some other general resemblances to animal physiology.

The crocus is perhaps the best example of a flower which opens and shuts in accordance with changes of external circumstances. The crocus is especially sensitive to changes of temperature. If a light index is fastened into one of the petals or divisions of the flower, very small movements are made visible, and in this way it has been shown that the crocus actually appreciates a difference of temperature of one degree Fahrenheit.[1] I have seen a crocus distinctly open when a hot coal was brought near it. The use of this power of movement is connected with the fertilisation of the flower. In the warm sunshine the flower opens wide, and the bees are soon hard at work, and carry pollen from one flower to another. If, now, a cloud hides the sun, the temperature falls, and the crocus begins to close, and by the time the sky has become overcast and the first drops of rain fall, the precious pollen is housed safe beneath the roof of petals. The crocus is warned of the coming danger by the shadow of the cloud just as the fly is warned by the shadow of the approaching hand. The crocus is sensitive to changes of light and darkness as well as to changes of temperature, and the sum of these influences alternately acting by night and day produce a periodic opening and shutting which resembles the periodic movement or sleep of the Sensitive Plant. Corresponding to the regular repetition of the stimulus of light and heat, an internal periodicity has arisen in the flower which shows itself in a curious manner. This phenomenon is best shown by certain flowers which are not so sensitive to temporary changes, but which open and close regularly by day and night. Raising the temperature in the evening does not produce nearly the same amount of divergence of the petals as a similar rise in the morning. With the white waterlily, *Oxalis rosea*, and some other flowers, the same thing is well seen.[2] If the flowers have been allowed to close at the natural hour in the evening it is hardly possible to perceive the least opening of the petals even when the temperature is raised from 50° to 82°. On the other hand a considerable lowering of temperature does not produce so much effect in the morning as it does towards evening. In all biological problems it is necessary to consider the internal condition of the organism quite as much as the other element, viz., the external condition. It is a familiar fact that similar external causes do not produce like results. A man may fall ill after exposure to wet and cold at different times of his life and the kind of illness may be very different. Once it may be rheumatic fever, another time pleurisy, or some other malady, so that in the case of the flowers which, under a given change of temperature, behave differently at different times of day, we see the variability in the internal condition or receptive state of the organism exemplified, the most interesting fact being that the receptiveness varies not capricously but with periodicity.

[1] Pfeffer, "Physiologische Unters.," 1873, p. 183.
[2] Pfeffer, "Physiologische Unters.," p. 195.

The same phenomenon may also be seen when the cycle is a yearly and not a daily one. A German physiologist has lately made a long and patient research on the yearly periodicity in the growth of buds.[1] The method consisted in ascertaining the weight of 100 cherry buds gathered at frequently repeated intervals throughout the year. In order to discover whether the growth of buds would be equally increased in rapidity at all times by a given increase of temperature, branches were cut and kept in a greenhouse at a temperature of 60 to 70 at various times of the year. This experiment showed that branches thus treated in the beginning of December were hardly at all hurried on in growth, while the rise of temperature at once produced energetic growth in buds in the middle of January. If this fact is to be classed with the very similar effects of temperature on the daily periodic changes in flowers—and I can hardly doubt that it ought to be so classed—a difficulty arises. The buds being new growths, have never experienced a previous winter or spring, so that the periodicity cannot originate in their tissues; it must, therefore, depend on some property common to all the branches, some periodicity common to the nutrition of the tree. Askenasy describes the case as the occurrence of some chemical change which goes on in the buds, rendering them sensitive to rise of temperature at a certain period. The case bears a resemblance to the hybernation of animals. Thus, Berthold[2] says that when the dormouse, *Myoxus avellanarius* first goes to sleep in the autumn, it can be partly awakened, and then sent into deep sleep by alternations of temperature, answering, like the crocus, to alternations of heat and cold; but when the winter sleep has fairly set in, no effect could be produced by raising the temperature,—just as the oxalis and water lily when once shut for the night could not be made to open.

I have no doubt that many closer analogies will some day be shown to exist between the behaviour of plants and animals, as regards nerve-physiology. The after-effect of stimuli seems to be represented in the movements of plants. If a stimulus is suddenly applied and then removed, the nerves acted on do not cease to be disturbed the instant the stimulus ceases. The molecular change, whatever it is, which goes on in the nerve, cannot leave off directly the stimulus ceases. The molecular action goes on like the vibration of a bell after it has been struck. When a wheel is turned round rapidly before our eyes the image of a new spoke strikes the retina before the image of the old one has died away, so that we cannot distinguish one from another. In the same way a burning stick whirled round looks like a circle of fire. This after effect of stimuli is represented in plants by heliotropism and geotropism. I have myself observed it in the latter. I took a young growing shoot and put it through a hole in a cork, so that it was firmly fixed into a bottle of water. I then put the bottle on its side in a vessel filled with wet sand, and fixed it firmly by piling wet sand over it. The shoot thus projected horizontally from the vessel of sand. It now began to straighten itself by geotropism, that is to say, the tip of the shoot began to curve upwards. I applied a delicate means of measuring this upward movement, and allowed it to continue for some time. I then turned the bottle round on its axis, so as to rest on what had been its upper surface, and the action of gravity being now reversed as far as the shoot went, the tip ought to have reversed its direction of growth, and curved upwards, but instead of this it went on curving towards the earth in consequence of the after-effect of the old stimulus. And it was more than an hour before it could reverse its movement, and again grow upwards.

With this case I conclude my comparison of plants and animals. Some of the points of resemblance which I have attempted to point out are purely analogical. Nevertheless, I have tried to show that a true relationship exists between the physiology of the two kingdoms. Until a man begins to work at plants, he is apt to grant to them the word "alive" in rather a meagre sense. But the more he works, the more vivid does the sense of their vitality become. The plant physiologist has much to learn from the worker who confines himself to animals. Possibly, however, the process may be partly reversed—it may be that from the study of plant-physiology we can learn something about the machinery of our own lives.

[1] Askenasy, *Bot. Zeitung*, 1877, No. 50, 51, 52; abstract *Naturforscher*, 1878, p. 44.
[2] Berthold, *Müller's Archiv*, 1837, p. 63.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—The Vice-Chancellor has published, for the information of the Senate, a statement received from the University Commission. There appears to the Commissioners to be sufficient evidence of needs which will ultimately require a contribution equivalent to, at least, ten per cent. of the net income of the Colleges. The Commissioners think it will be sufficient to specify in general among the purposes for which provision should be made :—

"1. Additional buildings for museums, laboratories, libraries, lecture-rooms, and other rooms for University business.

"2. The maintenance and furnishing of such buildings, including the provision of instruments and apparatus, together with the employment of curators, assistants, skilled workmen, and servants.

"3. Additional teaching power by the institution of new permanent or temporary professorships, and the employment of lecturers and readers, including the increase of the stipends of some of the existing professorships and the provision of retiring pensions.

"4. Grants for special work in the way of research, or for investigations conducted in any branch of learning or science connected with the sudies of the University.

"The sources from which funds for the purposes described should be obtained appear to be clearly pointed out by the Act itself, when it empowers the Commisioners to enable or require the several Colleges, or any of them, to make contributions out of their revenues for University purposes, regard being first had to the wants of the several colleges in themselves for educational and other collegiate purposes.

"The principles on which payments from the Colleges should be contributed are, in the opinion of the Commissioners, as follows :—

"That such contributions should be made by the several colleges as nearly as possible on a uniform scale throughout, whether by annual payments to the proposed common University fund, or by a capital sum to be provided by the college out of money belonging to it in lieu of such annual payments; or by annexing any college emolument to any office in the University, with specified conditions of residence, study, and duty; or by assigning a portion of the revenue or property of the college as a contribution to the common fund, or otherwise, for encouragement of instruction in the University in any art, or science, or other branch of learning, or for the maintenance and benefit of persons of known ability and learning, studying, or making researches in any art or science, or other branch of learning in the University; or by providing out of the revenue of the college for payments to be made, under the supervision of the University, for work done or investigations conducted in any branch of learning or inquiry connected with the studies of the University within the University.

"The Commissioners think it probable that over and above the contributions to be required from the college on a uniform basis, some colleges may be willing, following in this respect the example of Trinity College, and in consideration of prospective additions to their revenues, or for other reasons, to contribute to the wants of the University by founding professorships or otherwise."

OXFORD.—The vacant Burdett Coutts Scholarship has been awarded to Mr. Edward B. Poulton, B.A., Scholar of Jesus College. The examiners have also announced that Mr. Francis H. Butler, B.A., Worcester College, distinguished himself in the examination and is worthy of honourable mention.

GLASGOW.—At a private meeting of the members of the University Council to consider who should fill the vacancy in the Chancellorship caused by the death of Sir William Stirling-Maxwell, fifty members voted for the Duke of Buccleugh, and thirty-one for Sir Joseph Hooker. A committee was appointed to endeavour to concentrate the vote upon the duke.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, March 7.—"Experimental Researches on the Temperature of the Head," by J. S. Lombard, M.D., formerly Assistant-Professor of Physiology in Harvard University, U.S. Communicated by H. Charlton Bastian, M.D., F.R.S., Professor of Pathological Anatomy in University College, London.

"Addition to Memoir on the Transformation of Elliptic Functions," by A. Cayley, F.R.S., Sadlerian Professor of Pure Mathematics in the University of Cambridge.

March 14.—"On the Function of the Sides of the Vessel in maintaining the State of Supersaturation," by Charles Tomlinson, F.R.S.

Anthropological Institute, February 26.—Mr. John Evans, D.C.L., F.R.S., president, in the chair.—The following new Members were announced :—Mr. W. Cohen and Mr. Gabriel.—A weapon from New Zealand was exhibited by Mr. Hyde Clarke.—Mr. J. Sanderson exhibited some stone implements and fragments of pottery from Natal, and read a paper on the subject of the present native inhabitants and their legends. The President remarked that the great bulk of the implements exhibited were extremely rude; and in respect to the pottery, observed that it presented remarkable similarity in pattern to pottery found in this country, a statement confirmed by the Rev. Canon Greenwell, who remarked that the pottery was hard and well-baked, and probably made for use in the household.—Mr. W. St. Chad Boscawen read a paper on the primitive culture of Babylonia, in which he referred to the rudely pictorial character of early Babylonian writing, and to its gradual development into a syllabic character, as shown in the syllabaries of Assur-banipal, which he illustrated by reference to the growth of pronominal ideas and the change of the archaic forms through hieratic into a court, or script hand. Treating the earlier forms as pictorial, he suggested that they gave evidence that the original form of dwelling was a cave, which then gave place to a construction of wattle and daub, and that to a structure supported by wooden beams on columns, and having doors and windows. To these were probably attached gardens about the entrance. The honour in which women were held by their children is indicated by the ideograph for mother, which signifies "home-divinity." Mr. Boscawen then stated, as his opinion, that the early Babylonians used the fire-stick to kindle their fires. The ideograph for "prison" is "dark-hole." In these early cities there were policemen who patrolled day and night. A vast number of other curious illustrations of the manners of ancient Babylon were deduced by Mr. Boscawen from the ideographs and syllabaries, and his lecture was listened to with great interest.

Physical Society, March 2.—Prof. W. G. Adams, president, in the chair.—The following candidates were elected Members of the Society :—Mr. J. P. Kirkman and Dr. W. J. Russell, F.R.S.—Mr. Sedley Taylor exhibited the colours produced in thin films by sonorous vibrations. A piece of thin brass perforated with a triangular, circular, or rectangular aperture, and bearing a thin film of soap solution, was placed horizontally on one end of an L-shaped tube; the beam of the electric lamp, after reflection from it, was received on a screen. It was shown that when a sound is emitted in the neighbourhood of the open end of the tube, the film takes up a regular form which is indicated by the different colours of the reflected light, and each note has its own particular colour figure; and further, with different instruments we have different figures. Thus when a square film was employed a kind of coloured grating was the result, which was modified by changing the note, and with a circular film concentric rings traversed by two bars at right-angles were observed.—Mr. W. H. Preece exhibited and described the phonograph. After referring to the manner in which the preceding communication bore on the subject of the telephone, he went on to explain the construction of the two instruments exhibited, which have been made in accordance with the published accounts of the apparatus and details received from the inventor, Mr. T. A. Edison, by Mr. Pidgeon and Mr. Stroh respectively. In the first of these the receiving and emitting discs are distinct, the former being of ferrotype iron, and the latter of paper, whereas, in the second form of apparatus, both these functions are performed by one and the same disc of iron. They also differ in that in Mr. Pidgeon's apparatus the drum receives its motion by hand, and in that of Mr. Stroh a descending weight is caused to communicate motion by a suitable train of wheels, which motion can be controlled and regulated by an adjustable pair of vanes. In both cases the drum is of brass traced over by a spiral groove, and the whole is mounted on a screw of the same pitch. The manner of using the phonograph is extremely simple. The drum having been covered with tinfoil, a uniform movement of rotation is given to it, and a fine metal point, firmly fixed to the centre of the receiving plate, is

brought in contact with it, care being taken to place the point accurately over the groove. If now this plate be sung or spoken to, the tinfoil will be indented in accordance with the vibrations communicated to the plate. The emitting plate having been provided with a resonator, its point is now brought into the position initially occupied by the point of the receiving plate, and on rotating the drum, with the same velocity, fairly identical sounds are given out. It will be seen that Mr. Stroh's apparatus has an advantage over that of Mr. Pidgeon, in that it secures a constant rate of rotation; but on the other hand, the sounds emitted by the paper disc appeared to be more distinct than those from the iron. A number of experiments were performed with the instruments. The sounds were reproduced at times with remarkable distinctness, and when Mr. Spagnoletti and Mr. Sedley Taylor sang "God Save the Queen," as a duet, through a double mouthpiece, the two voices could be clearly distinguished on its being reproduced. It was shown that even when an indented sheet of tinfoil has been employed to emit sounds, it retains its form with such perfectness that the sounds can be reproduced by means of it a second, and even a third time, with nearly equal distinctness. Prof. Graham Bell pointed out that the articulation of the instruments was very similar to what he had observed in the earlier forms of telephone, and he had no doubt, judging from his own experience of that instrument, that the phonograph will ere long be so adjusted as to articulate much more perfectly. He anticipated that the *quality* of the sound would be found to vary as the rate of rotation was altered, as well as the pitch, and this proved on experiment to be the case.

Royal Microscopical Society, March 6.—Mr. H. J. Slack, president, in the chair.—Mr. Chas. Stewart described a new species of coral said to have been obtained from an island in the vicinity of Tahiti, and which was referred to the genus *Stylaster*. The characteristics of the genus and the distinctive features of the new species were explained and illustrated by black board drawings, and specimens of the coral were exhibited under the microscope.—A paper on a new operculated infusorian from New Zealand, by Mr. Hutton, of Otago, was read by the president.—A paper by Mr. Adolf Schulze on a new and simple method of resolving the finest balsam-mounted diatom tests, was read by the secretary, and described the success which had attended the examination of this class of objects by means of the reflex-illuminator, and the immersion paraboloid, moistened with castor oil in place of water. The lines on *Amphipleura pellucida* were shown in this manner by Dr. Dickson, in illustration of the paper.—Lissajous curves drawn microscopically upon glass by Mr. West, were exhibited by Mr. Curties.

Institution of Civil Engineers, February 26.—Mr. W. H. Barlow, vice-president, in the chair.—The paper read was on liquid fuels, by Mr. H. Aydon.

Victoria (Philosophical) Institute, March 4.—A paper was read by the Rev. Dr. Rule, in reference to ancient Oriental monuments.

CAMBRIDGE

Philosophical Society, February 11.—Mr. J. W. L. Glaisher made a communication on the mode of formation of the factor table for the fourth million, now in course of construction.

PARIS

Academy of Sciences, March 11.—M. Fizeau in the chair.—The following papers were read:—On the phenomena connected with vision of coloured objects in motion, by M. Chevreul. He is able to show on a circle, one-half of which is black, the other half coloured, the complement of this colour, and prove that it is due to the arrangement of the two surfaces with regard to circular motion.—On some applications of elliptical functions (continued), by M. Hermite.—On the relative affinities and reciprocal displacements of oxygen and halogen elements combined with metallic substances, by M. Berthelot. The comparative reactions of the halogens and oxygen on various metals, and specially the reciprocal displacement between iodine and oxygen, depend neither on type nor on atomic or other formulæ of the combinations, but on the quantities of heat liberated by direct combination of the metals with each of the antagonistic elements taken in equivalent weights.—Influence of M. Pasteur's discoveries on the progress of surgery, by M. Sedillot. He shows the relation (to those discoveries) of Lister's treatment of wounds and its results; also Guerin's (with wadding, &c.). M. D'Abbadie stated that on the shores of the Red Sea the natives have a maxim that a wound, to be healed, should remain in contact with air; and he found this was the case. He thinks the air may there be free from microbes.—The vibrations of matter and the waves of the ether in chemical combinations, by M. Favé.—On Mr. Edison's phonograph, by M. du Moncel.—On the industrial applications of electricity, by M. Du Moncel. This is a short summary of vol. v. of his "Exposé des Applications de l'Électricité" (third edition).—M. Cialdi was elected correspondent for the section of Geography and Navigation, in room of the Emperor of Brazil, elected Foreign Associate.—On elliptic polarisation by reflection at the surface of transparent bodies, by M. Cornu.—Note on the vibrations of liquids, by M. Barthélemy. A claim of priority.—Discovery of a small planet at the Observatory of Pola, by M. Palisa.—Observations of small planets, by M. Palisa.—On the fundamental points of the system of surfaces defined by an equation with partial derivatives of the first algebraic order, linear with regard to these derivatives, by M. Fouret.—On a class of transcendant functions, by M. Picard.—On the variations of terrestrial magnetism, by M. Quet. He examines, with the aid of calculation, the theory which attributes to the sun a direct action on the magnetic and electric fluids of the earth.—On the precise orientation of the principal section of Nicols, in apparatus of polarisation, by M. Laurent. For this purpose he places between polariser and analyser a diaphragm, one-half of which only is covered with a thin plate of quartz parallel to the axis, having the thickness of half a wave. When the Nicol, *e.g.*, has to be placed at a determinate angle to certain reticular wires, the border of the plate is brought into the position, then the Nicols are placed accordingly.—Study of chloride of sulphur, by M. Isambert. There is only one chloride of sulphur in which the chloride is dissolved in considerable proportion at a low temperature.—On the substitution of sulphur for oxygen in the fatty series, by M. Dupré.—On the catechines (third note). Catechines of gambirs, by M. Gautier.—Action of fluoride of boron on organic matters (benzylic aldehyde, ethylene), by M. Landolph.—On a new pyrogenous derivative of tartaric acid, dipyrotartaric acetone, by M. Bourgoin.—On the acid of gastric juice, by M. Richet. The hydrochloric acid of gastric juice is in combination with tyrosine, leucine, and perhaps other similar substances.—Experimental researches on the inequality of the corresponding regions of the brain, by M. Le Bon. He examined 287 skulls in the Museum of Anthropology, and found 125 with predominance of the right side over the left, 111 with predominance of the left side, and 51 in which the bones were unequal but compensated each other, making the right side nearly equal to the left.—Classification of Stellerides, by M. Viguier.—On Garnierite, by M. Garnier.—Artificial production of brochantite, by M. Meunier. This was done by keeping fragments of galena about eleven months in a moderately concentrated solution of sulphate of copper.—The Silurian Tigillites, by M. Crié. He attributes those in the west of France to ancient plants, of calamitoid aspect, that lived in shallow water.—On the *rôle* of the retina in vision of near or distant objects, by M. Fano.

CONTENTS

THURSDAY, MARCH 28, 1878

SCIENTIFIC WORTHIES

XII.—WILLIAM HARVEY,[1] BORN APRIL 1, 1578, DIED JUNE 3, 1658

WILLIAM HARVEY was born three hundred years ago, on the first of April, 1578, at Folkestone, in Kent. He was the eldest son of his father; who seems to have been a substantial farmer, wealthy enough to send his eldest son to the university and to embark his five other male children in the mercantile pursuits in which they all acquired riches. At sixteen, Harvey was sent to Caius College, Cambridge, and graduated B.A. at nineteen. But, desiring to become a physician, Harvey wisely determined to proceed with his medical studies at one of the great continental seats of learning; and, by good hap, chose the University of Padua, which had been famous for a long succession of admirable anatomists, among them Vesalius and Fabricius of Aquapendente, who was the incumbent of the anatomical chair in Harvey's time.

After five years' study at Padua, Harvey took his doctor's degree in 1602, returned to England, and obtained the doctorate of his own university. In 1604, he married, began practice in London, and five years afterwards became physician to St. Bartholomew's Hospital. In 1615, Harvey was elected "Professor of Anatomy and Surgery" by the College of Physicians, and his first course of lectures was delivered in 1616. It is possible that he expounded his ideas respecting the circulation of the blood on this occasion; but, in this case, it is not obvious why he himself, in the dedication of the "Exercitatio Anatomica de Motu Cordis et Sanguinis," published in 1628, should not have said so. On the contrary he writes:—

"Meam *de motu et usu cordis et circuitu sanguinis* sententiam E.D.D. antea sæpius in prælectionibus meis anatomicis aperui novam; sed jam per novem et amplius annos multis ocularibus demonstrationibus in conspectu vestro confirmatam, rationibus et argumentis illustratam, et ab objectionibus doctissimorum et peritissimorum anatomicorum liberatam, toties ab omnibus desideratam, à quibusdam efflagitatam, in lucem et conspectum omnium hoc libello produximus."

Why "jam per novem et amplius annos," if he had really taught the circulation "per duodecim annos?" Harvey is so careful a writer that I cannot doubt he had a meaning in the use of the particular words he has adopted, and that he did not wish to lay claim to having enunciated his complete views before 1618 or 1619.

However this may be, the famous treatise itself was not given to the public until 1628, and its appearance conferred upon its author a fame which rapidly extended over the civilised world. James the First died in 1625, and it is, on the whole, pleasant to reflect that Harvey owed nothing to that foul pedant. But his son was a man of a different stamp, and whatever the verdict on his political deeds may be, shines as one of the few English sovereigns who have shown an enlightened sympathy with letters, with science, and with art. Harvey became Charles the First's physician about 1632, and the monarch repaid the real respect and affection with which his eminent subject evidently regarded him, in the only way for which Harvey was likely to care; namely, by doing his best to aid him in his investigations, and taking a cordial and intelligent interest in them.

Between 1630 and 1632, Harvey travelled on the Continent with the young Duke of Lennox; and, in 1636, he was physician to the Earl of Arundel's embassy to the Emperor. During this visit, he is said to have tried to convince Caspar Hofmann, of Nuremberg, of the circulation of the blood, experimentally, but in vain. When the troubles between the King and the Parliament broke out, Harvey accompanied his master in his campaigns He was at the battle of Edgehill, in charge of the Prince of Wales and the Duke of York; and he told Aubrey that "he withdrew with them under a hedge, and tooke out of his pockett a booke and read. But he had not read very long before a bullet of a great gun grazed on the ground neare him, which made him remove his station."

By the King's order, Harvey was elected Warden of Merton College, Oxford, in 1645; and to the same efficient cause, or to the fact that he was the King's physician, we must probably look for the conference of an honorary degree by the University of that day on a mere scientific discoverer. But, after the surrender of Oxford in the following year, Harvey retired from public life altogether, and spent the remainder of his days at the homes of one or other of his brothers, in the neighbourhood of London.

In 1649 Harvey, published his two letters to Riolan, which form a supplement to the "Exercitatio Anatomica;" and, in 1651, when he had reached the ripe age of seventy-three, the "Exercitationes de Generatione" appeared. "The rest is silence," save a few letters. In the last of them we have, dated April 24, 1657, he writes to Vlackveld:—

"Frustra autem calcar mihi addis, ut in ætate hac, non solum matura, sed etiam fessa, ad aliquid noviter moliendum me accingam. Videor enim jam mihi, meo jure, rudem deposcere."

No man had a better right to claim an honourable discharge from duty. Six weeks later the wished-for release arrived, and on June 3,

Spectatum satis et donatum jam rude,

Harvey died in the eightieth year of his age, full of honours as of years, more than sufficiently wealthy, and able long before his death, to say that the great truth he had discovered and taught was accepted by all whose opinion was worth having.[1]

The only works which Harvey published are the famous treatise on the Circulation (1628), with the two letters to Riolan (1649), and the "Exercises on Generation (1651)." But he was a most diligent observer and writer, and he incidentally refers to a "Disquisition on the Causes and the Organs of Respiration," to "Medical Observations," to a treatise "On the Generation of Insects," and to many observations on Comparative

[1] The portrait of Harvey will be presented to our readers in one of the May numbers of NATURE. Though every facility has been afforded by the College of Physicians, there has been unavoidable delay in its preparation. ED.

[1] "Circuitum sanguinis admirabilem, a me jampridem inventum, video propemodum omnibus placuisse: nec ab aliquo quippiam hactenus objectum esse, quod responsum magnopere mereatur."—*Exercitationes de Generatione*, Ex. lii.

Anatomy, the whole of which appear to have been destroyed when his house was plundered with the connivance, if not by order, of the Parliament, during his absence from London with the King.

Of the "Exercitatio de Motu Cordis et Sanguinis," I have treated so fully elsewhere [1] on a recent occasion, that I will again not touch upon the subject except so far as to repeat that, in my judgment, Harvey is entitled, beyond dispute, to be regarded as the sole discoverer of the circulation of the blood, and of the method of its propulsion by the heart.

The story of the extraction of the manuscript of the "Exercitationes de Generatione" from Harvey is well told by Ent, who undertook the charge of seeing the work through the press; a task of no small magnitude if we consider the superlative badness of the extant specimens of Harvey's handwriting.

The preface contains a singularly interesting disquisition on scientific method; and, among other observations the following, which is, perhaps, the weightiest in small compass ever laid before the student of physical science.

"For those who read the words of authors, and to whom impressions of their own senses do not represent the things signified by those words, conceive, not true ideas, but falsæ eidola and inane phantoms; whence they fill their minds with shadows and chimæras, and their whole theory (which they think to be science) represents but a waking dream or a sick man's delirium."

As in the case of the circulation of the blood, the scientific opinions of the day respecting the conditions of generation and the embryogenic process had descended from the Greeks. No one doubted that a large proportion of the lower forms of life owed their origin to equivocal or spontaneous generation, or, as it is now termed, abiogenesis; and, with respect to sexual generation, it was believed that the embryo originated at the time of sexual union, by the combination of two substances poured out *ad hoc*, the one being derived from the female, the other from the male parent. In this opinion both Aristotle and the Medici, following Galen, agreed; but they differed in the view which they took as to the nature and function of the two sexual elements. According to Aristotle, the female supplied merely the material of the embryo, by the excretion of a substance which he regarded as the purest part of the catamenial blood; this was coagulated, and endowed with the faculty of developing into an organism, by the spermatic fluid, of the male. The Medici, on the other hand, considered that the female produced a true spermatic fluid, analogous to that of the male, and having an equal formative energy; and indeed, that the sex of the embryo was determined by the predominance of the one or the other spermatic fluid.

As regards the embryogenic process itself, the Greeks had studied the development of the chick, and had learned somewhat respecting the fœtal state of viviparous animals; while, since the revival of learning, several important embryological investigations had been undertaken. Of these the most notable were those of Aldrovandus, of Coiter, of Harvey's master, Fabricius of Aquapendente, of Vesling, and of Parisanus, on the development of the chick. Fabricius' treatise, "De Ovo et Pullo," was accompanied by figures of the stages of development, which, for the time, must be termed very good; and it served Harvey as a sort of text-book, to which he constantly refers.

[1] "William Harvey," *Fortnightly Review*, February 1, 1878.

The "Exercitationes" show no advance on the knowledge of the ancients respecting the conditions of generation. Innumerable passages show that Harvey believed, as firmly as his predecessors and contemporaries did, in equivocal generation.[1] The persistent ascription to Harvey of the contrary opinion is simply astounding, and can only be explained on the supposition that those who quote what they are pleased to call "Harvey's aphorism," "Omne Vivum ex Ovo," against the holders of the doctrine of spontaneous generation, have never read the works of their authority.

I cannot discover the exact phrase "omne vivum ex ovo" anywhere in Harvey's works, though it is true that the sense of the words is expressed by him over and over again. But the context shows his meaning to be, not the assertion of the doctrine of biogenesis; but simply a declaration that, in whatever way a living being is generated, the nature which it at first possesses is that of an egg. And what Harvey wants to impress, by the frequent iteration of his opinion on this subject, is the difference between his view, that a germ is something which comes into existence more or less as a unit and has an individuality of its own, and that of his predecessors, who held that it is formed by the coalescence of separate entities. Nevertheless, there is an indication that Harvey was on the right track in respect of the question of spontaneous generation; and that, if his papers on the generation of insects had not been destroyed, he might have anticipated Redi; for the forty-first exercise contains the following remarkable passage:—

"But on these matters generally we shall have much to say, when we shall show that many animals, especially insects, take their origin and are generated from elements and seeds so small as to escape observation (like atoms floating in the air), which are scattered and dispersed hither and thither by the winds; yet these animals are supposed to arise spontaneously, or from putrefaction, because their germs are nowhere to be found."

It was exactly this thesis that Redi adopted and proved to demonstration, seventeen years afterwards, and therefore long before Harvey's death; and it is by following up the same line of argument that modern investigators have deprived abiogenesis of its last supposed experimental evidence. In whatever way, however, the germ of a plant or of an animal is produced it is the equivalent of an egg, and what Harvey means by an egg is clearly shown, in the following as in many other passages:—

"In the generation of all living things (as we have said) this is established, that they arise from some primordium (primordio aliquo) which contains not only the matter but the power of generation; and is, therefore, that out of which and from which the thing generated takes its origin. Such a primordium in animals (whether they proceed from parents, or arise spontaneously or out of putrefaction) is a humour contained in a membrane of some kind, or shell; in fact, a homogeneous body (corpus nempe similare) possessing life, either actually or poten-

[1] I pointed this out twenty years ago in my "Lectures on General Natural History," published in the *Medical Times and Gazette*. Take one passage out of fifty that might be cited: "Atque etiam terra suâ sponte plurima generat sine semine" (Exercit. xxix.). M. Pouchet might have taken this sentence for a motto.

tially. This primordium, if it is generated within an animal and remains there, until a like animal (univocum) is produced, is vulgarly called a *conception;* if, however, it is thrust out by parturition, or if it has originated elsewhere by chance, it is termed an *ovum* or *vermis.* I think, however, that, in either case, that from which an animal arises should be called primordium; just as plants produce their young from seeds; and that all these primordia are of one kind, namely, living things.

"I find a primordium of this sort in the uterus of all viviparous animals, before any fœtus can be discerned. In fact, there is a clear, viscid, white [colourless] fluid, like the white of an egg, inclosed in a membrane, which I term the egg of these animals; and, in red-deer and fallow deer, in sheep and other cloven-footed animals, it fills the whole uterus and both its cornua." [1]

It will be observed that, in the foregoing passage, Harvey insists upon one main quality of the primordium, namely, that it is a *corpus similare;* or, in other words, that it is relatively homogeneous; and, in the seventy-second exercise, "De humido primogenio," he insists strongly on what he believes to be the fact that the embryo takes its rise in a certain "humidum radicale et primigenium," "simplicissimum, purissimum et sincerissimum corpus," in which all the parts of the embryo are present potentially, but not actually, and out of which they arise by a gradual process of differentiation.

"The first rudiment of the body is a mere homogeneous and soft jelly, not unlike a spermatic coagulum, which, becoming changed (in accordance with the law of generation) and at the same time split or divided into many parts, as by a divine command, as we have said (let bone arise here, muscle or nerve there, here viscera, there receptacles of excretion, &c.) out of the inorganic arises the organic, out of the similar the dissimilar; out of the one and the same nature, many things of diverse and of contrary natures; not, indeed, by any transposition or local motion (as when by the power of heat homogeneous things unite, or heterogeneous things are separated), but rather by the disaggregation of homogeneous things, than by the aggregation of heterogeneous things." [2]

In this passage, as in those in which he advocates *epigenesis*, Harvey shows a complete grasp of the great truth that development is a gradual process of change from relative homogeneity to heterogeneity, put into such clear light in our own time by Meckel and von Baer.

Again, when Harvey dwells upon the close resemblance of the early conditions of the higher animals, and accounts for harelip as a retention of an embryonic condition, we see him hovering on the brink of some of the most important embryological generalisations of a century and a half after his time.

After Harvey, embryological theory distinctly retrograded for a full century, until, in fact, a hundred and eight years had elapsed, and, in 1759, Caspar Friedrich Wolff published his "Theoria Generationis." In the interval, the great truths laid down by Harvey, that all germs are homogeneous relatively to the forms to which they give rise, and that all those of the higher animals, at any rate, pass by *epigenesis* into the perfect living thing—"Fabrica a parte aliqua tanquam ab origine incipit: ejusque ope reliqua membra adsciscuntur: atque hæc per *epigenesin* fieri dicimus: sensim nempe partem post partem: estque isthæe, præaltera, proprie dicta generatio" (Exercitatio xlv.); these verities, justified by all our present knowledge, were ignored, and the doctrine of the "pre-existence of germs" and of "evolution" took their place. And so strong was the hold of the latter, that even Wolff's conclusive investigations produced little effect, and the full acceptance of Harvey's generalisations dates from the last half-century.

But while Harvey's views respecting the general nature of the embryogenic process were as much in advance of his time as were his doctrines respecting the motion of the heart and the circulation of the blood, his demonstration of them is a failure, the phenomena being too subtle and recondite for the means of investigation which he possessed.

So far as the process of fecundation is concerned, he is further from the truth than were the Greeks; for he steadily denies that the male element enters into the substance of the egg, or even comes into physical contact with it; and he ascribes the efficacy of the male to a sort of contagion, by which the female organism is infected, and in consequence of which, the ova, which he justly declares to be formed like any other growth, acquire the property of developing into embryos.

Again, though Harvey's discovery, that the region of the cicatricula in the hen's egg is the seat of the changes which give rise to the embryo, was of primary importance, he has not the least notion of the real nature of the cicatricula or of its relations to the yolk. The "primigenial radical humour," which he supposes to be the first commencement of the embryo, is nothing but the amniotic fluid, which is really formed long after the rudimentary body of the chick has appeared. And Harvey's supposition that the blood is that which is first formed and that the substance of the body grows round the vessels "like a mucor or fungus," is an error, which is, of course, enormous, and may seem unpardonable to any one who has not tried to make out the early stages of the development of the egg with the naked eye, or even aided by a hand-glass. It was the discovery that the rudiment of the body of the chick exists in the egg, long before Harvey supposed, that was one of the chief causes of the adoption of the notion of the pre-existence of germs which led to the "evolution" and "*emboitement*" hypotheses. Buffon, in fact, went so far as to say that the chick "exists fully formed (*en entier*) in the middle of the cicatricula when the egg leaves the body of the fowl," [1] thereby erring as far as Harvey did, but in the opposite direction.

After due deduction is made for these errors and shortcomings, however, the great merit of having been the first to grasp the true principle of interpretation of the process of development, must, I think, be accorded to Harvey; and if we consider the part which the study o development has played, and must henceforward continue

[1] "De Uteri Membranis et Humoribus." Elsewhere (Exercitatio xxvi.) Harvey says:—

"Ovum itaque est corpus naturale, virtute animali præditum: principio nempe motûs, transmutationis, quietis, et conservationis. Est denique ejusmodi, ur, ablato omni impedimento, in formam animalis abiturum sit; nec magis naturaliter gravia omnia, remotis obstaculis, deorsum tendunt; aut lævia sursum moventur: quàm semen et ovum in plantam aut animal, insita a naturâ propensione, feruntur. Estque semen (atque etiam ovum) ejusdem fructus et finis, cujus est principium atque efficiens."

[2] Ex. lxxii. "De humido primogenio."

[1] Buffon, "Histoire Naturelle," t. ii., ed. 2, 1750, p. 351.

to play in biology, the "Exercitationes de Generatione," though second to the "Exercitatio Anatomica," can hardly be said to have another rival in the contemporary literature of biological science.

Modern morphology, no less than physiology, has its root in the work of William Harvey. T. H. HUXLEY

ZÖLLNER'S SCIENTIFIC PAPERS

Wissenschaftliche Abhandlungen (Erster Band). Von F. Zöllner. (Leipzig: L. Staackmann, 1878.)

IF we take a somewhat different course in reviewing this work from that which we should naturally adopt with works professedly scientific, we hope at least to justify our conduct to the reader before we finish. For, alas, all is not scientific that professes to be science, and even celestial minds can harbour very curious feelings and express them with most unmistakeable vigour, while not always striking above the belt.

The key-note of this work, as well as of a great deal of the other somewhat voluminous writings of Prof. Zöllner, is struck by himself in a foot-note to p. 129, where he tells us that "the aim of all his scientific efforts has been to contribute, as far as the ability given him permits, to the realisation" of a certain "hopeful project":—viz., the explanation of *all* molecular actions by means of that Law of Electric Attraction (due to W. Weber) which "has already been so fruitful in cöordinating under one principle all electric and magnetic phenomena."

Very good and laudable:—though we may permit ourselves to say, in passing, probably very unpromising. But it is quite impossible to say what hints a competent mathematician may not obtain while he is attempting to prosecute the applications of any theory—however remote its principles may be from those which the experimental facts themselves suggest to the physical investigator in his laboratory. Unfortunately even this concession is thrown away upon Prof. Zöllner:—for he not only does not claim to be considered as a mathematician, but has on a former occasion (in his work on *Comets*) expressly denounced those who attempt "by differentiating and integrating" to get at natural laws. He is, as Helmholtz long ago said, a *genuine* Metaphysician, and (as such) is a curiosity really worthy of study:—not of course merely because he is a Metaphysician, but because in this nineteenth century he attempts to bring his metaphysics into pure physical science.

To a man whose whole object in scientific life is the establishment of Weber's Law as the fundamental fact of the Kosmos, of course all works are an Abomination in which even an attempt is made to show that *action at a distance* can be (and therefore *ought* to be) dispensed with. Hence Clerk-Maxwell's Theory, which, even its opponents must allow, has succeeded at least as well as Weber's in connecting and explaining the phenomena of electricity, magnetism, and light, must be demolished at all hazards. But the reader of Maxwell's great work on *Electricity*, who has seen in its very *Preface* that the main object of that work was to carry out to their legitimate mathematical developments the physical ideas of Faraday, will scarcely be prepared to find that Prof. Zöllner accepts Faraday and denounces Maxwell!

This *tour de force* is worthy of so accomplished a metaphysician. It is absolutely refreshing in its coolness! According to Prof. Zöllner, both Clerk-Maxwell and Sir W. Thomson (to whose advice the former owns his indebtedness) quote Faraday correctly, and yet altogether misapprehend his meaning! In fact we are now told, though not in so many words, that Faraday, whom we had all looked on as an opponent of action at a distance, was really a firm believer in it, and a strenuous advocate of it! Not only Faraday, but even Newton himself:—in spite of the celebrated *Letters to Bentley*, in which all of us have hitherto read the inconceivability of distance-action to any mind which "has in philosophic matters a competent faculty of thinking"—even Newton himself, it seems, believed in action at a distance!

On this no farther comment is necessary than one I made some time ago, when Prof. Zöllner, to his own satisfaction at least, proved me to be ignorant alike of Latin and of the very *First Law of Motion;*—viz, that "Prof. Zöllner should not attempt to criticise . . . until he acquires sufficient knowledge of British technical terms . . ."

That a good deal of Prof Zöllner's censure is due to his imperfect apprehension of English, will, I think, be allowed by every candid reader. I say nothing of numerous misspellings—sometimes ludicrous, such as "*in his sobber* (*sic*) *senses*"—which occurs twice at least (pp. 142, 711), because there are quite as many misspellings in the German, and all are, therefore, probably due to the printer. But it is a wonderful piece of information for us benighted islanders to be told that our foremost scientific men, while quoting Newton accurately, entirely miss, or rather misrepresent, his meaning. So wonderful that I certainly shall not be believed, unless I refer definitely to some of the inculpatory passages:—

[The passage (pp. 141–152) is too long for translation, so I give a small part only; restricting myself to the *tone* in which British authors are spoken of, for the *substance* of the accusation, such as it is, has been already indicated.]

"One's impaired power of discovering contradictions prevents his recognising them as such even when the effect of the contrast is heightened by juxtaposition. Hence we must ascribe the non-retractation of such by their authors not to moral weakness but to incapacity. Hence also the surprising *naïveté* with which such men (*i.e.*, Sir W. Thomson, Clerk-Maxwell, *et hoc genus omne*) hand over to their critic the weapons with which to exterminate them, &c., &c. He who thinks it superfluous to bother himself with the thoughts of his predecessors and contemporaries loses *ipso facto* all right to consideration for himself and his writings. Such an author will in after time be forgotten, just as he has forgotten his predecessors, and this in the name of Eternal Right. For, only in the continuity of the mental work of successive generations is there security for the progress of Humanity!"

The reader of this will perhaps think that he has seen enough of Prof. Zöllner and his work:—enough at least to enable him to form a pretty shrewd guess as to the scientific value of the whole. But I must be excused if I trouble him with a few additional remarks on another aspect of the book.

Some years ago Prof. Helmholtz kindly undertook to revise the German translation of *Thomson and Tait's Natural Philosophy*, and was in consequence somewhat

wildly attacked by Prof. Zöllner in the *Preface* to his book on *Comets*. To that attack Prof. Helmholtz replied in a very admirable article, of which a translation has already appeared in NATURE (vol. xi. pp. 149, 211).

The great crime which according to Prof. Zöllner was committed, was a double one. Sir W. Thomson and I ventured to express an opinion (to which we still adhere) unfavourable to theories such as that of Weber:—and Prof. Helmholtz so far forgot his duty as a German as to be responsible for the reproduction of our work in his native tongue! As we now know that the promulgation and extension of Weber's Theory has been the object of Prof. Zöllner's life-work, perhaps it was not unnatural that he should complain of such conduct. But it is quite another thing when, after being completely demolished from the *scientific* point of view, he returns to the attack in another style—bringing against the various persons named charges of a totally different character—though all equally groundless.

A great many of these arise undoubtedly from imperfect acquaintance with the English language. Thus, to take a ludicrous one, Prof. Zöllner evidently imagines that "smoke-rings" *must* be formed with tobacco-smoke? And he fancies that it was in a smoking party that Sir W. Thomson hit upon his hypothesis of vortex atoms. For, after translating part of Thomson's own account of his theory, he says that a "skilled and powerful tobacco-smoker was necessary to the experimental verification of it."

Smokers, to whose charmed circle Prof. Zöllner evidently does not belong, can alone judge how skilled and powerful they would have to become before they could produce from their own lips the vortex rings, *full of sal-ammoniac crystals* and somewhere about six or eight inches in diameter, which Sir W. Thomson describes in the paper referred to. But Prof. Zöllner comes back to this notion, as he does to others, with absolutely "damnable iteration." Here is an instance (p. 103) which we paraphrase as follows:—

"The reader will note that 'Tobacco-smoke' and a 'creative act' are the inseparable companions of Thomson's Vortex-atoms:—although in the whole of Helmholtz's paper, on which Sir W. Thomson has erected the airy structure of his hypotheses, there is not a single passage in which such things are alluded to.

"Since Sir W. Thomson and the mathematical supporters of his hypotheses continually employ tobacco-smoke for the explanation of their views, I also may be permitted to employ the same medium to make clear my notions. Were I to describe the feelings with which I crossed the threshhold of the Vortex-world of Thomson after leaving the clear and bright Thought-world of Newton, Kant, and Faraday, I could not succeed better than by comparing them to those of the Alpine traveller who leaves the enlivening freshness of the clear mountain air to enter the tobacco-laden atmosphere of a muggy beerhouse!"

We next have Thomson's (and Helmholtz's) speculations as to the origin of life on the earth:—once more overhauled and torn to shreds. Then the unfortunate "luminous corpuscle" of Thomson and Tait has again to perform its antics—but in a somewhat new phase. For it is now shown to be due to the same inaccuracy of thought (Denkfehler) as the "moss-grown fragments."

"Only the yet undeveloped understanding of a child can content itself with such hypotheses, as it does with the answer to the child's question, 'Where did the new-born little brother or sister come from?' The mother soothes the childish causation-excitement with the answer, 'The Stork brought it':—on the correct presumption that the child will not farther inquire whence or from whom the Stork received the infant."

So far as I can judge without an attentive perusal of the whole 732 pages of the work (*Erster Band*), such as, amusing though it is throughout, I cannot spare time to bestow, Prof. Zöllner seems to think that Clerk-Maxwell, Thomson, and myself *believe in the existence* of those imaginary beings (invented by Maxwell, and called *Demons* by Thomson) who were introduced for the purpose solely of showing the true basis on which the Second Law of Thermodynamics has to be received as a fact in physical science! Hence we are treated to a whole Chapter called "*Thomson's Dämonen und die Schatten Plato's*." [1]

But it was well that this Chapter should be written. For Prof. Zöllner has recorded in it a discovery *of the very first order*:—if it be correct. He has held the two ends of a cord (sealed together) in his hand, while trefoil knots, *genuine* IRREDUCIBLE TREFOIL KNOTS, of which he gives us a picture, were developed upon it! He shows us the reasoning by which he was led to predict the possibility of this very wonderful achievement—absolutely unique in character, so far as I know, throughout the whole range of science. Prof. Klein, of Munich, some time ago showed, as is well known, that knots cannot exist in space of four dimensions. Hence Prof. Zöllner was led to conclude that beings (not, of course, *Thomson's Dämonen* nor *die Schatten Plato's*, for these are unscientific, and therefore impossible) in space of four dimensions could put an irreducible knot on an earthly string of which the ends were fastened together! It is some time since the Astronomer-Royal for Ireland told me *his* jocular mode of arguing from Klein's discovery:—viz., that all the secrets of the spiritualistic "rope-trick" could be at once explained by supposing that *inside* the mysterious cabinet (in which the tambourines and the musical boxes fly about) space was of four dimensions—so that the well-corded performers were at once loosed from their bonds on entering it! But Prof. Zöllner (with the assistance of the spiritualists) has tied knots by means of beings who exist in four dimensional space!!! Those who tied can of course loose, so that there is now (thanks to Prof. Zöllner and the spirits) no such thing as an irreducible knot!

I need say nothing of the treatment which Prof. Zöllner bestows on other scientific men with whom he has the misfortune to disagree: such as the imaginary *execution-scene* (pp. 377–416) of a distinguished Physiologist! Plain men in this country, and in Germany also I doubt not, have uncomfortably plain terms for such outbursts. But such things are not for a scientific journal. I can hardly divest myself of the impression that Prof. Zöllner, in spite of his oft-expressed utter detestation of "Jokelets" of all kinds (Witze, Scherze, &c.) has been led by his feelings of "sittlicher Entrüstung" to attempt the perpe-

[1] This is not the place to continue discussions with Prof. Clausius, but the reader of Prof. Zöllner's book should be warned that, extensive as is his reading, it does not always seem to include the most cogent arguments which have been presented on one or other side in several controversies of which he undertakes to give an account.

tration of a gigantic joke upon his readers. For I have looked in vain through this large volume for anything that can well be called *Science;* with the one exception of some remarkable experiments due to Fresnel, to which it is well that attention has been called.

In conclusion, though I cannot make pretensions to any minute acquaintance with the German language, I think I may venture to suggest to Prof. Zöllner, for his next edition, a title which shall at least more accurately describe the contents of his work than does his present one. I cannot allow that the title "Scientific Papers" is at all correctly descriptive. But I think that something like the following would suit his book well :—

Patriotische
METAPHYSIK DER PHYSIK,
für moderne deutsche Verhältnisse.

Mit speciellem Bezug auf die vierte Dimension und den Socialdemokratismus bearbeitet.

With this little hint, which I hope will be taken, as it is meant, in good part, I heartily wish him and his work farewell.

P. G. TAIT

A DICTIONARY OF MUSIC

A Dictionary of Music and Musicians. By eminent writers, English and Foreign. With Illustrations and Woodcuts. Edited by George Grove, D.C.L. (London: Macmillan and Co., 1878.)

NO better proof of the spreading interest in musical subjects which is now taking place in England could be found than the publication of this important work. Although similar "Lexicons," some of them extending to the portentous dimensions which German monographs are apt to assume, are not uncommon in that country, there have, as far as the writer knows, been hitherto none in our language which exactly occupy the position aimed at by this. Those which most nearly approach it, are either somewhat antiquated, or, like the excellent little work of Dr. Stainer, propose to themselves a far more restricted object. Nor indeed is the reason of the difference in this respect between the two countries difficult to assign. In Germany the whole population is more or less musical; every little town or village has abundance of practical musicians, mostly playing stringed instruments, among its inhabitants, who not only can take their part efficiently in a quartette, or in a local orchestra, but who are sufficiently informed in musical theory and literature to furnish an intelligent public which can support and encourage extensive undertakings of a scientific and historical character.

In England, on the other hand, unlike Germany, there has been, until quite lately, as little of representative musical culture as there has been of really national soldiering. We had been content to leave the defence of our country, no less than the executive realisation of great artistic master-pieces, to a separate and stipendiary class; while the bulk of the nation had merely "assisted," according to the French sense of the word, by listening and applauding. In both instances we, to a considerable degree, realised the dreams of Plato's Utopia; and though in the one case our φύλακες, the army, in spite of its small size and its professional leaders, for education and gallantry are probably unparalleled, it is, perhaps, to be feared that the artistic class, the μουσικοὶ, have somewhat suffered from isolation and lack of responsibility.

To this cause, and to unthinking prejudice, must be referred the tone of depreciation if not of contempt, which in the last century attached to the name "fiddler." It is conspicuous in the "Tweedledum and tweedledee" epigram of Handel's day, and frequently crops out in the Johnsonian, and even in later periods. The altered feeling of the present day cannot be better illustrated, than by the public estimation of Rubinstein or Joachim, or the genuine national grief at the early death of Titiens.

But the reform in the republic of sweet sounds must come, and is coming, *ab extra.* Audiences themselves must be fairly proficient in an art to esteem its higher developments and manifestations. The supply, according to the laws of political economy, must precede the demand; nor can true æstheticism of any kind fully prosper until the bulk of the population have been educated up to its intelligent and critical comprehension. For the moment it may be that in this particular branch the outsiders have distanced the regular executants. It would be a severe, but not altogether false statement to make, that in modern England—which has really become a musical nation—all classes are musical except the musicians. It is certain that our grandest celebrations, such as those of Handel and that at Leeds, are festivals where the latter are only secondary to the hearty and enthusiastic willingness of a voluntary but well-disciplined non-professional choir. Indeed it might, *à priori,* be anticipated that such would be the case, since the fondness for music, although it may be materially developed by circumstances and education, still remains very much of a gift; and this gift, which forms the strongest motive to exertion in acquiring it, is far more likely to exist in one who turns to the subject from love than in those who have simply adopted its study by chance, or as a means of earning a livelihood.

That such is to a certain extent the fact, receives ample illustration from the very first page of Mr. Grove's initial number, in which are recorded the names of the contributors to the work. Including the editor himself, who, though not a professional musician, has earned, under the familiar initial which he here again adopts, a full title to speak with knowledge and authority on musical subjects, a large proportion of the writers are not dependent on the art or practice of music for their social status. Among them will be found clergymen, a consul, a colonel, a doctor, an engineer, a Queen's counsel, a schoolmaster, and many others, whose devotion to the cause of music must be purely voluntary and a labour of love. As it cannot be doubted that all alike have given proofs of their competence to undertake the task entrusted to them, it is surely no forced conclusion to regard their co-operation as evidence of the depth to which educated English society is now penetrated by this subtle and once neglected branch of æsthetic culture. To the same class, moreover, the work appeals for support, a support which is more than justified by the laborious care, the painstaking and punctilious accuracy displayed by the editor in its compilation.

The present instalment of the work is the first of a series of quarterly parts, and only contains the letter A, with part of letter B. On turning over the pages the articles which attract the eye are one on ABBREVIATIONS

in music and one on ARPEGGIO, by Mr. Franklin Taylor; an interesting account of the ACADÉMIE DE MUSIQUE, by Mr. John Hullah; an excellent little treatise on ACCENT in music, with abundant musical examples, by Mr. Ebenezer Prout; another on ACCENTS in plain song, by the Rev. Thomas Helmore; instructions as to ACCOMPANIMENT, by Mr. Hopkins, of the Temple, supplemented by another article on ADDITIONAL ACCOMPANIMENTS, by Mr. Prout; ÆOLIAN HARP is from the pen of Mr. Hipkins; ANTHEM is given by Dr. Monk, of York; ARRANGEMENT, by Mr. Hubert Parry; BAGPIPE, by the writer of this notice. In the biographical department, which is especially full, a long and exhaustive account of the BACH family, by Herr Maczewski of Kaiserslautern, stands foremost. There are also interesting notices of ADOLPHE ADAM and of AUBER, by Mr. Franz Hueffer; of many Italian composers, by Mr. Edward H. Pember, Q.C., of DR. ARNE, and of ATTWOOD, by Mr. Husk, Librarian of the Sacred Harmonic Society; of DR. ARNOLD, and a sympathetic biography of MICHAEL BALFE, by the late Dr. Rimbault. Sir Frederic Ouseley and the Editor contribute several smaller notices. The names of English musicians appear to have received especial attention.

There can be no hesitation in saying that the work just commenced promises to fill a gap in English bibliography, and that it furnishes excellent material for reference. Besides this, it presents the collateral advantage of offering a charming combination of amusement and instruction for desultory reading in the many *horæ subsecivæ* which occur even in the lives of the most busy.

W. H. STONE

OUR BOOK SHELF

Pioneering in South Brazil. Three Years of Forest and Prairie Life in the Province of Paraná. By Thomas P. Biggs-Wither. Two vols. With Map and Illustrations. (London: John Murray, 1878.)

MR. BIGGS-WITHER has written two volumes of genuine and varied interest and much instruction, as a result of his three years' work in a little-known region of South Brazil. He went out as one of an engineering party to open up a road between the Atlantic and Pacific, and he traversed much of the country on the banks of the rivers Ivahy and Tibagy, tributaries of the Paraná. Much of his time was spent in the forests of this region, virtually unexplored, and presenting a splendid field for any enterprising naturalist. Mr. Wither is an excellent observer, and his book abounds with information on the natives, the natural history, and physical geography of the region. He met with many adventures, and suffered much from heat and insects, but altogether he seems to have had a thoroughly enjoyable time of it. He writes throughout in an attractive and simple style, and his work must be regarded as an important contribution to a knowledge of the luxuriant region with which it deals.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

The Phonograph

WE shall be much obliged if you will allow us to draw the attention of your readers to a curious fact which the phonograph has allowed us to prove, and which we announced last Monday at a meeting of the Royal Society of Edinburgh. We have seen no mention of the fact elsewhere.

Not only are vowels unaltered by being spoken backwards, but the same fact is true of consonants. Whether the pulsations of air be made in a given order or in the reverse order the ear accepts the sound as indicating the same letter. This is true of all the simple vowel sounds and of all the simple consonant sounds, including of course several combinations which in English are spelt with two letters, as *th* or *ng*, but which are really simple consonants.

We tried the experiment on single pairs of syllables separated by a single consonant, as *ada*, *aba*, *aja*, *etc.* A person coming from outside and ignorant of what consonant had been spoken was able to identify the consonants quite as well backwards as forwards. The chief difficulty was found in distinguishing *affa* from *assa*.

We find that this peculiarity is not limited to consonants between vowels, but that *ab* said backwards becomes *ba*. We have here a standard as to what does really constitute a single letter or element of articulate speech; it is any one reversible part. Your readers who possess a phonograph may most easily verify this observation by saying a word backwards, and hearing the phonograph say it intelligibly forwards; for instance, *noshäeesossa* produces *association* beautifully.

We shall be glad to learn whether this fact has been already published, and also whether it was foreseen as a possibility by any writer.

FLEEMING JENKIN
J. A. EWING

Edinburgh, March 25

The Age of the Sun's Heat in Relation to Geological Evidence

1. IT is an admitted fact that the age of the sun's heat will not harmonise with the evidence of geology, on the supposition that this heat was *solely* derived from the approach of matter under the action of gravity. Dr. James Croll, in dealing with this question in a recent number of NATURE,[1] has suggested the existence of a previous proper motion in the colliding matter that formed the sun, whereby, in accordance with accepted physical principles, a store of heat adequate for any period might have been provided. However a difficulty is raised here by Dr. Croll, in the *Philosophical Magazine* (May, 1868), where this question is first dealt with, and as this difficulty would seem on examination not to be insurmountable, I venture to call attention to the subject here, more especially as attendant questions of interest would seem to attach to it.

2. Of course it is admitted that the age of the sun's heat is the limit to conditions of life on the earth, and the point in question is that if the sun had acquired such a store of heat as geological time would appear to demand, then the sun must have been (owing to the excessive heat) a very extensive nebula, probably extending far beyond the limits of the present solar system, and consequently, that even if such a store of heat had existed in the sun, it would not be available for geological time, since the earth could not then have existed as a separate planet, from the fact that the solar nebula would then have extended beyond the limits of the earth's present orbit. Dr. Croll says (p. 372): "But if the sun had originally possessed the amount of energy supposed, then his volume would have extended beyond our earth's orbit, and of course our earth could not at that time have existed as a separate planet." This, therefore, puts a difficulty in the way of the sun having possessed such a store of heat as would be available for geological time. The accepted principles of Laplace are, of course, admitted here, according to which the earth originally formed part of the nebulous mass of the sun, and became naturally detached through the rotation of the nebula at its contraction.

3. Here it seems to have been tacitly assumed (according to the quotations above given) that the present orbit of the earth was its *original* orbit. Is there, however, any necessity for assuming this? For in this lies all the difficulty. Are we not rather warranted in inferring from accepted principles that the present orbit of the earth was *not* its original orbit. For it is an admitted fact that resisting media (the ether, &c.) exist in space, by which, through friction, the orbits of the planets are gradually becoming contracted, so that they slowly approach the sun. It is a mere question of *time*, therefore, for the earth to have come in towards the sun from any distance, or its original

[1] NATURE, vol. xvii. p. 206; also *Quarterly Journal of Science*, July, 1877

orbit might (for anything we can say to the contrary) have been beyond the present orbit of Jupiter. However slowly we may suppose the earth to be approaching the sun, yet in the vast epoch of time (which is precisely what is *necessary* in order to harmonise with geological evidence) it may have approached millions of miles towards the sun. There is one point of peculiar harmony here which is worth noticing in connection with this, viz., as the sun cools down or gives out less heat to the planets, so the planets reduce their distance from the sun; thus tending to equalise the heat conditions suitable for life. Thus, although the heat of the sun when first formed may have been enormously greater than it is at present, yet on account of the distance of the planets (including the earth) from it at that remote time, the conditions for life may have been as favourable as now, and thus the first geological changes may have commenced on the earth at that remote epoch when the sun was an incandescent nebula occupying a vastly greater volume than now (perhaps even the volume of the earth's present orbit), or under these conditions any interval of time for life on the earth that geological evidence may require is afforded.

4. There is another point that would appear to be of interest in connection with this subject. The *rate* at which a planet approaches the sun through friction in the media in space would depend (admittedly) on its mass, or would be greater when its mass is less. It follows evidently from this therefore that the great planets, Jupiter, Saturn, &c., must have approached the sun at a slower rate than the earth (or the smaller planets generally). It would follow therefore (more particularly in view of the vast epoch of time demanded by geology) that the *relative* position of the planets must have changed from this cause, that the earth, for example, must at one time have been nearer Jupiter than at present; more especially as the greater velocity of the earth in its present contracted orbit causes greater friction (and thereby brings the earth more rapidly towards the sun). Indeed it is an evident consequence of this principle that it would require only a certain relative *difference* in mass of the planets (or in the length of the elapsed time) to have made the small planets occupy positions *beyond* the larger planets originally, and so the positions of the planets to have been reversed, *i.e.*, the smaller planets *furthest* from the sun, and the larger planets nearest. The tendency of the friction evidently is to arrange the positions of the planets, so that the larger are furthest from the sun.[1] This it may be noted is the position at the present time. We do not of course mean to assume necessarily that there has been an actual reversal in the positions of the planets; all we adduce is that friction must inevitably tend to *change* relative position, when the masses of the bodies are different, and whether the positions are reversed depends therefore on the *time* during which this cause was in operation (and here we are considering especially the vast interval of time required by geology)—the change of relative position being more rapid the greater the relative differences of the masses. Thus it is a known fact that a meteorite approaches the sun or contracts its orbit at an enormously more rapid rate than a planet. It is so far certain that through friction in the medium known to exist in space, the planets (whose masses are different) must have changed to some degree their relative positions, or that the earth (for example) must have been nearer Jupiter at one time than it is now. These it should be observed cannot be regarded as speculations, but rather as deductions dependent on accepted principles.

5. Time may evidently have as great significance in physical as in geological changes, or in giving time its full import great results may follow; and it will be admitted that it is of interest to trace the slow operation of causes into their legitimate results through lengthened time epochs, not confining the attention to the infinitesimally narrow range of human experience.

London, March 21 S. TOLVER PRESTON

[1] It would seem a rather curious fact to note that those planets which contain within themselves the greatest store of heat (*i.e.*, the large planets), and which therefore would probably be the longest time before they were adapted to the conditions of life, are those which approach the sun the slowest. It is also evident that the fact of the earth being a *small* planet, would tend to augment the difference between the range of its present, and that of its original, orbit.

English Lake-dwellings and Pile-structures

GENERAL LANE FOX has described the old, and, in some cases, successive pile-works in the peat of Finsbury and Southwark, outside Roman London (*Anthropological Review*, vol. iv. No. 17, April, 1867, pp. lxxi. *et seq.*). Another very interesting case was evidently under Sir C. Bunbury's observation in 1856, near Wretham Hall, six miles north of Thetford, where, in a drained mere, "numerous posts of oak-wood, shaped and pointed by human art, were found standing erect, entirely buried in the peat." Red-deer antlers, both shed and broken from the skull, and also *sawn* off, were found in this peat. (See *Quart. Journ. Geol. Soc.*, vol. xii., p. 356.)

Since writing the above, I have been informed that Mr. W. M. Wylie, F.S.A., referred to this fact in "Archæologia," vol. xxxviii., in a note to his excellent memoir on lake-dwellings. I can add, however, that remains of *Cervus elaphus* (red deer), *C. dama ?* (fallow deer), *Ovis* (sheep), *Bos longifrons* (small ox), *Sus scrofa* (hog), and *Canis* (dog), were found here, according to information given me by the late C. B. Rose, F.G.S., of Swaffham; who also stated, in a letter dated August 11, 1856, that in adjoining meres or sites of ancient meres, as at Saham, Towey, Carbrook, Old Buckenham, and Hargham, cervine remains have been met with: thus at Saham and Towey, *Cervus elaphus* (red deer); at Buckenham, *Bos* (ox) and *Cervus capreolus* (roebuck); at Hargham, *Cervus tarandus* (reindeer).

The occurrence of flint implements and flakes in great numbers in the site of a drained lake between Sandhurst and Frimley, described by Capt. C. Cooper King, in the *Journal* of the Anthropological Institute, January, 1873, p. 365, &c., points also in all probability to some kind of lake-dwelling, though timbers were not discovered.

Lastly, the late Dr. S. Palmer, F.S.A., of Newbury, reported to the "Wiltshire Archæological Society" in 1869, that oaken piles and planks had been dug out of boggy ground on Cold Ash Common, near Faircross Pond, not far from Hermitage, Berks.

T. RUPERT JONES

Selective Discrimination of Insects

AS bearing on the question discussed by "S. B.," and by Mr. Bridgman and others, at p. 163 *ante*, and in previous numbers of NATURE, the following observations may have some interest. One day in the latter part of July, 1877, I took on a flower of red clover (*T. pratense*) an humble-bee (*Bombus Carolina ?*), having the hairs of its body and legs densely dusted with pollen-grains of an *Althæa*, which was in full blossom in the same enclosure, about one hundred feet from the spot where I took the bee.

On the same day and at the same place I attempted to take another *Bombus*, which was ravishing a flower of the same species of clover. It escaped me, and, flying to a distance of about twenty feet, alighted on a flower of a Canada thistle (*Cirsium arvense*), into which it immediately plunged its tongue. After watching it feed for a moment or two, I again attempted to capture it, when it again escaped, and, flying to about the same distance as before, alighted on a flower of a larkspur (*Delphinium Consolida*), and upon my third attempt to take it, it flew away and disappeared.

As to whether insects are attracted by odour or colour, I wish to call attention to an observation of Mr. Crouch, as detailed by Mr. Gosse in "A Year at the Shore." "*Tealia crassicornis* is as good a mimicry of the great dahlias as the *Sagartiæ* are of the daisies." "Even bees are occasionally deceived. Mr. Crouch, when once looking at a fine specimen which was expanded so close to the surface that only a thin film of water covered the disc and tentacles, saw a roving bee alight on the tempting surface, evidently mistaking the anemone for a veritable blossom."

Covington, Ky., U.S.A. V. T. C.

The Telephone as a Means of Measuring the Speed of High Breaks

IN some experiments with an induction coil and wheel break which I have lately been engaged on I have found the telephone useful in determining the number of times per second in which the current is broken.

For this purpose it may be attached to the secondary terminals, or the whole or part of the primary current may be passed through it.

The telephone may also be used generally for determining the speed of electro-magnetic motors by taking advantage of the fact that the current driving them is either short-circuited or broken a definite number of times in each revolution. The telephone wires may in this case be attached at two points some distance apart on one of the battery wires. The note of the telephone gives the number of breaks per second.

Pixholme, Dorking, March 17 J. E. H. GORDON

Meteor

As meteors are rarely seen by day, I write to inform you that I observed one this morning, at exactly 10.20 A.M., not only in broad daylight, but in bright sunshine. I only caught a hasty glance of it as it was disappearing. It was in the eastern side of the sky, descending towards a point in the horizon nearly due north from us, at an angle of about 40°. As we are quite in the country, it could not have been anything else than a meteor. I found that two of our servants had seen it also, and described it as having a tail, which I did not see. JAMES ELLIOT

Goldielands, near Hawick, March 25

The Bermuda Lizard

IN his "Geographical Distribution of Animals" (Am. ed. ii. p. 135), Mr. Wallace states, speaking of the Bermudas, that "a common American lizard, *Plestiodon longirostris*, is the only land reptile found on the islands."

Plestiodon longirostris is not a common American species. It is peculiar to the larger islands of the Bermuda Archipelago. It was described by Prof. E. D. Cope (*Proceedings* of the Academy of Natural Sciences, Philadelphia, 1861, p. 313) from Bermuda specimens. It has never been found elsewhere. Its closest affinities are with a West African species.

G. BROWN GOODE

U.S. National Museum, Washington, January 21

Landslip near Cork

THE village of Coachford, on the River Lee, sixteen miles from Cork, has been the scene of a curious landslip, or subsidence of soil.

On Wednesday, the 13th inst., a man on his way to work, at about eight o'clock A.M., on going along a path beside a dyke or bank which separates two fields close to the village, noticed a breach in the dyke which had not existed before; and on going to examine, found a deep hole in the earth about a yard in diameter, the depth of which appeared to him to be about a hundred feet, and at the bottom of which he heard the sound of running water. From that time till six o'clock P.M. the hole gradually increased in diameter by the falling in of the sides, until it appeared as I saw it on Sunday, the 17th inst., a conical cavity fifty to sixty feet in diameter and thirty to forty in depth.

The soil is composed of gravel and sand, with a substratum of limestone.

The same thing has evidently taken place several times before in the immediate vicinity of the above-mentioned cavity, as there are no less than seven other similar depressions of various sizes in the same piece of ground, but the formation of none of these is remembered by even the oldest inhabitants of the place.

I should mention that the fields between which the landslip has taken place lie pretty high, and that the River Lee is about half a mile distant. A belief has long existed in the village that a stream, which is supposed to flow into the Lee, runs beneath the place, at some depth underground.

Cork, March 20 C. J. COOKE

JOACHIM JOHN MONTEIRO

A FEW days ago (NATURE, vol. xvii. p. 391) we recorded the melancholy fact of the death of this enterprising African traveller. We have since been favoured with a few particulars of his life and labours, which appear to us to demand more than a passing word of recognition. His work on "Angola and the River Congo" (Macmillan, 1875) is still fresh in the mind of the public, and has been made doubly interesting through the recent travels of Mr. Stanley. Mr. Monteiro commenced his scientific education at the Royal School of Mines, under the late Sir H. De la Beche, and at the College of Chemistry under Dr. Hoffmann, at both of which places he obtained first-class honours. His first visit to Angola was in the year 1858, when he went to work the Malachite deposits at Bembe, in that province, and also the blue carbonate of copper. This obtained honourable mention in the International Exhibition of 1862. It was while working these deposits at Bembe that the King of Congo came down to pay a visit, and was received with all honours. A very curious letter from this king, asking for a "piece of soap to wash his clothes with," is now in the possession of the British Museum.

It was during his stay at Bembe, and while exploring the country round, that he discovered that the fibre of the *Adansonia digitata* was so valuable for the purposes of making paper, but it was not until 1865 that he returned to the coast for the purpose of developing this extraordinary discovery. He continued to work this enterprise for many years, so as to fully establish the claim of this fibre to being the most valuable natural product for paper-making. Paper made exclusively of this fibre is scarcely to be distinguished from parchment, and it is owing to this remarkable quality that a small percentage of the fibre enables the manufacturer to utilise substances which would be otherwise useless. While at Bembe Mr. Monteiro procured some of the most interesting birds, and although the results of his first collecting were perhaps not so important in regard to novelties as those made later on, the value of this, our first contribution to the avifauna of Inner Angola, will never be underrated by ornithologists. In September, 1866, he accompanied Mr. A. A. Silva, the United States Consul, on an ascent of the River Quanza for the purpose of opening up the country to trade, and the natives were greatly astonished at their first experience of a "smoke-vessel." In April, 1873, he had the brothers Grandy as his guests at Ambriz, and supplied them with beads and goods for the arduous undertaking assigned to them by the Royal Geographical Society, of endeavouring to discover the sources of the River Congo, and of aiding Livingstone should he cross the continent and make for the West Coast. Mr. Monteiro accompanied the brothers Grandy five days inland. He explored the Congo as far as Porto da Lenho, in a steamer belonging to a Dutch house at the mouth of the river; and it was while on this expedition that he met by appointment, and at their desire, nine kings of Boma, whose curiosity he greatly excited by being the owner, as they said, of the first white woman, his wife, they had ever seen, and from her hand the kings were greatly pleased to receive a "dash" or present.

Mr. Monteiro was honoured with the friendship of Dr. Livingstone, who strongly desired him to accompany his expedition as mineralogist, but this wish he could not accede to, owing to his engagements in working out the fibre-scheme on the West Coast. His researches in the natural history of Angola have been of great importance to science. Among the many botanical specimens which he forwarded to England may be mentioned the plant and flowers of *Welwitschia mirabilis*, from which Sir Joseph Hooker was enabled to compile his splendid monograph of this extraordinary plant; besides many parasites, orchids, &c., which have been named after him. Perhaps the most interesting animal discovered by him was the beautiful little lemur (*Galago monteiri*), and the well-known chimpanzee, "Joe," which lived so long in the Zoological Gardens, was also brought to England by him. His second collection of birds was described by Dr. Hartlaub in 1865, and contained many new species, the most interesting of which were a Hornbill (*Tockus monteiri*) and a Bustard (*Otis picturata*), while he also procured a living specimen of the splendid Plantain-eater (*Corythaix livingstonii*) discovered by Dr. Livingstone in the Zambesi country.

Mr. Monteiro's eighth, and, as it has unfortunately proved, his last, visit to Africa, was one to Delagoa Bay, and here he expired, after a severe illness, on the 6th of January last. In company with his wife, who contributed so largely to his natural history collections, at which she worked with equal courage and zeal, he had set himself to develop the mineral and natural products of that Portuguese possession, and had already sent to England many valuable specimens, when his untimely death put an end

to all his projects. There can be no doubt that Angola, to the elucidation of the natural history of which Mr. Monteiro contributed so largely, still presents a fine field for the collector, and it is to be hoped that some one will be found who will continue the researches so well instituted by the deceased traveller.

SOUND COLOUR-FIGURES

THE great interest excited by Prof. Bell's telephone and Mr. Eddison's phonograph, in which an elastic disc or membrane faithfully takes up the highly complex vibrations due to sounds of the human voice, has directed renewed attention to the optical methods hitherto employed in studying the motion of resonant media. These have, in important instances, been based on observations of the secondary effects produced by sonorously vibrating bodies. Thus Chladni watched the behaviour of sand strewn upon sounding plates and membranes; König that of gas flames acted on by aërial vibrations. The present article describes an analogous method depending on the colours reflected from slightly viscous liquid films when thrown into sonorous vibration.

The ordinary phenomena called the "colours of thin plates" are sufficiently well known, but a short description of them, taken from a standard work on Physical Optics, may still not be out of place here as a reminder.

"If the mouth of a wine-glass be dipped in water, which has been rendered somewhat viscid by the mixture of soap, the aqueous film which remains in contact with it after emersion will display the whole succession of these phenomena. When held in a vertical plane, it will at first appear uniformly white over its entire surface; but, as it grows thinner by the descent of the fluid particles, colours begin to be exhibited at the top, where it is thinnest. These colours arrange themselves in horizontal bands, and become more and more brilliant as the thickness diminishes; until finally, when the thickness is reduced to a certain limit, the upper part of the film becomes completely black. When the bubble has arrived at this stage of tenuity, cohesion is no longer able to resist the other forces which are acting on its particles, and it bursts."—(Lloyd's "Wave-Theory of Light," p. 100.)

If the film, instead of remaining at rest, is thrown into sonorous vibration, totally distinct colour-phenomena instantly present themselves. A rough idea of their general character may be obtained without the aid of any apparatus as follows. While washing the hands, after getting a good lather, a film can easily be formed between the thumb and forefinger of one hand held in a horizontal plane; the other hand supplies an extemporised tube through which a note can be sung, and so vibrations caused to impinge on the *lower surface* of the film.

If this is done the reflected colours will be seen to be in regular motion, and, in particular, a number of small eddies of colour will be observed whirling about fixed centres of rotation. Steady coloured bands may also be sometimes recognised, but with much greater difficulty.

Fixed bands and stationary vortices form, in fact, the constituent elements of all the sound colour-figures obtainable by film-reflection.

In order to study these in detail a specially arranged apparatus is, of course, requisite. I have found the following give excellent results.

An L-shaped cylindrical brass tube is permanently fixed upon a wooden stand, with its two limbs vertical and horizontal. The vertical limb terminates in a narrow flat circular ring. The open orifice of the horizontal limb is fitted into a caoutchouc tube of equal bore, ending in a trumpet-shaped mouth-piece. For the purpose of supporting the films operated on, I use a series of metallic discs pierced with apertures of various shapes and sizes. On covering one of these, by means of a camel-hair brush, with some weak solution of soap,[1] a film of considerable durability will be formed upon it. The disc should first be held in a vertical plane until the coloured bands have begun to show themselves, and then laid gently upon the horizontal ring prepared for its reception. The observer places himself so as to get a good view of the assemblage of colours reflected by the film, and the instrument[2] is ready for use. Sounds of tuning-forks, whistles, organ-pipes, &c., or notes of the human voice have only to be produced near its mouthpiece, in order that their vibrations may be conducted to the film, and the resulting phenomena observed.

The forms thus presented are of endless variety and great beauty. They almost invariably include both motionless curvilinear bands of colour very regularly disposed, and also a system of colour-vortices revolving about fixed nuclei. The contrast between the steady and moving portions of the figures is always very striking, and the effects of changing tint which accompany the progressive thinning of the film gorgeous in the extreme. When the moment of its dissolution is close at hand, patches of inky blackness invade the field, until at last there is sometimes nothing left but an ebony background, with here and there a few scraps of light, either at rest or still flying round their former orbits, the remnants of fixed bands and whirling vortices.

That the results obtainable by the mode of experimenting above described are likely to present a practically endless variety of form, will be at once obvious from an enumeration of the several causes which may influence the assemblage of colours reflected at a given instant from a given film acted on by the vibrations of a given sound. These are:—1. The shape of the film; 2. Its size; 3. Its consistency; 4. The intensity of the sound; 5. Its pitch; 6. Its quality; 7. The direction in which the sound-vibrations take place with reference to the plane of the film.

It thus appears that each colour-figure observed *may* be a function of not less than seven[3] independent variables; and on experiment this proves to be the fact. An alteration made in any one of these elements, while all the rest are kept constant, produces a corresponding change in the appearances observed. The intensity of the sound does not, it is true, affect the form of the figure, but controls the rate of its vortical motion; the louder the sound the more rapid the rotation of the colour-whirls. All the other elements act directly on form.

It is evident from what has preceded that an attempt at anything like a general classification of sound colour-figures would afford materials for a considerable volume. All that can be done within the present narrow limits is to draw attention to a few points of special interest.

Dependence of Form on Pitch.—This is perhaps most distinctly shown by alternately stroking with a resined bow two mounted tuning-forks of different pitch, the open ends of whose resonance-boxes are placed close to the mouthpiece of the Phoneidoscope. As long as the same aperture is used, and the film kept at one degree of consistency by frequent renewal, each note will instantly call forth its own colour-figure for any number of alternations. This mode of experimenting has the advantage of giving perfectly steady and sharply defined figures. But the successive alterations of form due to changing pitch are more interestingly shown by singing[4] the diatonic or chromatic scale, on some single vowel, into the Phoneidoscope. The complete change of figure consequent on

[1] Castile soap, I find, answers extremely well.

[2] It is manufactured and sold under the title of the "Phoneidoscope," by S. C. Tisley and Co., Philosophical Instrument Makers, 172, Brompton Road, S.W.

[3] A reader of Helmholtz will see that I might have added an eighth element by taking into account differences of phase among partial tones, which, though inoperative on quality, directly affect mode of resultant vibration.

[4] A pitch-pipe with a sliding piston may be substituted for the voice in this experiment.

perhaps but a semitone's alteration of pitch, is often most surprising. It was these sudden kaleidoscopic bounds from one form to another which suggested the name given to the observing instrument. In general the complexity of the figure increases with the acuteness of the exciting sound. With low notes a comparatively simple arrangement of a few rings and pairs of vortices occupies the film. As the pitch rises, the separate parts of the figure diminish in size and increase in number, so that the whole field is covered with a regular pattern which is constantly growing more and more minute. With very shrill sounds the pattern can only be made out by using a magnifying-glass.

Effects of Quality.—These are easily observed by employing unison organ-pipes of different *timbres*, *e.g.*, treble C's belonging to stopped and open diapasons, claribella, and hautbois, respectively. By sounding them consecutively in the above order, figures rapidly increasing in complexity are obtained.

Prominent among differences of quality are those which distinguish vowel-sounds of the human voice sung successively on one and the same note. Marked corresponding differences of colour-figure are recognisable in many instances, but I have not at present succeeded in extending the observation to *all* the European vowel-sounds.

Effects due to Direction of Vibration.—The best mode of observing these is to strike a tuning-fork, and hold it with one of its prongs *close* to the surface of the film.

By moving the fork it is easy to show that both the axis of symmetry, and to some extent also the form, of the colour-figure thus produced, are dependent on the position of the fork with respect to the film, and therefore on the direction in which the exciting vibrations impinge upon it. The steady bands of a figure obtained by this method shift to and fro upon the film in obedience to the fork's movements, almost as though under a magnetic influence resident in its prongs.

Resultant Figures due to Combined Sounds.—If the sounds of two tuning-forks are separated by a considerable interval of pitch, say an octave, they will generate, when alternately applied to the same film, very different figures. When both are applied together there results a figure different from either of those due to each fork by itself. It is in fact a compromise between the two. In order to convince himself of this the experimentor should first get the forms of the component figures well into his memory by repeatedly producing them, and then watch the effect, *on some one band in either figure*, of mixing the two sounds in various degrees of relative intensity. Let us suppose that fork 1 produces figure 1, and fork 2 figure 2, respectively, and that a band in figure 1 is selected for observation. Then if fork 1 be struck sharply, and fork 2 weakly, the band will alter its form so as to exhibit a slight approach to the arrangement in the corresponding part of figure 2. As the note of fork 2 is more loudly sounded this approach will be more decided. If fork 2 is made preponderant the result will be the arrangement of Fig. 2 with some modification towards that of figure 1. The same thing holds good for the rotating portions of the figures. Complex colour-flows are seen to result from a compromise between simpler component vortices.

Effect of Beats.—When two sounds of very nearly the same pitch coexist, slow fluctuations of intensity called "beats" are known to be produced. If a film is exposed to the simultaneous action of two sounds so related, the fixed parts of the resulting figure take up a swaying motion about their mean position, each complete oscillation synchronising exactly with one entire beat. The vortices show, in general, an increased speed of rotation during one half of each beat, and a diminished speed during the other half. But in particular cases a bolt forward every alternate half-beat seems to be followed by intermediate quiescence, or the direction of motion may be actually reversed, so that a vortex rotates positively during one half-beat and negatively during the next.

Representation of Dissonance.—When the beats become too rapid for separate recognition, and coalesce into the effect which we call discord, the colour-figure presents a tremulous appearance, like that shown by the tip of a singing gas flame. Prof. Helmholtz has remarked how unpleasant is the impression which a flickering light makes upon the eye, and pointed out its analogy to the effect of rapidly intermittent sounds on the ear. In the present experiment, acoustical and optical dissonance are exhibited in a direct and interesting connection.

As the phenomena described in the above article admit of such facile reproduction in all their beauty of form and splendour of hue, I have thought it needless to attempt illustration by diagrams, which could convey but an inadequate notion of the former, and none at all of the latter. SEDLEY TAYLOR

Trinity College, Cambridge, March 6

REFLECTION OF LIGHT[1]

PLACE the heliostat in position, and bring a slender beam of light into the darkened room. Then get a small looking-glass, or hand-mirror, and a carpenter's steel square, or a sheet of stiff paper, having perfectly square corners. Hold the mirror in the beam of light. At once you see there are two beams of sunlight, one from the heliostat and another from the mirror. Hold the glass toward the heliostat, and you will see this second beam going back toward the window.

This is certainly a curious matter. Our beam of light enters the room, strikes the mirror, and then we appear to have another. It is the same beam, thrown back from

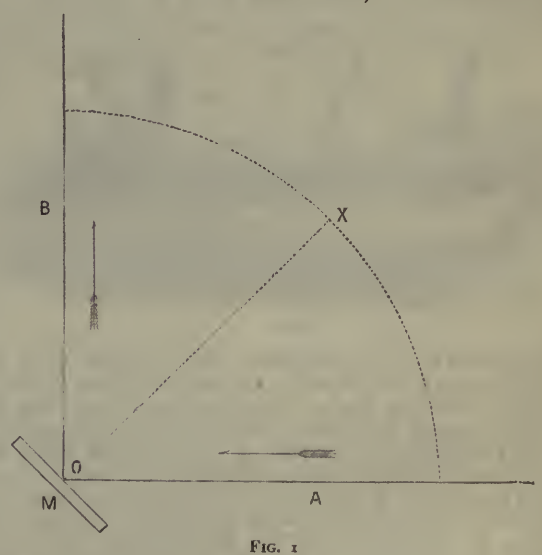


FIG. 1

the glass. This turning back of a beam of light we call the reflection of light.

Place a table opposite the heliostat, and place the mirror upon it, against some books. Turn the mirror to the right, and the second or reflected beam of light moves round to the right. Turn the glass still more, and the beam of light will turn off at a right angle, and there will be a spot of light on the wall at that side of the room. Now bring the carpenter's square or the piece of square paper close to the mirror, so that the point or corner will touch the glass just where the sunlight falls upon it. Now

[1] From a forthcoming volume of the "Nature Series"—"Light: a Series of Simple, Entertaining, and Inexpensive Experiments in the Phenomena of Light, for the Use of Students of Every Age," by Alfred M. Mayer and Charles Barnard.

one edge of the square is brightly lighted by the sunbeam, and if the mirror is placed at an angle of forty-five degrees with the sunbeam, the other edge of the square is lighted up by the second beam.

In Fig. 1, A is the beam of light from the heliostat, and B is the beam reflected from the mirror, that is marked M. To make this more simple, we call the first beam the beam of incidence, and we say that it travels in the direction of incidence, as shown by the arrow. The second beam, marked A, we call the beam of reflection, and the course it takes we call the direction of reflection. The point marked O, where the light strikes the mirror, is called the point of incidence.

In the diagram is a dotted line representing a quarter of a circle reaching from the beam of incidence to the beam of reflection. A quarter of a circle, as you know, is divided into ninety degrees. Another dotted line extends from O at the mirror to X on the quarter-circle, and divides it into two parts. Half of ninety is forty-five, and hence the mirror stands at an angle of forty-five degrees with both beams of light. Now the line A and the dotted line reaching from O to X make the angle of incidence, and the angle between B and the line from O to X is the angle of reflection; and the curious part of this matter is, that these two angles are always equal. Here they are both angles of forty-five degrees.

Move the mirror about in any direction, and measure the angles of incidence and the angles of reflection, and these angles will always be exactly equal.

If you look at the diagram you will see that the mirror is at an angle of 45 degrees with the beam of incidence, and that the beam of reflection is at an angle of ninety

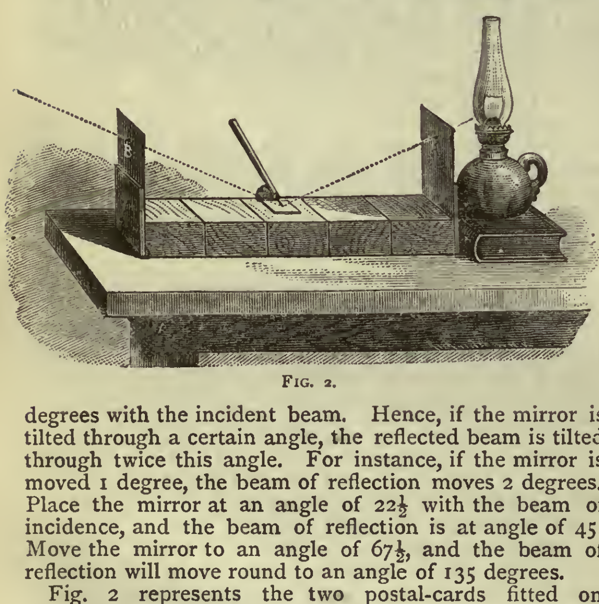


FIG. 2.

degrees with the incident beam. Hence, if the mirror is tilted through a certain angle, the reflected beam is tilted through twice this angle. For instance, if the mirror is moved 1 degree, the beam of reflection moves 2 degrees. Place the mirror at an angle of 22½ with the beam of incidence, and the beam of reflection is at angle of 45. Move the mirror to an angle of 67½, and the beam of reflection will move round to an angle of 135 degrees.

Fig. 2 represents the two postal-cards fitted on blocks of wood that we used in a former experiment, and the three blocks of wood we cut out at that time. The five blocks are placed close together in a line, and with the postal-cards at the ends. A lighted lamp is placed near one of the cards, and on the middle block is a small piece of window-glass that has been painted with black varnish. A single coat of black varnish on one side of the glass is all that is required to give us the black mirror needed in this experiment. Place the lamp close to the card in such a position that the flame will be just on a level with the hole in the card. If the lamp is not convenient the blocks and cards may be placed upon a table facing a north window in full daylight.

When everything is ready look through in the postal-card marked B, down upon the black mirror, and on it you will see a single spot of light, the reflection from the lamplight or the light from the window shining through the hole marked A in the drawing. Get the needle-pointed awl and place it so that the point will just touch the spot of light in the black mirror, and then fasten the awl in this position with a piece of wax, as represented in the picture.

You will readily see that this experiment is the same as the last. Again we have a beam of light reflected from a mirror. The beam of incidence passes through the postal-card at A and finds its point of incidence on the mirror, and the beam of reflection extends from the point of incidence to the second card at B.

Take a sheet of stiff paper 10 inches (25·4 centimetres) long, and about 4 inches (10 centimetres) wide, and hold it upright between the two cards, with the bottom resting on the mirror. With a pencil make a mark on the edge of this at the point of incidence marked by the awl, and at the hole in the card where the beam of incidence enters, and marked A in the drawing. Draw a line between these two points and you have an angle formed by this line and the base of the paper. This angle marks the angle of incidence. Put the paper on the blocks with the ruled line toward the card B, and you will find that the line fits here equally well. It now extends from the point of incidence to B, and proves that this angle is the same as the other, that both sides are alike, and that the angle of incidence and the angle of reflection are equal.

Take out the block in the middle and move the others nearer together till they touch. Repeat the experiment: make a measurement with a piece of paper as before, and draw a line on it from the point of incidence to either of the holes on the cards, and then compare the angles thus found, and in each case they will be exactly alike. Take out another block and try it again, and you will reach the same result.

These experiments show us that there is a fixed law in this matter, and the more we study it the more we are convinced that it has no exceptions.

Experiment in Multiple Reflection

Choose a south room on a sunny day and close the blinds and shutters at all the windows save one, and at this window draw down the curtain until only a narrow space is left at the bottom. Close this space with a strip of thick wrapping-paper, and then cover the rest of the window with a blanket or shawl so as to make the room perfectly dark. Then cut a round hole the size of a five-cent piece in this paper, and through this hole a slender beam of sunlight will fall into the darkened room.

Bring a hand-mirror into this beam of light and the beam of reflection will make a round spot of sunlight on the wall above the window. This spot of light is a picture of the sun thrown by the mirror upon the wall. Hold the mirror at an oblique angle in the sunbeam and direct the beam of reflection upon the opposite wall. Now there are several reflections, brilliant spots of light. If the spots of light do not stand out sharp and clear, turn the mirror slowly round and you will soon find a position for the glass that will give six or more reflections.

How does it happen that a common looking-glass can thus split a single sunbeam into several beams? If you touch a pencil to a mirror you will notice that while the point of the pencil touches the glass the point of the reflected pencil seen in the mirror does not meet the point of the real pencil, and that there is a little space between them. The reflection we see in the glass is from the smooth surface of the quicksilver at the back of the glass, and the space between the reflection and the pencil is filled by the glass.

Hold a sheet of common window-glass before a lighted lamp or candle, and you will see a faint reflection of the flame in the glass, and at the same time you can readily see through the glass. This shows us that the outside

of any piece of smooth glass will reflect light, and our experiment is designed to show a still more curious matter.

Fig. 3 represents the single beam reaching the point of incidence on the outside of the mirror at O, and reflected to the wall at 1. Part of the light goes through the glass to B, and here is another point of incidence, and a new beam of reflection is thrown through the glass to the wall at 2. If you look at the reflections on the wall, you will see that the second spot of light is the brightest. This comes from the quicksilver, for, as this is a better reflector than the glass, it sends out a brighter beam of reflection. When this second beam of reflection passes through the glass, a part of its light is reflected from the under side of the surface, and is turned back

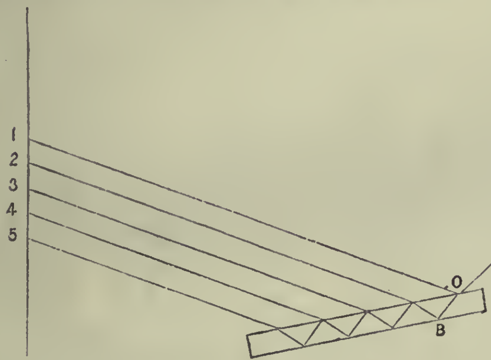


FIG. 3.

against the quicksilver again. Once more it is reflected, and a new beam of reflection makes number 3. The drawing shows the path these beams of light take in the glass, and the quivering spots of light on the wall show how one beam of light may be reflected again and again in different directions. If the reflector was perfect and returned all the light, these multiple reflections might be repeated many times over; but every time light is reflected from any bright surface a part of the light is lost, and thus each reflection grows fainter and fainter till the light is spent. Look at the multiplied reflections on the wall, and you will see that the first reflection from the glass is bright, and that the second, from the quicksilver at the back of the glass, is brighter still; and that the others grow fainter and fainter till all the light is spent, and the reflections disappear.

Second Experiment in Multiple Reflection

Light a lamp and place it on a table, and get the two postal-cards and the blocks that we used in the experiment in reflection. With a sharp knife cut a slit in one card, just at the pin-hole, about $\frac{3}{4}$ inch (19 millimetres) long and $\frac{1}{25}$ inch (1 millimetre) wide. Then place this card close to the lamp, as in the other experiment, and set up the other card about fifteen inches away from it. Then lay a looking-glass on the table between the two. Look at Fig. 2, and arrange the cards as there represented, and put the mirror in place of the blackened glass on the blocks. On looking through the small hole in the postal card (marked B in the drawing), you will see in the mirror several bars of yellow light, placed one over the other. Again we have an instance of multiplied reflection. Instead of seeing the reflections thrown upon the wall, we can look down upon them and see them, just us they stand, each at its point of incidence on the glass and the quicksilver. Study these brilliant bars of light, examine the diagram carefully, and you will readily see that this experiment simply exhibits in a different manner the same thing that we saw in the last experiment.

Experiment with Mirror on Pulse

Get a small bit of looking-glass, about an inch (25 millimetres) square, and some wax. Warm the wax in the hand till it is soft, and then make three small pellets about the size of a pea. Put one of these on the back of the little mirror, near the edge and half-way between two corners. Place one at each of the opposite corners, so that the mirror will have three legs or supports placed in a triangle. Put the heliostat in place, and bring a small beam of sunlight into the dark room. If this is not convenient any beam of sunlight in a dark room (as in former experiments) will answer.

Turn back your coat-sleeve, and, while standing near the beam of light, place the little mirror on the wrist, with one of the wax legs resting on the pulse. Then bring the arm into the beam, so that the light will fall on the mirror. Hold the arm steady, and watch the spot of reflected light thrown upon the wall. See! It moves backward and forward with a curious, jerking motion. It is like the ticking of a clock, or like the bending of one's pulse. It is the motion of your pulse. The mirror moves with the pulse, and the beam of reflection thrown on the wall moves with it, and, though this movement is very slight, the reflection on the wall moves over a space of several inches, and we can see it plainly. In our first experiment in reflection we learned that when a mirror was moved to the right or left, the beam of light reflected from it moved also to the right or left, and each time through twice as great an angle as the mirror.

This experiment is a wonderfully interesting one, and may be tried with a number of boys or girls, and each may see the peculiar beating of his or her pulse pictured on the wall in the most singular and startling manner. If any of the persons whose pulse-beats are thus exhibited get excited, laugh at the exhibition, or are in any way disturbed, the change in the movement of their pulse will be quickly repeated on the wall, where a hundred people can see it.

Experiment with Glass Tube

Procure a glass tube, about $\frac{3}{4}$ inch (19 millimetres) in diameter and 12 inches (30·5 centimetres) long, and paint the outside with black varnish. If this is not convenient, cover the tube with thick black cloth, and fasten it down with mucilage, taking care to have the cloth square at the ends. Punch a hole in a postal-card with the sharp point of a pair of scissors, and with a knife make the

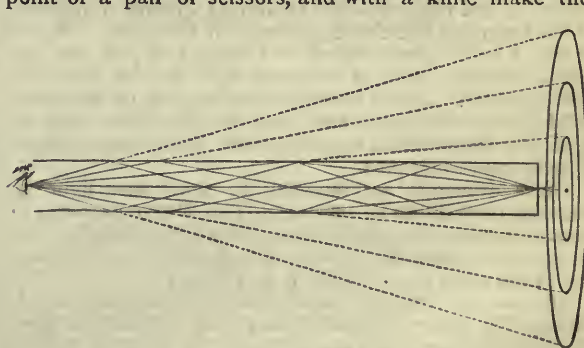


FIG. 4.

ragged edges of the hole smooth. Hold the card at one end of the tube so that the hole will come just at the centre of the opening, and then, while facing a window or a bright lamp, look through the tube with one eye, and you will see a spot of light surrounded by a number of beautiful rings.

Here we have another example of multiplied reflection. The light entering the tube through the hole in the card falls on the smooth surface of the interior of the tube, and appears to the eye in the form of rings.

Fig. 4 represents a section of the tube, and shows the paths the different rays of light take, and shows how each is reflected from side to side till they all meet in the eye. The dotted lines and the rings projected beyond the tube show how they appear to the eye. By studying this drawing carefully, and trying cross cuts and slits in the card in place of the single hole, you will get a very correct idea of repeated reflection, and find the tube a source of considerable amusement.

Experiments in Dispersed Reflection

Get a small piece of black velvet or cloth and take it to a dark room where the heliostat will give us a slender beam of sunlight. If this is not convenient use a common beam of sunlight in a dark room, as in some of our former experiments. Hold the velvet in the hand between the fingers, and so as to leave the palm of the hand clear. Turn back the coat-sleeve so as to expose part of the white cuff, and then bring the velvet into the beam of sunlight. You will observe nothing in particular, for the black rough cloth does not reflect the light at all. Now move the hand so that the spot of light will fall on the palm. See what a pretty rosy glow of light falls on the wall! This is the reflected light from the hand. The skin is rough, and the light is diffused and scattered about, and instead of a bright spot of reflected light, as with a mirror, we have this glow spread all about on the wall and furniture. Now move your hand so that the sunlight falls on your cuff. Immediately there is a bright light shining on the wall and lighting the room with a pale bluish-white glare. Move the hand quickly so that the black cloth, the hand, and the white cuff will pass in succession the beam of light. Observe how the different things reflect the light in different degrees. The cuff is the smoothest and whitest, and gives the brightest reflection; the hand gives less light because it is less smooth; and the cloth, that has a very dark and rough surface, gives no reflection at all, and the spot of sunlight falling upon it seems dull and faint.

This experiment shows us something more in the reflection of light. A piece of glass, the surface of water, polished metals, ice, and all substances having very smooth surfaces, reflect light in one direction. The linen cuff also reflected light, but apparently in a very different manner from the mirrors we have been using.

Place a lighted lamp upon a table and lay a mirror before it, and you can see a clear and distinct reflection of the lamp and the flame pictured on the glass. Put a sheet of white paper before the lamp, and you can see only a confused spot of reflected light on the brightly-lighted paper. Lay a freshly-ironed napkin or handkerchief before the lamp, and even the indistinct spot of light has disappeared, and the white cloth reflects light equally from every part.

These drawings are intended to show how light is reflected from different surfaces. The first represents a smooth surface, like glass, that sends all the beams in one direction, because the points of reflection for the beam are in the same plane. (See 1, 2, 3, Fig. 5.)

The second drawing represents a slightly-roughened surface, like paper. Some of the points of reflection turn the light one way, some another, and the beam of reflection is no longer formed of parallel rays. They are scattered about, and the image they form is confused and indistinct. In the third drawing we have a rough surface, like cloth, and here the rays of the beam of reflection are scattered in every direction, and we can see no image.

It is in this manner that we are enabled to see the people and things about us. The light of the sun or a lamp falls upon them, and is reflected into our eyes, and we say we see the objects. Very few things reflect light so brightly that we obtain from them a reflected image of the source of the light, and we generally see only dispersed and scattered light, that does not blind or dazzle the eye, and enables us to look upon these objects with ease, and to readily see all their parts.

The clouds, the water, the grass, rocks, the ground, buildings, the walls inside, clothing and furniture, and everything we can see, reflect light in every direction again and again, and thus it is that all spaces, without and within, are filled with light so long as the sun shines. At night the sun sinks out of sight, and still it is light for

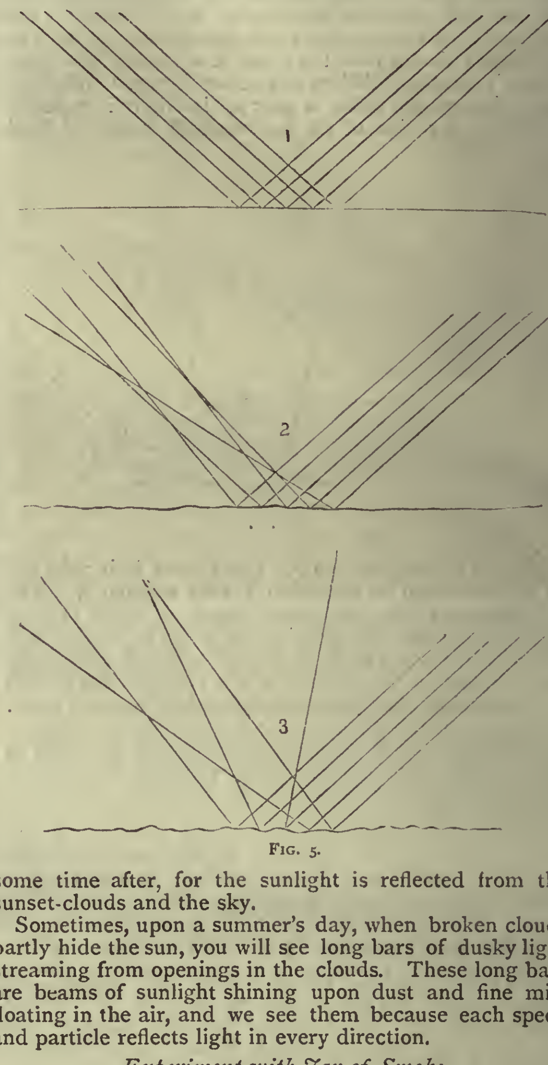


Fig. 5.

some time after, for the sunlight is reflected from the sunset-clouds and the sky.

Sometimes, upon a summer's day, when broken clouds partly hide the sun, you will see long bars of dusky light streaming from openings in the clouds. These long bars are beams of sunlight shining upon dust and fine mist floating in the air, and we see them because each speck and particle reflects light in every direction.

Experiment with Jar of Smoke

Fig. 6 represents a large, clean glass jar, such as one sees at the confectioner's. It is standing upon a black cloth laid upon a table in a dark room, and on top of the mouth is laid a postal-card, having a slit, one inch (25 millimetres) long, and $\frac{1}{25}$ inch (1 millimetre) wide, cut in it. Above the jar is a hand-mirror, so placed that the beam of sunlight from the heliostat (or from a hole in the curtain) will be reflected downward upon the postal-card on top of the jar.

This simple apparatus is designed to show how light is reflected from small particles floating in the air. Set fire to a small bit of paper and drop it into the jar. Place your hand over the mouth of the jar, and in a moment it will be filled with smoke. When the paper has burned out, put the postal-card in place, so that the slit will be in the centre of the mouth of the jar. Let the beam of reflected light from the mirror fall on this slit.

Look in the jar and you will see a slender ribbon of light extending downward through the jar. Elsewhere it is quite dark and black. Here we see the light streaming through the opening in the card, and lighting up the particles of smoke in its path.

Take off the card, and let the reflected beam fall freely into the jar. The smoke is now wholly illuminated, and the jar appears to be full of light, and every part of the bottle shines with a pale-white glow.

FIG. 6.

Put the postal-card on again and let the light fall through the slit. The smoke has nearly all disappeared, and the ribbon of light in the jar is quite dim. Curious streaks and patches of inky blackness run through it. What is this? Nothing—simply nothing. The smoke is melting away, and the beam of light disappears because there is nothing to reflect it and make it visible.

This part of the experiment appears quite magical in its effects, and is exceedingly interesting.

The Milk-and-Water Lamp

Take away the jar and put a clear glass tumbler in its place. Fill this with water and throw the beam of reflected light down upon it, and the water will be lighted up so that we can easily see the tumbler in the dark. Now add a teaspoonful of milk to the water and stir them together. Throw the beam of light down once more. This is indeed remarkable. The tumbler of milk-and-water shines like a lamp, and lights up the room so that we can easily see to read by its strange white light. Move the mirror and turn aside the beam of light, and instantly the room becomes dark. Turn the light back again, and once more the glass is full of light.

Here the minute particles of milk floating in the water catch and reflect the light in every direction, so that the entire goblet seems filled with it, and the room is lighted up by the strange reflections that shine through the glass.

AMERICAN GEOLOGICAL SURVEYS

MISSOURI

THE State of Missouri boasts of abundant mineral wealth. Its seams of coal and its stores of iron and lead mark it out as one of the great centres of the future industry of the United States. Such a country might have been supposed only too anxious to have its mineral formations accurately mapped, so as to know exactly where and how its subterranean resources lie. Yet the history of its official action in this matter is by no means a gratifying one. As far back as the year 1849 a memorial was presented to the General Assembly of the State, praying for the formation of a Geological Survey, with liberal appropriations for constructing maps and publishing reports; for investigating causes affecting health, the agricultural capacities of different soils, the water system, and the rocks and minerals of the country. It was not until the early summer of 1852 that the State geologist, who, in response to this memorial, was appointed, began operations. Five annual reports, consisting for the most part of only a few leaves, appeared up to the year 1861, and, with one exception, contained mere statements of progress. Perhaps the Legislature began to think that the results obtained were not worth the expenditure to secure them. At all events, in 1861 the Survey was disbanded. The authorities, however, seem to have been unwilling that the fruits of the long years of work of their geological staff should be lost; they accordingly arranged to have them published, but finally abandoned this idea on account of the expense. For nine years nothing further appears to have been done in the matter. At last, in 1870, the Legislature once more roused itself to consider the expediency of having the country properly explored and mapped. A "Mining, Metallurgical, and Geological Bureau" was now created, and a new State geologist was appointed. This arrangement, however, not proving satisfactory, the act was amended next year, but soon thereafter the State geologist resigned, and Mr. R. Pumpelly took his place. The body by which the geological work of the State was controlled, now called the "Bureau of Geology and Mines," consisted of a board of five managers, with a staff formed of a State geologist, an assistant palæontologist and geologist, an assistant chemist, and such additional assistance as might be possible within the limits of an annual appropriation of 10,000 dollars.

By the spring of 1872 a more liberal spirit had appeared in the assembly. An additional chemical assistant was allowed, and the annual vote was raised to 20,000 dollars. The Survey now set to work with prodigious vigour. Mr. Pumpelly and his associates undertook an extensive exploration of the iron and coal districts, while the chemists were busy analysing the minerals sent into them from the field. By the end of the year a large mass of information had been collected, and as the liberality of the Legislature had shown no sign of waning, a large appropriation was asked for the publication of the results obtained in 1872, and another grant for the issue of the still unprinted reports of previous years. Both these appropriations, amounting to 9,000 dollars in the one case, and 3,000 dollars in the second, were voted. Accordingly two volumes duly appeared next year. The Report for 1872 was sumptuously printed and illustrated. Moreover, it was accompanied by a monstrous atlas of chromo-lithograph maps and sections. Some parts of the coal-fields were carefully illustrated by sections to show the structure of the areas and the relative positions of the seams in different districts. Perhaps some of these sections were on a needlessly large scale. Certainly the whole atlas was issued in a style so luxurious as to suggest that the Legislature must not only have become more liberal, but must be anxious to atone for former delinquencies by an almost extravagant expenditure in print and paper.

But this golden age was not destined to last. Mr. Pumpelly resigned, very shortly after the appearance of his meritorious though costly volumes. His successor, Mr. G. C. Broadhead, who had previously acted as chief assistant-geologist, found the fund at his disposal so depleted by the heavy expenses of the winter and spring of 1873, that he had to reduce his field-staff. The Board of

Management likewise determined that the cost of the Annual Reports should in future be paid out of the yearly appropriation, thereby of course, considerably narrowing the possible amount of work to be done in the field. In spite of these drawbacks, however, the State-geologist succeeded, during his first year of office, in doing some useful work, and yet kept a sufficient balance to publish a bulky report with a quarto atlas of plates. His plan was to attack first of all those branches of inquiry which presented the greatest interest or had the closest bearing upon the industrial resources of the State. The ground was surveyed by counties, Mr. Broadhead himself taking a lion's share of the hard work. The two lead regions of South-west and Central Missouri were likewise examined. Many analyses were also made of the ores, slags, coals, and other mineral substances sent up to the office. The Report which gave an account of these labours cannot fail to be of great service in the development of the mineral resources of the State. Mr. Broadhead is evidently exactly the kind of director needed to keep the Missouri Geological Survey in full activity and to satisfy the demands of a utilitarian legislature.

The oldest rocks in Missouri appear to be certain granites and other crystalline masses, on which lie somewhere about 3,000 feet of Lower Silurian strata, including representatives of the Potsdam, Black River, Birdseye, Trenton, and Cincinnati groups of other parts of the United States. Upper Silurian rocks are much more feebly represented, but Dr. Shumard has recognised beds probably equivalent to the lower Helderberg and Niagara groups. The Devonian groups of Hamilton and Onondago are still more sparingly developed, only about 100 feet of strata being referable to those horizons. The Carboniferous system, however, is well displayed, and contains the following groups :—

Lower.	Upper coal-measures (poor in coal)	1,307 feet.
	Middle ,, (with 7 ft. of coal)	324 ,,
	Lower ,, (with 13 ft. 6 in. of coal) ...	250–300 ,,
Upper.	Chester group (sandstone) from a few feet to ...	100 ,,
	St. Louis ,, (limestone), maximum	250 ,,
	Keokuk ,, (shale and chert), perhaps exceeding	200 ,,
	Encrinital or Burlington group	60 ,,
	Chouteau limestone	100 ,,
	Vermicular sandstone and shales	75 ,,
	Lithographic limestone	55 ,,

No later formations occur until we reach the "Drift. This consists of two divisions ; the lower, formed of dark blue clay, overlaid and interstratified with beds and pockets of sand sometimes inclosing remains of terrestrial vegetation ; the upper composed of stiff, tenacious, brown, drab, and blue clays, often mottled, and containing rounded granitic pebbles. Large boulders of crystalline rocks from a northern source occur in the lower division, up even to a height of 1,050 feet above the level of the Gulf of Mexico. Most of the observed boulders occur in the valleys. They diminish in numbers and size as they are traced southwards, the Missouri River seeming to limit their extension in that direction. Above these clays lies the "bluff," or loess, a very fine light brown siliceous marl, with occasional concretions of limestone. With sufficient consistency to weather out into perpendicular escarpments, this deposit forms a belt of hilly country receding ten miles from the river, and then changing into a stiff clay which may be part of the "drift." The low alluvial lands lie on what is termed the "bottom prairie," generally a dark tenacious clay, often containing concretions of bog-iron, and rarely beds of sand.

From the early part of last century lead and iron have been worked in Missouri. The mining industry of the State has gradually developed, and is now making rapid progress. In the year 1872, 13,550,135 pounds of lead were produced in the State. During the first six months of 1874, 5,050 tons of pig-lead were sent by railway into St. Louis. The yield of iron and zinc is likewise steadily increasing. Vast quantities of sulphate of baryta are said to be raised, and to be used in the improvement (that is, the adulteration) of white lead. A territory so richly stored with mineral wealth ought to be able to equip and maintain a sufficient staff for the thorough exploration of the geological and mineralogical structure of the ground, and for the formation of a museum where the rocks, minerals, fossils, and manufactured mineral products may be displayed, and made practically useful and instructive. ARCH. GEIKIE

OUR ASTRONOMICAL COLUMN

DUN ECHT OBSERVATORY PUBLICATIONS, VOL. II.—In this handsomely-printed volume of two hundred pages we have the first portion of results of observations made during Lord Lindsay's expedition to the Mauritius on the occasion of the late transit of Venus, an expedition which for the care and forethought bestowed upon the arrangements and the excellence and completeness of the equipment, compares favourably with any of those fitted out by the various Governments which took part in the observation of this rare phenomenon.

It was upon the strong recommendation of the eminent Secretary of the German Transit of Venus Commission, Prof. Auwers, that Lord Lindsay was induced to take out a heliometer, and an instrument of this class, similar to those intended to be used in the Russian expeditions, was ordered in the spring of 1872 and completed in due time by the joint exertions of Messrs. Repsold, of Hamburg, and Messrs. Cooke and Sons of York. In the investigation of the constants of the instrument previous to the expedition, experience was obtained of the great precision to be attained in the measurement of angular distance between two stars by its means, and this experience led to a determination to take advantage of a near opposition of the minor planet Juno, occurring during the anticipated period of residence at the Mauritius, to investigate the solar parallax, from the diurnal parallax of the planet, by measuring its distance and angle of position with respect to a star, both morning and evening. On November 4 Juno in perigee was distant 1·029, and though the parallactic displacement in such case is considerably less than in a transit of Venus, or an opposition of Mars, it was believed that the great accuracy attained in measures with the heliometer would more than compensate for this disadvantage.

Vol. ii. of the publications of Lord Lindsay's Observatory is devoted to the discussion of the observations of Juno, preceded by a very detailed account of the instrument and its adjustments and of the methods adopted in determining its instrumental errors, as errors of scale divisions and errors of screw and of the method of observation and calculation of instrumental results. And in the event of criticism of any of the processes it must be stated that the whole of the work is so presented as to admit of future discussion, with any modification of plan that may be deemed advisable. It was originally intended that the observations should commence on October 10 and continue to the end of November. Circumstances, however, prevented so long a series of measures ; Lord Lindsay's yacht with the instruments did not arrive at the Mauritius until November 2, and it was not till November 10 that the first heliometric observations could be made. The first reliable series was obtained two evenings later, and from this time to November 30, observations were secured on twelve evenings and eleven mornings, some of them not being so complete as was desirable. It will thus be seen that Juno was past opposition before work could be commenced, and this first attempt to determine the solar parallax, through measuring the diurnal parallax of a minor planet with the aid of the heliometer, was consequently made under less favourable conditions than may

be secured in future investigations of the same kind; nevertheless, it is certain that Lord Lindsay and Mr. Gill have been amply justified by the result in the confidence they placed upon the proposed methods of observation, and have proved that one means of determining the solar parallax, admitting comparatively of very frequent repetition, is comparable in point of accuracy with methods involving far greater difficulty and expense and chance of failure. In the correction of the equations of condition for errors in the tabular places of Juno, derived from observations at Greenwich, Washington, and Cambridge, U.S., it was found desirable to work upon two systems, the probabilities being rather in favour of the second. The definitive result for the mean solar parallax is 8″·77, according to the first system, and 8″·76 according to the second. To these values and their probable errors (± 0″·04) the authors do not attach high importance, indeed, a discordant value from observations on November 15 being included, they say, "if we were asked what we believe to be the most probable value resulting from the determination, we should reject this result; the values then become 8″·82 – first system; and 8″·81 – second system. At the same time we are aware that the rejection of any observation is quite unsound." In a longer series, however, it is probable, as they observe, that the single discordant value would have been counterbalanced by another.

So far as we know, this is the first application of the heliometer to observation in the southern hemisphere. We think it must be generally conceded by astronomers that Lord Lindsay and Mr. Gill have rendered an important scientific service in this introduction of the most accurate of measuring instruments in the investigation of the sun's distance, by a method admitting of such repeated confirmation. Three of the minor planets approach the earth in the present year within the distance at which Juno was observed at the Mauritius in 1874.

THE SATELLITES OF MARS.—Prof. Asaph Hall, to whom, as the discoverer of these bodies, the right of selection of names appertains has definitively decided for *Deimus* for the outer moon and *Phobus* for the inner one, agreeably as he mentions to the suggestion of Mr. Madan in these columns, founded on the lines in the "Iliad," which Pope thus renders:—

> "With that he gives command to *Fear* and *Flight*,
> To join his rapid coursers for the fight;
> Then grim in arms, with hasty vengeance flies,
> Arms that reflect a radiance through the skies."

THE DATE OF EASTER.—Easter Sunday falling on April 21, is considered late this year, and it is thirteen days after the mean date, but it is to be remarked that in no year since the introduction of the Gregorian calendar into England has the festival occurred on the latest possible date, April 25, though in two years, 1761 and 1818, it fell on March 22, which is the other limit. In 1886, Easter Sunday will fall on April 25, in the new or Gregorian style, for the first time since the year 1734, or eighteen years before this style was accepted in England. The only other occasion since the reformation of the Calendar by Pope Gregory XIII., upon which Easter has fallen on the latest possible date was in 1666, and after 1886 this will not again occur till 1943.

BIOLOGICAL NOTES

THE AGRICULTURAL ANTS OF TEXAS.—Mr. H. C. McCook has presented to the Academy of Natural Sciences of Philadelphia a memoir on the habits of these most curious and interesting ants (*Myrmica molefaciens*, Buckley = *M. barbata*, Smith). An abstract of the memoir will be found in Sheet 20 of the *Proceedings* of the above Academy (p. 299). The author encamped in the midst of a large number of the ant hills during the summer of 1877, and carefully studied the habits of the inmates; the spot selected was in the neighbourhood of Austin, Texas, upon the tableland to the south-west of the Colorado River and its affluent, Barton Creek. The limestone rock here and there cropped up, the soil was black and tenacious, varying in depth from a few inches to three feet. The formicaries were very numerous, and were to be found along roads, in open fields, and in the very streets, paths, gardens, and yards of Austin; indeed, one was even seen in the stone-paved courtyard of an hotel. They are commonly flat circular clearings, hard and smooth; a few have low mounds in the centre, composed of bits of gravel of one or two grains' weight; the clearings vary in width from twelve to two or three feet. From each, roads three to seven in number, diverge into the surrounding herbage. These are often of great length, and during the working hours are thronged by the ants going and returning. The ants take their siesta during the meridian heat of the sun, generally stopping work about twelve, and not returning to it until two or three o'clock. The seeds collected were always taken from off the ground, they were chiefly seeds of small Euphorbiaceous and Rubiaceous plants, and of grasses. The ants proved to be true harvesters. The seeds were carried into the granaries through the central gates. They were shelled, and the hulls were carried out and deposited in refuse heaps, which, when carefully searched, yielded no perfect fruits. They seemed to be most fond of the grass called *Aristida stricta*, and it even seems possible that they sow this for themselves, though the author does not commit himself to this as a fact. The interior economy of the ant-hill is fully described. Here it may be noted that the ants are clever in attack, that their "sting" is as bad as a wasp's, and that they are so well versed in the science of war, that they would have been more than a match for Mr. McCook, had he not himself employed a small army (of two men) to fight with those ants that would fight with him while he was pulling their granaries, their nurseries, and their queen's palace to pieces, in order to let us know all about them. Prof. Leidy made some remarks on this paper, adding that he had studied the habits of an allied species (*M. occidentalis*) which he had met with during a summer in the Rocky Mountains. The habits of this species were very like those of the species described by Mr. McCook, but in addition Prof. Leidy mentioned that his species fostered a fine large Coccus for its saccharine production.

THE FIRST STAGES OF DEVELOPMENT IN PLANTS.—Great interest attaches to the earliest changes occurring after the fertilisation of the germinal cell or oösphere in plants; and the difficulty of the subject has taxed the ability of the best histological botanists. To satisfy the doctrine of evolution many students think it necessary to be able to trace homologies in the development of all stem-bearing plants. The latest investigation, which appears to carry the comparison further than has yet been attempted, is that of Mr. S. H. Vines, of Cambridge, who has diligently sought out and compared all the embryological evidence, derived from the writings of Hofmeister, Hanstein, Fleischer, Mettenius, Pringsheim, and many others. He shows that in all stem-bearing plants the germinal cell (that which is fertilised) divides into two portions, one of which gives rise to an embryonic tissue called suspensor, in higher forms, while the remainder alone produces the true embryo. This comparison is of especial interest in relation to mosses. In these plants it is the spore-capsule which is the product of the fertilisation of the germ-cell, and it is this capsule which corresponds to the whole leafy plant of a fern. Following out the analogy, the seta or stalk of the capsule in a moss corresponds with the part called "foot" in an embryo fern, and with the suspensor in flowering plants. Mr. Vines's paper is contained in the

January number of the *Quarterly Journal of Microscopical Science.*

RHIZOPODS IN AN APPLE TREE.—Freshwater rhizopods are beginning to be well known, but Prof. Leidy has lately discovered a number in an apple-tree. While waiting for a railway train, last December, his attention was attracted to a large-apple tree which had then quite recently been thrown down by a storm, and from the fork of its trunk he collected a small bunch of moss, which, on examining it carefully, he found to contain a number of rhizopods. Of these one was *Difflugia cassis*; it was abundant. Another, which occurred in smaller number, was *D. globularis*, and in addition, some specimens of *Trinema acinus*, *Euglypha alveolata*, and *E. brunnea*, were met with. The moss from which they were washed with filtered water was found at a distance of about eight feet from the ground (*Proceedings*, Acad. Nat. Scien. Philadelphia, 1877, p. 321). We hope this hint will not be lost by the investigators of our British or Irish rhizopods.

THE AERONAUTIC FLIGHT OF SPIDERS.—Many observations have been made on this singular phenomenon, but the Rev. H. C. McCook is pursuing his inquiries with a perseverance that succeeds in detecting many new details in the performance. Recently (October, 1877) he paid attention to groups of young wolf-spiders (Lycosidæ), which crowded the tops of railings in a meadow. Their faces were turned in the direction from which the wind was blowing; the abdomen in each was elevated at an angle of 45°, the claws brought in, and the legs stiffened, thus raising the body. From the spinnerets at the apex of the abdomen a single thread was exuded, and rapidly drawn out to several feet by the breeze. Gradually the foremost pair of legs sank to the level of the post, and the entire attitude became that of intense resistance. Then suddenly and simultaneously the eight claws were unloosed, and the spider mounted with a sharp bound into the air, and went careering across the meadow. As far as could be observed, it appeared that the spider took a voluntary leap at the moment of loosing its hold. One spider, by good hap, was followed through its flight. The position of the body was soon reversed, the head being turned in the same direction as the wind. The legs were spread out, and were united at the claws by delicate filaments of silk. After flying a distance of about eighty feet, the spider gradually settled down upon the meadow. The difficulty of this observation will be understood by entomologists, for it required exact suitability of position as to light, the limitation of the flight to a moderate height, and a comparative moderation of its speed. (*Proc.*, Acad. Nat. Sci. Philadelphia, 1877, p. 308.)

TURKOMAN GREYHOUNDS.—The Jardin d'Acclimatation has lately been enriched (we learn from *La Nature*) with three Turkoman greyhounds of great beauty, the first specimens imported into Europe. The animals are known in the country under the name of Tazi, and are employed in catching hares, like the Sloughi in Algeria and the greyhounds in Persia. They are of noble aspect, and have great strength of muscle; their head is remarkably long and delicate in form. The hair on the body is short; but the ears (which are very large) are covered with long silken hair. Their legs are also covered with well-developed hair, and the contrast of this with the upper smooth part of the body is surprising at first sight; the dogs appearing as if they had large waving pantaloons, or reminding one of some kinds of fowl. One of the three dogs was obtained from the Kirghises of Emba, the two others at Samarkand (and by M. de Ujfalvy). We believe that it is among this breed that, as mentioned by Hamilton Smith, the *stop* greyhound is found so trained, that when a whole pack of them is in pursuit of a doubling hare, a stick thrown before it instantly produces a general halt, and one only is then signalled out to pursue the game.

GEOGRAPHICAL NOTES

CHINA.—Mr. E. C. Baber's long-deferred Report on the journey of the Grosvenor Mission through Western Yünnan, from Tali-fu to Têng-yüeh, contains much matter which is of interest from more than one point of view. The most important of his surveys is that of the route from Tali-fu to Têng-yüeh, as it connects Garnier's explorations with the work of Sladen's expedition, and thus puts Bhamo in topographical communication with Shanghai and Saigon. The survey next, but not much inferior, in importance, is the route from Yünnan-fu to Tali-fu, in which the track followed was different to Garnier's. Mr. Baber has also prepared a running survey of his route across China from Hankow to Têng-yüeh. His remarks on the native races are interesting, especially in regard to the Kutung people. What or where Kutung is he was unable to ascertain; he describes the men as of a dark reddish complexion, with rather prominent features, above the average height and well-proportioned, dressed in close-fitting woollen garments, which in some cases were neatly cut and handsomely embroidered. The women seen would have been considered handsome anywhere; paler in colour than the men, their oval intelligent faces reminded the observer of the so-called Caucasian type, and in every step and movement there was a decision and exactness very different from the motion of a Chinese. One of the women, too, was particularly remarkable for a peculiarity of her long hair, which was naturally wavy, a feature never met with among the Chinese. Mr. Baber was fortunate in seeing the quarterly fair at Tali-fu, at which some 5,000 people were present, many of them being Lolos, Shans, Thibetans, &c. At this stage of his journey he propounds a not improbable explanation of the term "golden teeth," as applied to the inhabitants, viz., that it arose from the discoloration of the teeth produced by chewing betel with lime. Mr. Baber's observations on the extent of the poppy cultivation will hardly be found encouraging by those who desire to see the consumption of opium put an end to, for he says that his party walked some hundreds of miles through poppies; and a similar remark applies to his account of the trade-route into Yünnan from Burmah. The valleys, or rather abysses, he says, of the Salwen and Mekong must long remain insuperable difficulties, not to mention other obstacles between Yünnan-fu and Têng-yüeh. The members of Col. Sladen's expedition appear to have assumed that, when the latter place is reached, the obstacles to a highway into Yünnan have been surmounted, whereas the fact is that the difficulties begin at that place. Loth as most Englishmen are to admit it, Mr. Baber adds, the simple and evident approach to Eastern Yünnan is from the Gulf of Tonquin, but it by no means follows that the same holds true of the western part of the province. In conclusion we may mention that an interesting feature in Mr. Baber's report is his comparison of Marco Polo's narrative with his own experiences, and his verification in many respects of the Venetian's information respecting a country almost entirely unknown to Europeans.

PRJWALSKY'S JOURNEY TO LOB-NOR.—In the *Isvestia* of the Russian Geographical Society, and as Supplement 53 to Petermann's *Mittheilungen*, the narrative of Prjwalsky's journey from Kuldja to Lob-Nor and the Altyn-Dagh, is now published, with maps showing the route and the discoveries made. We have already referred to the results of this important journey between August, 1876, and July, 1877, a journey which the enthusiastic Dr. Petermann regards as the crown of Central Asiatic exploration, and as equal in importance to Stanley's journey down the Congo, or even the attainment of the Pole. Prjwalsky gives ample details as to what he saw along the route, and his observations will be of special value to the ethnologist as containing important

details concerning the various peoples he met with. The zoologist and botanist will also find much to interest them. Not only does he bring certain information on the Lob-Nor, which is little better than a marsh, apparently drying up, but also makes an important contribution to our knowledge of the great mountain plateau which separates India from Central Asia. The Lob-Nor basin forms the foot of the Kuen-luen and of the great plateau which stretches from the plains of India over the Himalayas, the Karakorum, the highlands of Khor, in an unbroken sweep to the basin mentioned. Close by the Lob-Nor this mountain rises like a wall out of the low plain, some of the lowest valleys having a height of 10,000 feet above the sea. From this northern slope on the Lob-Nor, at about 60° W. long., the plateau stretches away south, for 13° (850 miles) to its southern slope on the Indian plain. At the meeting, on February 20, of the Russian Geographical Society the Secretary read a letter from Col. Prjwalsky, dated Fort Zaisan, January 11. The traveller said that, after having seen the impossibility of penetrating into Tibet *viâ* Lob-Nor, he was compelled to try the indirect route *viâ* Guchen and Hami, whence he proposed to go south to Tsandam and to Hlassa, crossing the sources of the Blue River. Thus, he left Kuldja on September 9, and reached Guchen. As along the whole of the route to Guchen, which passes through the towns Shikho and Manas, there were Chinese troops, as also many *champans* (convicts condemned to hard labour), Col. Prjwalsky followed another route, viz., to Lake Ebi-nor, thence north to the Saur Mountains, and thence to Guchen, along the route followed in 1875 by Col. Sosnovsky. Thus, he reached Guchen about the beginning of November, but here a serious illness compelled him to return to Zaisan, which he reached on January 13. A later telegram announced that the indefatigable traveller had recovered and that he was again on his way to Tibet.

MONGOLIA AND SIBERIA.—At the same meeting a letter from the traveller Potanin dated Bjisk, January 14, stating that he had arrived at the end of his Mongolian journey, after obtaining many hypsometrical and topographical data, as well as making rich botanical, zoological, and mineralogical collections. The Secretary of the Society gave a review of the activity of the Siberian department. Two expeditions were sent out by this department during the past year, one, which will be absent for several years, under the leadership of M. Czerski to investigate the shores of Lake Baikal geologically, the other conducted by M. Agapidin, to examine the flora of the district of Balagansk in the government of Irkutsk.

NEW GUINEA.—The Rev. S. Macfarlane has just sent home a report of a voyage which he made towards the close of last year from Murray Island to the east end of New Guinea, and in the course of which he visited several places previously unknown. He mentions having gone on shore near Killerton Point, not far from East Cape, where he found himself unable to communicate with the people except by signs, for they could not understand any of the dialects spoken at Teste Island, Port Moresby, and Murray Island, nor the Eastern and Western Polynesian languages, though upon inquiring the names of things, Mr. Macfarlane's companion thought he detected a resemblance to the Raratongan. The locality visited not suiting their purposes, the party went six or seven miles further to the eastward, and landed at the mouth of a river or mountain stream, where the hills slope down to within a short distance of the beach, and behind the village there is a well-wooded, fertile, and lovely valley. Mr. Macfarlane describes the neighbourhood as thickly populated, though the people are scattered in small villages within hailing distance of each other. Speaking generally, he says that the country about the east-end of New Guinea has a totally different appearance from that in the vicinity of Port Moresby, and the contrast was very striking. The former looked lovely and luxuriant, like the South Sea Islands, whilst the latter had a barren, brown, parched appearance, as if two days' sail had brought the party into a new country in quite a different latitude. We hear that Mr. Andrew Goldie, to whose gold discoveries in New Guinea we have before alluded, has sent home to the Earl of Glasgow an account of his recent explorations, accompanied by a sketch map and several drawings. At Mr. Goldie's request, his Lordship has handed the papers to the Geographical Society, and they will probably be read at one of the meetings during the present session.

LAKE NYASSA REGION.—A paper was read at Monday' meeting of the Royal Geographical Society, by Mr. H. B. Cotterill, "On the Nyassa, and a Journey from the North-East to Zanzibar." In August last he met Capt. Elton and some friends at the south end of the lake, and ran up the west coast. They were detained some days under Mount Chombi, which he ascended, and found to be about 4,000 feet above the lake. The high land on the west of the lake was found to trend off in a north-westerly direction. They at last made a start with about fifty men. Their route crossed the Chombaka River. The whole of the country was covered with groves of banana They procured other carriers and crossed the Chombaka Valley, crossing the river several times and passing two very beautiful little lakes. In crossing the Chombaka for the third time at a point where it flowed through a very deep ravine, they struck more towards the north. They found stretching away to the east and south-east a great plain bounded in the far distance by a towering range of mountains that evidently ran up from the eastern side of the Nyassa towards the north-west. The native name for these mountains and the surrounding country is Kondi. They had been gradually ascending since they left Nyassa, and when they reached Mazote's, they were at an elevation of about 6,000 feet above the sea. It was decided that some of them should push on to Mereri's Town. So Capt Elton and he and another started off, and having crossed the Kondi Range, they found themselves on a great plateau, 7,000 feet high, called Uwanji, a splendid cattle country, watered by many streams. Crossing the Makesumbi River, they found themselves in an undulating coun ry, covered with thick bush. There Capt. Elton began to break down, and at South Ushekhe breathed his last. They then had to traverse some 350 miles of the Ujiji caravan route, and on the last day of February reached Zanzibar.

INDO-CHINA.—Dr. G. Barrion, a French naval surgeon is about to undertake an exploring journey to the Indo-Chinese peninsula.

MR. STANLEY has announced to the Paris Geographical Society that he will visit Paris in June, before his departure for America, to receive the medal the Society has awarded him.

NOTES

ROBERT JULIUS V. MAYER, whose name is so intimately associated with the mechanical theory of heat, died at his native town, Heilbronn, on the 21st inst., in his sixty-fourth year. We can only intimate the event this week, but hope next week to be able to speak in detail of Mayer's life and work.

IN connection with our article on Harvey in this number, we may remind our readers that for some time a movement has been on foot for the erection of a statue to Harvey in his native town, Folkestone. Only 800*l.*, half the sum requisite, has been obtained, and we are sure many of our readers, on being made aware of the deficiency, will be glad to help to fill it up. Donations may be sent to the hon. treasurers of the fund, Sir George Burrows and Mr. Prescott Hewett, or to the hon. secretary, Mr. George Eastes, M.B., 69, Connaught Street, Hyde Park Square, W.

A great banquet, under the auspices of the College of Physicians, to be held on the day of the Harveian oration, is also talked of, but judging by the apathy shown generally on the subject of Harvey's tercentenary, it is not very probable it will come off. How is it that we take so little trouble here to keep alive the memory of our great dead?

WE notice the death of Prof. A. Lamy at Paris on the 20th inst. For a number of years he has occupied the Chair of Industrial Chemistry at the École Centrale. As an investigator his name is chiefly known in connection with the metal thallium. Very shortly after the detection of its spectrum by Mr. Crookes in 1861, he observed the same phenomenon in the lead works at Lille; and his isolation of the metal and descriptions of its properties followed so closely on the announcements of the English chemist that the question of priority was vigorously discussed for some time, until finally decided against him. Contemporaneously with Mr. Crookes he submitted the new element to a careful examination, and it is to him we owe the first determination of the atomic weight 204, the discoveries of the poisonous properties, of the close relations with the alkaline group, of the remarkable thallium alcohols, and the preparation of thallium glass. In 1869 Lamy invented the two valuable pyrometers associated with his name, the one based on the dissociation-tension of calcium-carbonate for temperatures above 800°, and the second containing instead of carbonate the compound $CaCl_2, 8NH_3$ for temperatures below 42°. In physics he studied the electric properties of sodium and potassium, and was the first to produce induction currents by means of terrestrial magnetism.

THE death is announced of Michel-Charles Durieu de Maissonneuve, on February 20, aged eighty-two. He was honorary director of the Gardens of Bordeaux. As member of the Scientific Commission of Algeria he was known to botanists for his researches in the flora of that country.

WE regret to announce the death of Prof. Gustav Willmanns of Strassburg University, well known through his African explorations and discoveries. Prof. Willmanns was only thirty-two years of age.

IN the course of a few weeks a festival will be held in the city of Liége, to celebrate the fortieth year of the professorship of Theodore Schwann, the author of the cell-theory. To some of our readers it will be a startling piece of intelligence that the founder of modern histology is actually at this moment alive, and teaching as Professor of Physiology in the Belgian University. The committee charged with the management of the celebration desire the co-operation of scientific bodies and of individuals in this country. We are authorised to draw the attention of officials of the learned societies and other corporations to the approaching event, and to beg them to obtain some expression of sympathy with the object of the celebration—viz., the doing homage to the genius of Theodore Schwann. It is requested that letters intended to be read at the celebration may be forwarded either direct to the secretary, Prof. Edouard van Beneden, Liége, or to Mr. Ray Lankester, Exeter College, Oxford. All Englishmen of science who have specially occupied themselves in the field of work opened up by Schwann, are begged to communicate individually with either of the above-named gentlemen, and to forward their photographs for insertion in an album which is to be presented to the founder of the cell-theory.

M. RAOUL PICTET, at Geneva, in consideration of the importance of his discoveries with regard to the liquefaction of gases, has had the honorary title of Doctor of Medicine conferred upon him by the University of Jena.

IT was stated at the last meeting of the Royal Dublin Society that a new explosive agent has been discovered by Prof. Emerson Reynolds, in the Laboratory of Trinity College, Dublin. It is a mixture of 75 per cent. of chlorate of potassium with 25 per cent. of a body called sulphurea. It is a white powder, which is very easily prepared by the mixture of the materials in the above-named proportions. The new powder can be ignited at a rather lower temperature than ordinary gunpowder, while the effects it produces are even more remarkable than those caused by the usual mixture. Dr. Reynolds states that his powder leaves only 45 per cent. of solid residue, whereas common gunpowder leaves about 57 per cent. It had been used with success in small cannon, but its discoverer considered that its chief use would be for blasting, for shells, for torpedoes and for similar purposes. Dr. Reynolds pointed out that one of the advantages this powder possesses is that it can be produced at a moment's notice by a comparatively rough mixture of the materials, which can be stored and carried without risk so long as they are separate. The sulphurea, the chief component of the new explosive, was discovered by Dr. Reynolds about ten years ago, and could be easily procured in large quantities from a product of gas manufacture which is at present wasted.

THE annual meeting in London of the Iron and Steel Institute commenced yesterday, and will be continued to-day and to-morrow. Discussions will take place on papers read at the Newcastle meeting, and several papers will be read on subjects of technical interest.

THE great forge of Creusot has just despatched for an Italian ironclad two steel plates, weighing respectively 23,000 and 31,000 kilogrammes. They required a special railway train constructed for the purpose. The recent experiments at Spezia show that vessels protected by these plates are absolutely imperforable by any missiles so far known.

THE rare phenomenon of St. Elmo's fire was observed at several localities in the Harz Mountains during the past month. At Blankenburg it occurred at a temperature of + 0°·5 C. and pressure of 721·5 mm., after a series of storms. The air was so laden with electricity, that canes held aloft emitted from their points light blue flames five inches in length and three in breadth. In Döblitz the phenomenon occurred in the midst of a storm, half snow and half rain, when the ends of the branches in an entire grove were surmounted by flames from four to five inches in length.

A THEORY of the chemical action of light recently propounded by M. Chastaing is controverted by M. Vogel (in the reports of the German Chemical Society), who cites various facts to show that rays of any kind are capable of producing either an oxidising or a reducing action on inorganic substances, according to the nature of the substance by which they are absorbed; there is no ground for attributing to the less refrangible rays in all cases an oxidising, and to the more refrangible a reducing, power. M. Chastaing's second proposition, that light has an oxidising action on organic substances, which is strongest in the violet and weakest in the red, is also opposed by M. Vogel.

CAPE COLONY, New Guinea, the Australian Colonies, the South Seas, and, it would appear, almost every known portion of the southern hemisphere, have been suffering from a severe and protracted drought. Shade temperatures of 124° and 127° are reported from the interior of Australia, the heat being much less intense near the coast, owing to the strong sea-breezes which prevail in connection with the great heat of the interior. Sheep, cattle, horses, and the wild animals of these regions are dying off in thousands. In Cape Colony, in particular, complete ruin has overtaken large numbers of the settlers, many of the homes of hitherto well-to-do colonists having been broken up, and the several members gone into menial service in exchange for the barest necessaries of life. We have received several letters on this subject already, and shall be glad if our readers in the regions named will favour us with any information of which they may be in possession, suggesting or disproving the cyclical character of these droughts.

A Paris correspondent sends us the following:—On March 15 a parricide was guillotined at Evreux (Eure), and a fraternity for burying the dead existing in the place, the body was not, as usual, thrown into a large basket and sent hurriedly to its grave. An ordinary coffin was prepared, and as soon as the execution was completed the corpse was laid in it. To the horror of the spectators the body was seen to be agitated by spasms so powerful that it almost jumped twice out of the coffin, and it was necessary to use force in order to control its motions. These contractions were, of course, unaccompanied by consciousness.

In connection with the lamentable catastrophe to the *Eurydice* Sir George Airy sends to the *Daily News* some valuable information as to the meteorological condition on Sunday:—On Sunday, March 24, between 1h. 30m. and 3h. 0m., the wind, which had previously been almost imperceptible, had four times risen to a pressure of $1\frac{1}{2}$ lb. per square foot; but from 3h. 0m. to about 3h. 55m. it was nearly calm, the pressure scarcely exceeding $\frac{1}{4}$ lb. per square foot. During the former of these two periods the direction of the wind had been fluctuating on both sides of west, but during the latter it was for the most part west-south-west. At 3h. 56m. nearly the direction changed very suddenly to north-north-west, and the force changed with most unusual suddenness to 4 lb., from which it rose at 4h. 3m. to 9 lb. per square foot. It declined for a time, but rose at 4h. 40m. to 10 lb. It fell and rose once more, and finally sank at 5h. 30m. to almost perfect calm. The fluctuations of the barometer were very inconsiderable. At 2h. 30m. it stood at 29·35 inches; at 3h. 56m. it was 29·28; and at 5h. 30m. was again 29·33. The temperature about 2h. 0m. had been as high as 49°, diminishing with fluctuations (probably produced by clouds) to 45° just before the squall. With the squall it sank most rapidly to 38°, and continued to fall, till at 5h. 0m. it was about 32°."

From an inquiry on the electromotive force and internal resistance of some thermopiles, those of Noë, and of Clamond, modified by Koch, M. Beetz concludes (*Ann. der Phys.*, No. 1) that the latter, from its great solidity, is preferable for technical purposes. That it requires to be heated long before use is of little consequence, and once in action, it works on with great constancy, both as regards electromotive force and resistance. Though, with an equal number of elements, the electromotive force is under that of the Noë pile, its utility is not less, as the elements can be easily increased. Only the burner must be improved in construction. On the other hand, the Noë pile offers the great advantage for laboratory purposes, that (by coupling several cylindrical piles) a productive current-source is readily obtained, with very constant electromotive force; the duration has been considerably improved in the new construction.

According to *La Nature* the telephone is finding great favour in Spain; a goodly number are being produced in Barcelona, and numerous applications made of them. Telephonic chambers are being constructed designed to isolate the hearer from external noises and render communication more easy and sure. These chambers are of small size and have glass windows for light; the doors are closed with pads of caoutchouc. Telephony was lately tried between Barcelona and Saragossa, which are about 364 kilometres apart. The communication was satisfactory at the former place (notwithstanding bad weather); at Saragossa it was somewhat imperfect, which is accounted for by the telephonic chamber having been used at one place but not at the other.

As the latest instance of collections of personal contributions to scientific literature, we notice the appearance in Paris of a handsome volume containing Prof. Kuhlmann's various researches during the past half-century. The work affords not only an interesting glimpse into the lines of investigation followed out by a single mind, but also into the general progress of applied chemistry since 1830; for there is probably no chemist alive who has done more for the practical application of his science than this Lille professor. The present volume contains detailed accounts of the baryta industry, which he created, of the general introduction of crystallisation into technical operations, of the phenomena accompanying the use of cements and the formation of stone, as well as the minute studies on the formation of nitrates and artificial manures, on the crystallisation of insoluble bodies, on the madder dyes, as well as a great variety of other technical and purely scientific subjects. Prof. Kuhlmann is now in his seventy-fifth year, but is still able to contribute occasionally the results of new investigations.

Under the editorship of Heinrich and Gerhard Rohlfs, assisted by a numerous staff, Hirschfeld, of Leipzig, is publishing a new quarterly journal under the title *Deutches Archiv für Geschichte der Medizin und medizinische Geographie.*

The French Academy has published the seventh edition of its "Dictionnaire de la Langue Française." Scientific terms have not been admitted into the general vocabulary except such as are now in common use and cannot be ignored even by unscientific persons.

Mr. Stab, corresponding member of the Society of Arts, at Smyrna, reports that the plague of field-mice, or rats, has again broken loose, and that they are wasting the fields far and wide, digging up the seed-corn, and devouring all they can. This is the plague from which Homer records that Apollo Smynthius delivered the Greeks. As the Smynthian Apollo no longer has believers, Mr. Stab wishes to know what remedy can be recommended. The western states of America suffer much from this pest.

Dr. A. B. Meyer, of the Royal Zoological Museum, Dresden, writes, in answer to Mr. Boulger's inquiry (vol. xvii. p. 392), that the reason why Mr. W. W. Wood did not send specimens of Navicula (Nature, vol. xii. p. 514) is, that he died a short time after he wrote that letter. Dr. Meyer heard this from a Manila friend.

A Botanical Exchange Society has been established at Buda-Pesth for the purpose of exchanging specimens of the native plants of Hungary, Transylvania, Croatia, Sclavonia, and, as far as possible, of Turkey and Russia, for those of other parts of the world. During the last two years upwards of 300 botanists have joined the Association, and more than 120,000 specimens have been distributed. All communications and applications for further information should be addressed to Herr Richter Lajos, Erzherzogin Marie Valerie Gasse, Nro. 1, Buda-Pesth, Hungary, accompanied by a subscription of 4 marks, or 5 francs, for which sum an exchange of 100 specimens will be effected.

A lecture will be delivered in the Theatre of the Royal Engineer Institute, Chatham, at five p.m., on April 3, by Prof. Huxley, F.R.S., on "The Geographical Distribution of Animals; and on Collecting and Observing in Aid of the Investigation of the Problems connected therewith."

Electric lights are becoming very common in Paris. The Lontain system is now working daily at the Lyons railway terminus at the expense of the Company. M. Jamin has published an elaborate article on the subject in the last number of the *Revue des Deux Mondes.*

The telephonic signal invented by MM. Henry Brothers was exhibited at a lecture delivered at Montrouge under the auspices of the Paris municipal authorities. One apparatus was placed at the Mansion House and another at the Public School, at a distance of 500 metres. When each apparatus was used as a

signal giver and connected with an ordinary telephone as receiver an air played at one end of the line could be heard by the whole audience at the other end. The Henry signal is constructed to work with a dry element, and requires no other wires than those of the telephone.

THE *Midland Naturalist* continues to keep up the promise of its first number. No. 3, for March, has the first part of a lecture by Dr. Cobbold on the Parasites of Man, and among other interesting papers we may note those of Mr. Robert Garner on Edward Forbes and his Country, and the Ray and Palæontographical Societies: An Appeal, by Mr. W. R. Hughes.

Photographic Rays of Light is the somewhat unhandy title of a new photographic quarterly published in Baltimore, U.S.A. The contents are varied, and the journal seems likely to prove useful to photographers. The first number contains a photographic plate, "A Study in Artistic Photography."

IN the February session of the Berlin Anthropologische Gesellschaft, Dr. Rahl-Rückhard delivered an elaborate address on the anthropology and ethnology of South Tyrol, a subject which has hitherto been untouched. This region has been swept over by so many tidal waves of invasion that the character of the original inhabitants has hitherto been entirely unknown. In order to solve the problem a large collection of skulls was obtained from an ancient charnel-house at Meran, and submitted to careful measurements. The results showed that they belonged to two sharply-defined classes. The first, a brachycephalic type, was evidently identical with that of the ancient Rhætians who formed the aboriginal population at the advent of the Teutonic tribes. The second variety, an orthocephalic type with dolichocephalic tendencies, cannot easily be classified. It is, however, certain that it does not coincide with the cranial type of the ancient Helvetians in the neighbouring parts of Switzerland.

AT the workshops of the Michigan Central Railway at Jackson, Michigan, an interesting experiment was recently made, in order to ascertain the very shortest time in which a locomotive engine could be mounted ready for use from the finished component parts. Up to the present this work had been generally done by about five or six workmen in the space of from nine to fourteen days. When the fact became known that a Mr. Stewart of Jackson had done the work with fourteen workmen in twenty-five hours, and a Mr. Edington, with the same number of workmen, in 16½ hours, a bet between these two gentlemen was the result; and before a number of spectators they eventually both proceeded to mount a locomotive engine, each being assisted by fourteen workmen, and having all the parts of which the engine consists ready at hand. They accomplished the task in the remarkably short period of two hours and fifty-five minutes. The bet was won by Mr. Edington, who finished one minute sooner than his antagonist.

THE additions to the Zoological Society's Gardens during the past week include a Rhesus Monkey (*Macacus erythræus*) from India, presented by Mrs. Baxter; a Green Monkey (*Cercopithecus callitrichus*) from West Africa, a Vervet Monkey (*Cercopithecus lalandii*) from South Africa, presented by Mr. Jas. Bennett; a Malayan Bear (*Ursus malayanus*) from Malacca, presented by Mr. S. Palmer; a Short-toed Eagle (*Circaetus gallicus*), South European, presented by Mr. H. M. Upcher; a Savigny's Eagle Owl (*Bubo escalaphus*) from Persia, presented by Dr. J. Huntly; two Reindeer (*Rangifer tarandus*) from Lapland, deposited; a Beccari's Cassowary (*Casuarius beccarii*) from South-East New Guinea, a Plantain Squirrel (*Sciurus plantani*) from Java, a Spotted Eagle Owl (*Bubo maculosus*) from South Africa, a One-streaked Hawk (*Melierax monogrammicus*) from West Africa, two Matamata Terrapins (*Chelys matamata*) from the Upper Amazons, an Anaconda (*Eunectes murinus*) from South America, received in exchange.

MIMICRY IN BIRDS

WE have received two interesting contributions to this subject. One is contained in *The New Moon*, or Crichton Royal Institution Literary Register for November, 1873, being the observation on a starling by Dr. Crichton, the Medical Superintendent.

"Two or three years ago," he states, "in accordance with our principle of encouraging birds to become denizens of the grounds, we put up a few boxes for starlings. One of these was placed on the window sill of the writer's bedroom. Two years ago one of these birds took possession of the aforesaid box. Every morning, for two or three hours, he perched himself on an iron railing, erected to protect flowers, within two feet of the window, and there executed a comic medley with all the precision and effect of a finished *artiste*. The attention of the writer was first called to this extraordinary performance by having his window every morning surrounded by what appeared to be a general assembly of the whole tribe of *Aves*, wild and tame. The quacking of the duck, the screech of the lapwing, the eerie notes of the moorland plover, and many others, were imitated with a precision worthy of Mimos himself. He failed, however, to secure a mate for that year. Last year he was more successful. He revisited in spring his former cottage, and brought a mate with him. The usual family arrangements were made with the greatest care and despatch, when, in due time, a brood of young linguists made their appearance. During the hatching season our linguistic friend, every morning at dawn, resumed his perch within three feet of my bed, and for two or three hours, he repeated his extraordinary performance. The birds imitated always with the greatest precision are the hen, duck, goose, lapwing, plover, heron, and gull. The song or whistle of many small birds are also imitated. The only human note imitated is the whistle of the boy. This is frequently heard. It always begins on the same pitch, and passes downward through a major third, forming a beautiful musical curve. He is gradually adding to his vocabulary. During twelve months he has certainly added the cry of the heron, the gabble of the goose, and the cackle of the hen.

Mr. H. O. Forbes sends us the following instance:—

In the grounds of a friend in the neighbourhood of London, a colony of starlings had for many years built their nests in the trees in boxes placed here and there for their accommodation. The children of the house—all quite young then—a few years ago—at whose presence the birds showed not the slightest alarm, were constantly playing about close to the nests, and of course constantly calling each other by name. There was only one girl in the family, called *Maggie*, and as she was a great pet, perhaps her name was oftener mentioned than those of the others. Be that as it may, her father was one day greatly astonished by hearing his daughter's name pronounced in exact imitation of the voice of one of her brothers, whom he knew could not be near. For a moment he was puzzled, but close at hand, on the bough of an acacia tree, he detected the mocking-bird—a common starling—in the act of deception, which he continued to practice often afterwards.

AMERICAN SCIENCE

THE comparison of the intensities of light of different colours has long been considered one of the most difficult of photometric problems. In the February number of the *American Journal of Science and Arts* Mr. Rood describes a simple method of making this comparison. The luminosity of cardboard painted with vermilion, *e.g.*, was determined thus:—A disc of the cardboard is attached to the axis of a rotation apparatus, and smaller discs of black and white (in sectors) are fixed on the same axis, so that by varying the relative proportions of black and white a series of grays can be produced at will. The compound black and white disc is first arranged to give a gray decidedly darker than the vermilion; this tint is now gradually lightened till the observer becomes doubtful as to the relative luminosities of the red and gray discs; the angle of the white sector is then measured. Next a gray decidedly *more* luminous than the vermilion is compared with it, and diminished in brightness till the observer again becomes doubtful, when a second measurement is taken. (The manipulation is done by an assistant without the experimenter knowing the exact black and white discs chosen.) From a number of such experiments a mean is obtained, which (it is proved) expresses the luminosity very correctly.

In an interesting paper on the glycogenic function of the liver Mr. Leconte, after stating that "the sole object of this function is to prepare food and waste tissue for final elimination by lungs and kidneys; to prepare an easily combustible fuel, liver-sugar, for the generation of vital force and vital heat by combustion, and at the same time a residuum suitable for elimination as urea," points out that the function is not *sugar-making*, as usually supposed, and which is a pure *chemical process* and *descensive* metamorphosis, but *glycogen-making, a vital function*, and *ascensive* metamorphosis. In diabetes the true organ *directly* in fault is not the kidneys nor the lungs, but the liver, which fails to arrest the sugar as glycogen. The starch-making function in plants offers a striking analogy to the function under consideration; for plants change soluble forms of amyloids (dextrin and sugar), into the insoluble form of starch (corresponding to glycogen, which is animal starch), and store it away for future use. This analogy is more remarkable in the lower animals and in embryonic conditions; the function often residing *in all parts* in such cases (as plants), while in higher animals it is confined to the liver. And it is sluggish animals that accumulate most glycogen in their tissues. Plants, however, store away the starch as building materials; animals, as fuel for force-making. Further attention is called to the close relation between the functions of the liver and kidneys. As we descend the animal scale, we find cases (*e.g.* insects) in which the same organ performs both functions. The fact of a large percentage of glycogen being found in the tissues of entozoa, which do not need any internal source of heat, is regarded by Mr. Leconte as a striking proof (if any were still needed) that the prime object of respiration is not *heat-making*, but *force-making*. Heat is only a concomitant, often useful, but sometimes useless, and even distressing.

Mr. Trouvelot, of Cambridge, furnishes accounts of three celestial phenomena observed by him, viz., undulations in the train of Coggia's comet, sudden extinction of the light of a solar protuberance, and the zodiacal light of the moon.

The atomic weight of antimony having been variously given by MM. Schneider, Dexter, and Dumas (using different methods), as 120·3, 122·3, and 122 severally, Mr. Josiah P. Cooke, jun., was led to a fresh study of the subject. The general conclusion which he reaches, after a very patient and laborious investigation (which the chemist will find highly instructive) is that the most probable value is $Sb = 120$ when $S = 32$.

SOCIETIES AND ACADEMIES

London

Linnean Society, March 7.—Dr. Gwyn Jeffreys, F.R.S., vice-president, in the chair.—Mr. Thos. Christy exhibited a series of fruits, among which were Chinese quinces, chayottes, and a remarkable citron known in China as the "claw of Buddha."—Prof. Ray Lankester also brought forward and made remarks on a collection of fossil walrus tusks (*Trichecodon Huxleyi* [?]) from the Suffolk crag, and sent him for examination by Mr. J. E. Taylor, of the Ipswich Museum.—Examples of a variety of *Helix virgata* were likewise shown by Mr. Rich.—On nudibranchiate mollusca from the eastern seas, by Dr. C. Collingwood, was the first paper read. He remarks that residents searching carefully within limited areas have more chance of obtaining new and interesting forms than have zoologists or extensively equipped expeditions who but pay hurried visits to tropical coasts. Season and other influences have much to do with abundance or paucity of species in given localities. He gives curious instances of specimens of nudibranchs, isolated in a dish of sea-water spontaneously and uncommonly neatly amputating the region of their own mouth. With other information the author further describes sixteen new species, illustrating the same with coloured drawings from nature. Mr. Thos. Meehan's paper, on the laws governing the production of seed in *Wistaria sinensis*, was communicated by the Rev. G. Henslow in the absence of the author. The latter alludes to the fact that the Wistaria, when supported, grows amazingly, but is seedless; on the contrary, the self-supporting so-called "tree-wistarias" produce seeds abundantly. These cases illustrate the difference between vegetative and reproductive force; they are not antagonistic, but supplement each other. While Wistaria flowers freely without seeding, it has been supposed this arises from the bees not cross-fertilising. Mr. Meehan submits data, however, in which he thinks the question lies rather in the harmonious relation between the two above nutritive powers than with insect pollenisation.—The Rev. M. J. Berkeley in an examination of the fungi collected during the Arctic Expedition 1875–76, mentions twenty-six species were obtained, all determined save two. Seven are new species, and seventeen already known widely distributed forms. The *Agaricus Feildeni* and *Urnula Hartii* are unusually interesting.—A paper on the development of *Filaria sanguinis hominis*, and on the mosquito considered as a nurse, by Dr. P. Manson, was read by Dr. Cobbold. Discussing general questions, he proceeds to show that the female mosquito, after gorging with human blood, repairs to stagnant water and semi-torpidly digests the blood. Eggs are deposited which float on the water and become the familiar "jumpers" of pools. The filariæ thus enter the human system along with the drinking water. Dr. Manson got a Chinaman whose blood was previously ascertained to abound with filariæ to sleep in a "mosquito house." In the morning the gorged insects were captured and duly examined under the microscope. A drop of blood from the mosquito was thus found to contain 120 filariæ, though a drop from a prick of the man's finger yielded only some thirty. The embryo once taken into the human body by fluid medium pierces the tissues of the alimentary canal. Development and fecundation proceed apace, and finally the filariæ met with in the human blood are discharged in successive and countless swarms, the genetic cycle being thus completed.—Dr. Cobbold, on his own behalf, further contributed a paper on the life history of *Filariæ bancrofti*, as explained by the discoveries of Wucherer, Lewis, Bancroft, Manson, Sonsino, and others.—Mr. Charles C. B. Hobkirk, of Huddersfield, was duly elected a Fellow of the Society.

Chemical Society, March 7.—Dr. Gilbert, vice-president, in the chair.—The following papers were read:—On some new derivatives of anisoïl, by W. H. Perkin. The author has obtained orthovinylanisoïl boiling 195°–200° C., sp. gr. at 15, 1·0095; orthoallylanisoïl, boiling 222°–223° C. sp. gr. at 15, ·9972; and orthobutenylanisoïl, boiling 232°–234° C. sp. gr. at 15, ·9817. The author compares the physical properties of the ortho- and para- compounds; the former boil about 10° lower, have a slightly higher specific gravity, and crystallise with much greater difficulty.—Note on the action of ammonia on anthrapurpurin, by W. H. Perkin. The author has investigated the colouring matters produced by the action of heat on an ammoniacal solution of anthrapurpurin in sealed tubes at 100° and 180° C. At the former temperature an unstable substance was obtained dyeing alumina mordants purple and weak, iron mordants indigo blue. At 180° a new substance, anthrapurpuramide, was formed, which does not dye mordants.—On certain polyiodides, by G. S. Johnson. The author attempted without success to prepare a compound having the composition $AgRI_6$, or a similar substance having thallium in place of silver; various compounds of silver and potassium, thallium and potassium, and especially a very complicated substance containing lead, acetic acid, potassium, and iodine were formed and analysed. The latter substance crystallises in square prisms; of the six phases two have a dark purple and four a greenish golden reflection.—On an improved form of wash-bottle, by T. Bayley. The object of this contrivance is to prevent the reflux of steam or other gases, such as ammonia, into the mouth of the operator, without losing the advantages of the ordinary wash-bottle.—On the preparation of glycollic acid, by R. T. Plimpton. The author endeavoured to prepare this substance by the method recommended by Prof. Church, but only obtained quantities too small for analysi , using two ounces of oxalic acid.

Edinburgh

Royal Society, February 18.—Sir William Thomson in the chair.—Prof. Fleeming Jenkin read a paper on the application of the graphic method to the determination of the efficiency of a direct acting steam-engine. His results show that it is impossible to determine by empirical laws the efficiency of an engine as it varies with every change in the rate of action, the point at which the steam is cut off, &c.—Prof. Tait communicated a paper by Mr. Alexander Macfarlane, M.A., B.Sc., on the disruptive discharge of electricity. The difference of potential required to produce a spark between spheres for distances up to 15 centimetres is proportional to the square root of the distance between their centres and between parallel plates; it is a hyperbolic function of the distance between them; for a constant distance it is a similar function of the pressure of the gaseous medium for a range of pressures of from one atmosphere to 20 mm.—Mr. J. Y. Buchanan, of the *Challenger*, read a paper on the compressi-

bilities of distilled water, sea-water, solution of chloride of sodium, and mercury. They were determined by instruments resembling piezometers immersed in the sea when free from currents, the approximate pressure being ascertained by the sounding-line. The compressibilities at various temperatures relative to that of distilled water were ascertained by compression in a powerful Bramah press. The compressibilities being found, these same instruments were used for measuring depths of the sea accurately when currents, &c., affected the indications of the sounding-line. A water piezometer was found to be much more sensitive to pressure than to temperature, a mercury one very sensitive to temperature and not so to pressure. The approximate depth was ascertained by the sounding-line, to which were attached the two piezometers. From the indications of the line and of the mercurial instrument the temperature of the bottom was approximately determined. This, applied to the indication of the water instrument, gave the depth accurately, and hence the true temperature was found from the mercurial. He described a new method of getting the compressibility of glass.—Prof. Crum Brown and Mr. A. Blaikie gave a paper on the decomposition of the salts of trimethyl sulphine by heat.—Sir Wm. Thomson communicated extracts from letters of Prof. Quincke, who has found that the surfaces of glass and quartz which have been for some time cut, change very much in their indices of refraction.—Prof. Jenkin mentioned some experiments by Mr. Gott on the telephone, which, he maintained, completely confirm Prof. Graham Bell's theory of the telephone.

Scottish Meteorological Society, February 1.—It was stated in the report from the Council that the Government had paid 1,000*l.* to the Society for past services rendered by it to a public department; that the Society has 102 regularly observing stations, in addition to the sixty lighthouse stations on the Scottish coast, and a large number of rain-observing stations; and that during the past four months seventy-five new members had been added to the Society.—Mr. Buchan read a paper on the weather of 1877, more special attention being given to the rainfall, the paper being illustrated by thirteen maps coloured according to the quantity of rain which fell in each month in different parts of the country. The maps represented in a strong light the influence of the physical configuration of the land on the rainfall in relation to different winds, both as regards their direction and their height in the atmosphere.

Paris

Academy of Sciences, March 18.—M. Fizeau in the chair.—The following papers were read:—Motion of translation of cyclones; theory of a "rain motor," by M. Faye. He cites with satisfaction Prof. Loomis's recent conclusion from observations of the U.S. Signal Office, that "rainfall is not essential to the formation of areas of low barometer, and is not the principal cause of their formation or of their progressive motion"; and he regrets that the theory to which the "rain motor" belongs dies so hard.—On a trombe observed at sea, in December last, in the Straits of Malacca, by M. Faye. This was seen to descend from the clouds and penetrate the sea; the water rose round and exteriorly to the trombe.—M. Tisserand was elected member for the section of astronomy in place of the late M. Leverrier.—On the measurement of the mean density of the earth, by MM. Cornu and Baille. They have improved their apparatus by using four (instead of two) attracting spheres of mercury, and diminishing the distance of attraction. From an analysis of Baily's experiments, and with regard to resistance of the air, they show that the result was to assign too high a value for the mean density of the earth.—On the marine mollusca of Stewart Island (New Zealand), by M. Filhol. The number of these is 179.—Influence of rest and of motion on the phenomena of life, by M. Horvath. He placed in glass tubes a liquid favourable to multiplication of bacteria, and containing some alive. Some of the tubes were then continually agitated, while others, with the same quantity and at the same temperature, were left at rest. There was abundant multiplication in the latter, none in the former.—On interstitial fibromas of the uterus, by M. Abeille.—On improvements in the telephone, by M. Navez. He claims priority in use of the Ruhmkorff coil (which use, however, M. du Moncel carries back to Gray). For transmitter in Edison's system, he uses a battery of ten or twelve rundles of carbon. In the transmitter he uses a vibrating plate of copper covered with silver; in the receiver one of iron doubled on one of brass, and the two soldered together. Two magnets are employed in the receiver, with core and bobbin between, &c.—M. Vulpian presented M. Bernard's last volume, "Leçons sur les Phenomènes de la Vie commune aux Animaux et aux Végétaux, faites aux Muséum d'Histoire Naturelle."—Researches on absorption of ultra-violet rays by various substances, by M. Soret. *Inter alia*, distilled water, with a thickness of 10 mm., is considerably less transparent than quartz, and stops the last line of aluminium, but with greater thickness it takes the first rank, and it may be considered a solvent of almost perfect transparence. Absorption in the ultra-violet is subject to the same general laws as in the visible spectrum. Several substances are mentioned which give absorption bands in the ultra-violet.—On a new telephone called the mercury telephone, by M. Breguet. This is on the principle of a Lippmann electrometer. Suppose two vessels containing mercury with acidulated water above, and, dipping in the latter in each a tube partly filled with mercury and ending below in a capillary point. The mercury in the two vessels is connected by wire; likewise that in the two tubes. On speaking over one tube the air vibrations in it are communicated to the mercury, which translates them into variations of electromotive force, and these variations generate corresponding vibrations in the air-mass of the receiver. The practical form of the instrument is an improvement on this.—On the daily oscillation of the barometer, by M. Renon.—Investigation of oxide of lead in the hyponitrate of bismuth of druggists, by M. Carnot. The hyponitrate is sometimes given to the extent of 10 to 20 grammes per day, and this might include one or two decigrammes of oxide of lead.—Researches on gallium, by M. Dupré.—Action of ozone on iodine, by M. Ogier.—Researches on suspension of phenomena of life in the embryo of the hen, by M. Dareste. An egg taken from a hatching apparatus after two days and replaced after two days (the heart beats having quite stopped) develops a chick as usual.—Proofs of the parasitic nature of anthrax; identity of lesions in the rabbit, the guinea-pig, and the sheep, by M. Toussaint.—On a new bioxide of manganese couple, by M. Gaiffe. This consists of a carbon cylinder with holes parallel to its axis filled with grains of bioxide of manganese; it is placed in a glass containing water and about 20 per cent. of chloride of zinc.—On three bolides observed in January and February at Damblain (Vosges) and Chaumont (Haute Marne), by M. Guyot.

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THURSDAY, APRIL 4, 1878

THE SCOTTISH UNIVERSITIES COMMISSION

THE Report of the Royal Commissioners appointed to inquire into the Universities of Scotland, together with Evidence and Appendix, has just been issued. We will begin our reference to this important document with an extract (p. 49):—

"It would, we consider, be a misfortune if the separate individuality which has long characterised the Scottish Universities were impaired, and if the spontaneous and healthy development of different schools of thought were rendered impossible by laying an obligation on men of original genius to make their teaching subservient in all its details to the requirements of an extraneous examining authority. The admirable influence which the Scottish Universities have hitherto exerted upon the people of the country has been due not only to the prolonged and systematic course of mental discipline to which their students have been subjected, but to the stimulus and encouragement given to inquiring minds by distinguished men who have made the professorial chairs centres of intellectual life; and we cannot think it desirable that any such changes should be made as would tend to lower the Universities into mere preparatory schools for some central examining board."

These words are peculiarly noteworthy at the present time, when attempts at centralisation are becoming more rampant than ever:—and when the general tendency of so-called "Educational Reform" is to substitute for teaching in the highest sense, an almost Chinese system of examinations, with their inevitable attendant *Cram*. For the true definition of Cram is "preparation for examination, and for examination alone":—and its varieties are infinite, ranging as they do from processes closely resembling the manufacture of *foie gras* in the live bird, to those which are adopted in dressing diseased meat for the market. The Scottish Universities have, it seems, been hitherto singularly free from this monstrous evil; and, it is to be hoped, will remain so. The Commissioners who are now dealing with our great English Universities would do well to pay particular attention to this point, for Cram, in its worst forms, is by no means a stranger to them. The true cure for this evil is very well stated in the Report (p. 49):—

"The examination of the students of a University for their degrees by the Professors who have taught them is sometimes spoken of as an obvious mistake, if not abuse; but those who are practically acquainted with University work will probably agree with us that the converse proposition is nearer the truth. In fact, it is hard to conceive that an examination in any of the higher and more extensive departments of literature or science can be conducted with fairness to the student, unless the examiners are guided by that intimate acquaintance with the extent and the method of the teaching to which the learner has had access, which is possessed only by the teachers themselves."

Nothing could be more true, or more happily put. Let all University instruction (in England as well as in Scotland) be real *teaching*, such as is (or at least ought to be) given by Professors or Lecturers and their specially chosen Assistants, and let the teachers be in the main the examiners. Mere speed of writing, and other similar qualifications, are unworthy the notice of scientific men or scholars—and certainly ought to have no influence in a University Examination, at least until Universities are furnished with Professors of Caligraphy, Maintien, &c., attendance upon whose lectures shall be made compulsory. It is right and proper that such things should be looked to in Civil Service Examinations and the like—just as it is right that the candidates in some of these should be submitted to medical inspection. But who ever heard of medical inspection in a University examination?

But we now come to the one true difficulty in this part of the question:—*How to choose Professors*. On this point there are several very useful hints, both in the *Report* itself and in the *Evidence* appended.[1] The Commissioners do not seem very decided in their recommendations, so many widely differing and yet individually plausible schemes have been submitted to them. But practically the patronage seems from the evidence to be very fairly bestowed (*i.e.*, in very good hands) in the majority of the Scottish Universities. The main exception is that of Edinburgh, where several of the most important chairs were left by the Universities (Scotland) Act, 1858, virtually in the gift of the Town Council, which had been up to that date the supreme authority in the metropolitan University. Such a state of things is barely credible to us in England. For, though custom has familiarised us with great schools under the management of City Companies, we could hardly imagine the Mayor and Aldermen of Cambridge electing to the Lowndean or Lucasian Professorship. Yet the chairs once held by Maclaurin, Black, Leslie, Dugald Stewart, &c., are at the disposal of a Board of seven, four of whom are nominated by the Edinburgh Town Council! Instead of the heroic treatment which such malformation demands, and which would probably have made opposition impossible; the Commissioners propose merely to create two additional members of this Board, so as to place the Town Council representatives in a minority; a step whose timidity may only ensure a violent, and too probably a successful, resistance.

It appears clearly from these volumes that the one great want of the Scottish Universities is *money*. Over and over again, throughout the evidence, this is painfully brought out. Yet, with their few thousands these Universities are at present educating many more students than Oxford and Cambridge together, each of them with its annual hundreds of thousands. And the education given to each and all is generally of the highest order, because it is given by the Professors themselves. How many Cambridge men go for instruction to Cayley or Stokes—to Munro or Kennedy? Of names like these Cambridge is justly proud. But unfortunately such teaching as these men could give *doesn't pay*, so the "coach" is resorted to! In Scotland the Professors are the teachers, hourly accessible to all, and among the latest additions to their ranks we find the names of Jebb and Chrystal. They will do more good to students now in one year than they could have done in a lifetime spent in Cambridge! Comment on such a statement is needless.

After what we have just said, the reader will scarcely be

[1] The Analysis or Abstract of the Evidence, which is contained along with the Report itself in the first of these four Volumes, seems to be exceedingly well executed throughout. This is one of the specially good features of the work, and Prof. Berry, the Secretary to the Commission, deserves high credit for it.

prepared to hear that one great reproach to the Scottish University system is commonly thought to lie in the shortness of the session, as it is called. This is a great point with would-be University reformers—"Go to, ye are idle." But it will be found, on examination, that the compulsory working-time *per annum* is longer at the Scottish than at the English Universities :—whence men go down regularly whenever term divides. In Scotland the majority of the lectures continue uninterrupted (except by the week from Christmas to New Year) from the end of October to the middle of April; and by that time both students and professors *require* some relaxation, especially those who have to teach or attend the summer classes, which occupy the whole of the months of May, June, and July. The Commissioners have no hesitation on these points, and meet the grumblers very sharply. They say :—

"Without saying that the present arrangement of the academical year is the best that could be devised, it is that which long experience has shown to be the most suitable to the circumstances of Scotland. Nor is it without its advantages for the purposes of study. To the well-advanced and intelligent the vacation affords an opportunity for reflection and self-culture, so as to prevent his University education from degenerating into a mere acceptance of facts and conclusions from the mouth of his teacher. For a student, indeed, who is backward or indolent, the leisure afforded by the long vacation may be useless and hurtful. But to meet the case of such students the fitting remedy is that which we have already stated, the institution of summer tutorial classes where these do not now exist, and their extension, if necessary, where they do."

Our readers are already acquainted with the Report of the Devonshire Commission. A good deal of the evidence which that body collected has been taken as repeated before the present Commissioners, and they adopt, and strongly urge the carrying out of, several of the recommendations of their predecessors:—especially those which concern grants of public money for the extension of buildings and appliances for Science teaching in the Scottish Universities. It is well that this has been done, for attention has thus been recalled to one of the most important documents connected with education which has ever been laid before Parliament, and which (probably because of the moneys it recommended to be granted) has been practically shelved for some years.

So far we have been dealing with the Report as a whole. We must now more particularly examine it as regards *Science*. And this, we fancy, will be allowed to be its weakest point. The Commission was exceedingly strong on the literary, legal, and general-culture side :—but very weak—numerically at least—on the scientific. It is no disparagement to such men as Dr. Lyon Playfair and Prof. Huxley (who were the *two* representatives of Science among *twelve* Commissioners) to say that they cannot adequately represent *all* science. For there are three great divisions of Science, the Observational, the Experimental, and the Mathematical, and the third and greatest of these was altogether unrepresented on the Commission. This was a very grave defect, and the value of the Report is considerably reduced in consequence.

So strong, in fact overwhelming, was the general culture side—including Members (or ex-Members) of both Houses of Parliament, Scottish (and Indian) Judges and Advocates, &c.—that the Report cannot fail to surprise all readers by its general tenor. For there can be no question that in it Science has managed to carry the day against all comers :—the greater the pity that it was not fully represented, if but by the addition of a single mathematician. To make room for him, a lawyer might easily have been spared.

We cannot spare space for more than one instance of the proposed revolution :—but we choose an important and typical one, the modifications to be made in the mode of attaining the degree of M.A. This degree has hitherto, in Scotland, involved a certain amount of knowledge of *each* of the following seven subjects :—Latin, Greek, Mathematics, Logic, English Literature, and Moral and Natural Philosophy :—and has not been at all nearly so easy to attain as the ordinary (or Poll) degree in the English Universities—which, though at first styled only B.A., becomes M.A. by mere lapse of time and payment of additional fees, and is therefore practically the same thing. In Scotland it is now proposed that there shall be five distinct avenues (several with alternative branches) to this degree in addition to the present one :— (p. 25)

"Moved by these considerations, we have come to the conclusion that to secure a basis of general culture every student proposing to proceed to the degree of M.A. should be required to pass a 'First Examination' in Latin, Greek, Mathematics, English, and, when the state of education in the schools renders it practicable, in Elementary, Physical and Natural, Science. This examination should be passed at the beginning of the University session,—either the winter or the summer session,—every student proposing to graduate being required to pass it, whether he may have been previously a student in the University or not. With some modification, the examination might be so adapted as to apply not only to students proceeding to a degree in Arts, but to those also intending to graduate in Law, Science, or Medicine. In the case of persons proceeding in Law, we think that an examination in translating from French or German should be allowed as an option for Greek. Again, in the case of students proposing to graduate in Science or Medicine, we think that, as some knowledge of modern languages is most important to them, they should be examined either in translating from French and German, or in translating from one of the languages and in Greek. In this way, it would be necessary for them to show ability to translate from at least one modern language.

"As we shall explain afterwards, we regard this as the best equivalent for an entrance examination. Through its application to all proposing to graduate, whether previously students at the University or having come direct from school, a salutary reflex action on the schools will be secured by the encouragement given to them to send their pupils to the University in an advanced state of preparation. In a different shape, and if accompanied by the condition of exclusion from the University should the candidate fail to pass, an entrance examination would, in our opinion, be attended with injury rather than benefit.

"After passing the 'First Examination,' the candidate for a degree in Arts should be allowed to proceed in the present course, if he please, and as, no doubt, many will still do. If, however, he prefer to take a different course, we propose that he should be allowed to take any one of the five following departments or lines of study, viz. :—

"I. Literature and Philology.
II. Philosophy.
III. Law and History.
IV. Mathematical Science.
V. Natural Science.

"The branches to be included under these different departments we propose should be as follows:—

"I. Literature and Philology should comprise the subjects of Latin; Greek; and English Literature; together with one of the following subjects, viz.: Comparative Philology; Sanskrit; Hebrew; a Modern Language; Gaelic, with Celtic Philology. Questions on history and geography incidental to each subject should form part of the examination.

"II. Philosophy should include Logic and Metaphysics; Ethics and Psychology; and the Physiology of the Nervous System. The first two subjects are understood to embrace the History of Philosophy.

"III. Law and History should include Civil Law; either Constitutional Law or International Law; and Political Economy; together with the history of any one of the following groups, viz.: Greece and Rome; Modern Europe; Egypt, Syria, Palestine, and Arabia; India; Ancient and Modern America.

"IV. Mathematical Science should embrace Mathematics, pure and applied; Natural Philosophy; and Physical Astronomy.

"V. Natural Science should comprehend four groups, viz.:—(1) Applied Mathematics, Natural Philosophy, and Chemistry; (2) Natural Philosophy, Chemistry and Physiology; (3) Physiology, Botany, and Zoology; (4) Natural Philosophy, Chemistry, and Geology. A candidate should be allowed to take any two of these four groups; and the practical working of the arrangement would be that Natural Philosophy and Chemistry would be compulsory, while any option would be given between the mathematical and the morphological sciences.

"It may be explained that the subjects of examination in the sciences comprehended in Department V. are such as are required in the first Bachelor of Science examination as detailed in the Calendars of the Universities of Edinburgh and London (1877). The purpose we have had in subdividing the subjects of Department V. into groups has been, in the first place, to ensure a sound acquaintance with Physics and Chemistry, which lie at the foundation of all natural science; and, in the second place, so much being secured, to give fair play to individual intellectual tastes and peculiarities. It is rare to find a man equally capable of dealing with long chains of abstract reasoning, or with experimental research, and of observing and remembering the analogies and differences of form. The scientific aptitude, when strongly marked, is either for mathematics, for experimental investigation, or for morphology, rarely for all three.

"In regard to the scientific subjects, mere book knowledge should not suffice; practical work in the laboratory should be essential."

We are much mistaken if this Report does not produce great irritation, amounting in many quarters to white heat at least, and determined opposition. The dry husks of speculative "philosophy" which, feebly existent even in the present day (like Bunyan's *Pope* and *Pagan*), formed so large an ingredient in the mental pabulum of Scottish students in the past, are doomed to "cease from troubling":—but they will die hard. In their place will come the still oppressed truths of modern science, and the legitimate speculations which Experience and mathematical power alone can enable the human mind to originate and develop.

SUN-SPOTS AND RAINFALL

THE paper which we print from Mr. Meldrum this week, appearing as it does within a few days of the debate in the House of Commons on the Indian Famine expenditure, is one which should be interesting to many besides professed meteorologists. It will, for one thing, enable even the most unscientific among us to see the manner in which men of science are striving to arrive at the truths of nature the while the average Member of Parliament only refers to their labours in order to sneer at them even when their results may elucidate a question of high national importance.

Granting that the Member for Cambridge comes up to the average of our legislators, let us see how he distinguished himself on Tuesday. In his indictment of the policy of Sir John Strachey, he was unwise enough to touch on the question of the connection between sun-spots and the Indian rainfall. "It appeared that, according to the astronomer to the government at Madras, the absence of several important spots (*sic*) on the sun's disc was connected with the retarded rainfall." It is clear from this, we think, that Mr. Smollett, in his ignorance of all things solar, instead of taking a little trouble to inform himself, has built up a mental image of the physics of our central luminary, by likening it to the house of which we will grant again he is one of the most prominent units. The cause of the sun-spot minimum appears to him to be that at this time "several important spots"—let us say the Smolletts of the sun—are in the tea-room or at dinner, anyhow they are absent from the division, and the opposition carries the day—that is, if Mr. Pogson is right, but he proceeds to show that Mr. Pogson is wrong.

Dr. Lyon Playfair, as was to be expected, put this matter right before the house. He stated that "it was established that famines in India came at periods when sun-spots were not visible. Out of twenty-two great observatories of the world it had been shown in eighteen that the minimum rainfall was at times when there were no spots on the sun. That was as true in Edinburgh as in Madras, in St. Petersburg as in Australia. It was therefore essential for the Government of India to take that into consideration in calculating as to when famines were likely to occur. The Secretary of State for India had acted wisely in sending out photographers to the Himalayas to take photographs of the sun, and having seen some of those, he was sorry to say that on none which he had seen were spots to be detected." As Dr. Playfair is not in the habit of making statements without getting up his case, we may be thankful to Mr. Smollett for the sneer which called Dr. Playfair up.

Mr. Meldrum's communication contains a very condensed reference to his memoir on Sun-spots and Rainfall recently presented by him to the Meteorological Society of the Mauritius, a memoir which goes far to complete one portion of that magnificent edifice, the erection of which was foreseen by Sir Wm. Herschel at the beginning of the present century.

In this important paper Mr. Meldrum, than whom there exists no higher authority, states that the result of his seven years' work has been to convince him that the connection between sun-spots and rainfall is as intimate as that between sun-spots and terrestrial magnetism; and that having regard to the number of cycles at our disposal we should be as justified in rejecting the diurnal oscillation of the barometer as the curve along the hills and hollows of which the maximum and minimum rainfalls of the world lie.

This result of course will be received with incredulity

by many—and for many reasons. In the first place the enormous variation in the solar activity is a fact only fully realised by very few. Men grown old in the service of science are as a rule as little anxious to receive new ideas as men grown old in any other of the world's activities, and further and more than this, in the case of many there is what has recently been happily termed "a paralysis of the imagination"—a thing far removed from scientific caution—which may and indeed certainly would do much harm to scientific progress if those afflicted with it had any chance of having the exclusive say in the matter.

Now that things have arrived at this stage it is well to bring to the front some extracts from those papers of Sir Wm. Herschel's to which reference has already been made, to show the wonderful prescience of the man, and also to give an idea of the valuable time which has been lost by the neglect, during three-quarters of a century, to take in hand the work from which he predicted such a rich harvest of benefits would follow.

His first reference to the *changes* going on in the sun was made in 1801.[1] He writes :—

"On a former occasion I have shown that we have great reason to look upon the sun as a most magnificent habitable globe; and, from the observations which will be related in this paper, it will now be seen that all arguments we have used before are not only confirmed, but that we are encouraged to go a considerable step further in the investigation of the physical and planetary construction of the sun. The influence of this eminent body on the globe we inhabit is so great and so widely diffused that it becomes almost a duty for us to study the operations which are carried on upon the solar surface. Since light and heat are so essential to our well-being, it must certainly be right of us to look into the source from whence they are derived, in order to see whether some material advantage may not be drawn from a thorough acquaintance with the causes from which they originate.

"A similar motive engaged the Egyptians formerly to study and watch the motions of the Nile and to construct instruments for measuring its rise with accuracy. They knew very well that it was not in their power to add one single inch to the flowing waters of that wonderful river; and so, in the case of the sun's influence, we are likewise fully aware that we shall never be able to occasion the least alteration in the operations which are carried on in the solar atmosphere. But if the Egyptians could avail themselves of the indications of a good Nilometer, what should hinder us from drawing as profitable consequences from solar observations? We are not only in possession of photometers and thermometers, by which we can measure from time to time the light and heat actually received from the sun, but have more especially telescopes, that may lead us to a discovery of the causes which dispose the sun to emit more or less copiously the rays which occasion either of them; and if we should even fail in this respect, we may at least succeed in becoming acquainted with certain symptoms or indications, from which some judgment might be formed of the temperature of the seasons we are likely to have.

"Perhaps our confidence in solar observations made with this view, might not exceed that which we now place on the indications of a good barometer with regard to rain or fair weather; but even then a probability of a hot summer, or its contrary, would always be of greater consequence than the expectation of a few fair or rainy days.

[1] "Observations tending to investigate the Nature of the Sun in order to find the Causes or Symptoms of its Variable Emission of Light and Heat; with Remarks on the Use that may possibly be drawn from Solar Observations." By William Herschel, LL.D., F.R.S., read April 16, 1801.

"It will be easily perceived that in order to obtain such an intimate knowledge of the sun as that which is required for the purpose here pointed out, a true information must be first procured of all the phenomena which usually appear on its surface."

He then gives those wonderful observations which make this paper the basis of our knowledge of the smaller units of the sun's surface, and then sums up as follows :—

"From these two last sets of observations, one of which establishes the scarcity of the luminous clouds, while the other shows their great abundance, *I think we may reasonably conclude that there must be a manifest difference in the emission of light and heat from the sun.* It appears to me, if I may be permitted the metaphor, that our sun has for some time past been labouring under an indisposition, from which it is now on a fair way of recovering.

"An application of the foregoing method, however, even if we were perfectly assured of its being well founded, will still remain attended with considerable difficulties. We see how, in that simple instrument the barometer, our expectations of rain and fair weather are only to be had by a consideration of many circumstances besides its actual elevation at the moment of inspection. The tides also present us with the most complicated varieties in their greatest elevation, as well as in the time when they happen on the coasts of different parts of the globe. The simplicity of their cause, the solar and lunar attractions we might have expected, would have precluded every extraordinary and seemingly discordant results.

"In a much higher degree may *the influence of more or less light from the sun be liable to produce a great variety in the severity or mildness of the seasons of different climates and under different local circumstances*, yet when many things which are already known to affect the temperature of different countries and others which future attention may still discover, come to be properly combined with the results we propose to draw from solar observations, we may possibly find this subject less intricate than we might apprehend on a first view of it.

"If for instance we should have a warm summer in this country when phenomena observed in the sun indicate the expectation of it, I should by no means consider it as an unsurmountable objection, if it were shown that in another country the weather had not been so favourable.

"And if it were generally found that our prognostication from solar observations held good in any one given place I should be ready to say that with proper modifications they would equally succeed in every other situation.

"Before we can generalise the influence of a certain cause we ought to confine our experiment to one permanent situation, where local circumstances may be supposed to act nearly alike at all times which will remove a number of difficulties."

This was in April; in May he read another paper.[1]

"Having brought the solar observations relating to the symptoms of copious emission of the light and heat of the sun to the 2nd of March I gave them continued in this paper to the 3rd of May. It will be seen that my expectations of the continuance of the symptoms which I supposed favourable to such emissions, have hitherto been sufficiently verified; and by comparing the phenomena I have reported with the corresponding mildness of the season, my arguments will receive a considerable support.

"I have given the following observations without delay as containing an outline of the method we ought to pursue in order to establish the principles which have been pointed out in my former paper. But we need not in future be at a

[1] "Additional Observations tending to Investigate the Symptoms of the Variable Emissions of the Light and Heat of the Sun; with Trials to set aside darkening Glasses by Transmitting the Solar Rays through Liquids, and a few Remarks to Remove Objections that might be made against some of the Arguments contained in the former Paper," by William Herschel LL.D., F.R.S. Read May 14, 1801.

loss how to come at the truth of the current temperature of this climate as the thermometrical observations which are now regularly published in the *Philosophical Transactions* can furnish us with a proper standard with which the solar phenomena may be compared. This leads me to remark that although I have in my first paper sufficiently noticed the want of proper criterion for ascertaining the temperature of the early periods where the sun has been recorded to have been without spots, *and have also referred to future observations for showing whether a due distribution of dry and wet weather with other circumstances which are known to favour the vegetation of corn, do or do not require a certain regular emission of the solar beams*, yet I might still have added that the actual object we have in view is perfectly independent of the result of any observations that may hereafter be made on the *favourable or defective vegetation of grain in this or in any other climate* . . . . It may be hoped that some advantage may be derived even in agricultural economy, from an improved knowledge of the nature of the sun and of the causes or symptoms of its emitting light or heat more or less copiously."

It perhaps will be news to many that the idea of a possible connecttion between sun-spots and rainfall which has been represented as a modern idea, may really be credited to a man whose chief work was done in the last century.

DARWIN'S "DIFFERENT FORMS OF FLOWERS"

The Different Forms of Flowers on Plants of the same Species. By Charles Darwin, M.A., F.R.S. (London: John Murray, 1877.)

THIS is another of the remarkable series of volumes in which Mr. Darwin has given us the extremely valuable results of his researches in the vegetable side of biology. Mr. Darwin's method of investigation would in itself be a very interesting subject for consideration. It is, however, sufficient to point out that its characteristic feature is the combined attack upon a given problem from both its morphological and physiological aspects. This method Mr. Darwin employs with consummate success, and in turning over the pages of the present book—a considerable part of which has been before the world for more than a decade without being materially impugned—one is almost distracted from the intrinsic interest of the facts and speculations by the sagacity with which the research is carried on, and the skill with which the results are marshalled for our information. It is peculiarly worthy of notice in the present volume how the reader is allowed, in studying Mr. Darwin's pages, to form his own hypotheses in explanation of the facts, only to be compelled in due course, as the narrative proceeds, to admit that such hypotheses are utterly untenable. There is no impression so curious as to find oneself so distinctly under the hands of a master, and to realise that the calm flow of the argument proceeds over the *débris* of objections and difficulties which are found to be already comminuted as soon as one attempts to give them any definite form.

It would be quite impossible to treat, in the short space at our disposal, all that calls for notice in the present volume. Commencing with a short introduction, the body of the book falls into three divisions. The first treats of heterostyled plants, and contains in a connected form the substance of Mr. Darwin's various papers communicated to the Linnean Society. The second and third divisions are much shorter, and treat respectively of the passage of hermaphrodite into diœcious plants, and of cleistogamic flowers.

As has been already remarked, Mr. Darwin's researches on what are now termed heterostyled plants have been common scientific property for many years, and have filtered down into the current text-books. The seventh and eighth chapters are therefore the essentially new part of the book, and these we shall more particularly consider.

The vast majority of flowering plants are, as is well known, hermaphrodite, that is to say, they contain within the same floral envelopes both male and female organs. The governing principle in the morphological adaptations of flowers is apparently to escape the obvious consequences of such juxtaposition and evade self-fertilisation. This is effected either by their being dichogamic—that is the sexual organs in any one flower maturing at different times, or by their being entomophilous—that is calling in the intervention of insects to carry the pollen of one flower to the stigma of another, or by their being heterostyled—that is by the flower being modified in two or three ways, admitting of a certain number of reciprocal modes of fertilisation which are legitimate, and of others which are distinguished as illegitimate, and are more or less sterile.

Each of these modes of avoiding self-fertilisation practically sets up a functional separation of the sexes, and it might seem that the cases in which this separation is *structurally* accomplished are its natural sequence. Mr. Darwin points out, however, very conclusively that this is by no means the case.

"There is much difficulty in understanding why hermaphrodite plants should ever have been rendered diœcious. There would be no such conversion unless pollen was already carried regularly by insects or by the wind from one individual to the other, for otherwise every step towards diœciousness would lead towards sterility. As we must assume that cross-fertilisation was assured before an hermaphrodite could be changed into a diœcious plant, we may conclude that the conversion has not been effected for the sake of gaining the great benefits which follow from cross-fertilisation."

Mr. Darwin is led to find an explanation in the advantage to the plant in the diminished strain of producing sexual organs of only one kind instead of both. And the process of manufacturing diœcious plants is one which can be actually seen in process. The cultivated strawberry under the influence of the American climate is a marked instance. In such cases the hermaphrodite state can be traced into the diœcious with every intermediate grade. The ultimate fate of heterostyled plants is perhaps to be converted into diœcious ones, and in this instance the change would be more immediate and with fewer connecting links. The functional diversity already exists and the corresponding suppression of the sexual organs is all that is needed to render it complete.

The concluding chapter on cleistogamic flowers certainly does not yield in interest to any preceding portion of the book. The existence of these curiously-modified structures has long been known, but it is only within the last twenty years that they have been attentively studied, and Mr. Darwin's account is a very masterly discussion of all that has been written on a very puzzling subject, tested

and enriched by his own observations and experiments. As their name implies, these flowers never open, and in some cases they have been passed over as abortive bud-conditions of flowers of the normal conspicuous type. Their petals are, of course, superfluous, and are usually completely suppressed, or nearly so, the stamens and pistil are also much reduced in size, but though morphologically reduced, are physiologically fully developed, and such flowers are very fertile. In fact, in some instances, as in *Viola canina*, the production of seed is principally dependent upon them, the ordinary flowers, from want of pollen, or the absence of the visits of bees, rarely producing capsules.

At first sight the suggestion seems a tempting one, that in these curiously degraded flowers, in which all the laboriously-acquired adaptations for cross-fertilisation are entirely discarded, we have a reversion to a less highly organised ancestral type. And this may still to some extent be true, though Mr. Darwin shows that they "owe their structure primarily to the arrested development of perfect ones." In some cases, as Oliver has shown in *Campanula colorata*, and Scott in *Eranthemum ambiguum*, the same plant bears as well as cleistogamic and perfect flowers, intermediate forms between the two. What is, however, still more significant, is that the cleistogamic flowers are themselves sometimes the starting point of structural adaptations, to effect more perfectly the self-fertilisation which ordinary flowers have been so marvellously modified to avoid. Thus, in *Specularia perfoliata* the rudimentary corolla is modified into a perfectly closed tympanum, and in *Viola canina* the pistil is much modified. Mr. Darwin, however, has shown that cleistogamic flowers do not invalidate the general principle as to the disadvantage in the long run of self-fertilisation. After two years' growth, crossed seedlings of *Ononis minutissima* beat those produced from cleistogamic flowers in mean height in the ratio of 100 to 88.

It seems that the end really gained by cleistogamic flowers is the production of a large supply of seeds with little expenditure; the plant does the work more cheaply and makes the numbers pay. It is curious to reflect what, relatively speaking, an enormous expense a plant puts itself to in such a case as *Viola* in producing in the spring a large number of conspicuous flowers furnished with nectaries and all the complicated apparatus needed to insure cross-fertilisation, with the result, perhaps, of securing a very few cross-fertilised capsules. Having made these sacrifices, it proceeds during the summer to insure the production of a sufficient crop of less costly seeds by the inconspicuous aid of cleistogamic flowers.

Mr. Darwin, with characteristic ingenuity, adduces another instance of this balancing of conflicting advantages in the effort to secure before all things the perpetuation of the race. A seed in the ground—to parody a common proverb—is worth a good many exposed to depredation above it; and though dissemination is a gain, secure sowing is no less important. Many cleistogamic plants, therefore, having deliberately given up the advantage of cross-fertilisation, give up those attaching to change in the place of growth, and bury their fruits even before they are mature. This is the case with *Viola odorata* and *hirta* and *Oxalis Acetosella*. In other instances—and Mr. Darwin will pardon the remark that he has scarcely dwelt on the distinction—the buried fruit is the product of *subterranean* flowers. This is the case with *Vandellia sessiflora*, *Linaria spuria*, *Vicia amphicarpos*, *Lathyrus amphicarpus*, and *Amphicarpæa*, the three last cases belonging to *Leguminosæ*. The distinction is important because, while flowers produced under such abnormal circumstances as on subterranean branches must be necessarily cleistogamic, it by no means follows that aerial flowers which subsequently bury their fruits should also be cleistogamic, and Mr. Darwin very properly excludes the well-known earth-nut (*Arachis hypogæa*) from his list, as, though the ovaries are buried, the flowers are conspicuous. In such cases it is possible that the comparative humidity of the soil favours the maturation of the capsules, and especially so with small herbaceous plants in dry climates. Mr. Bentham in fact has pointed out in the case of *Helianthemum* that a prostrate habit which brings the capsules in contact with the surface of the ground postpones their maturity, and so favours the seeds attaining a larger size. *Cyclamen* (in every species except *C. persicum*), by the spiral contraction of its peduncle, brings its capsules down to the surface of the soil, though it does not appear to actually bury them, as some authors have supposed to be the case. If this is advantageous we need not wonder that the local amphicarpic races of *Lathyrus sativa* (of which there seem to be several) found in such dry countries as Portugal on the one hand, and Syria on the other, should acquire the habit of bearing actually subterranean fruit.

The steps, however, by which such a specialised mode of burying the fruit has been attained as exists in *Arachis*, are not easy to follow. Of few plants have the structure and habit been more misunderstood. Descriptive writers, from Rumphius to Endlicher, have represented it as having two kinds of flowers—and as being in fact what Mr. Darwin would call andromonœcious. It really, however, appears according to the careful examination of Poiteau and Bentham to have only flowers of one kind. These are apparently stalked, but the long stalk is in reality the attenuated calyx tube, which is a very peculiar feature for a leguminous plant. At the bottom of the calyx tube is the ovary which, after fertilisation, is gradually carried away by the development of a gynophore or subovarian stalk. It is the elongation of this gynophore—and not as Mr. Darwin states, by an oversight, the flower-stems drawing the flower beneath the ground—which buries the ovary. The careful observations of Correa de Mello show that though the gynophore may become three to four inches long, the ovary does not enlarge till it is buried, which confirms what has been said above as to the meaning of the habit generally. The details of the process by which the gynophore manages to bury the ovary would be a most interesting subject for investigation.

The obscurity which has attached to *Arachis* has also extended to *Voandzeia*, another leguminous plant cultivated like *Arachis* in hot countries for its subterranean pods. Mr. Darwin remarks that the perfect flowers are said never to produce fruit (pp. 327 and 341). Correa de Mello, however, never succeeded in detecting the cleistogamic flowers, and declares that it is "placed beyond all doubt that the hermaphrodite petaliferous flowers *do*

produce fruit."[1] Perhaps therefore *Voandzeia* may have to be expunged from the list of cleistogamic plants, while on the other hand *Krascheninikowia*, according to a thoughtful criticism of Mr. Darwin's book in the *Journal of Botany*, must be restored to it.

It may also be noted that according to Bentham *Martinsia* was a genus founded on a cleistogamic state of *Clitoria glycinoidis; Cologania* also should possibly be added to the list since Zuccarini's *Martia mexicana* appears to be an apetalous condition of some species of the genus.

Although the habit of producing cleistogamic flowers is pretty widely diffused amongst flowering plants it is locally concentrated in particular groups. This is particularly true in the case, as Mr. Darwin has pointed out, of *Malpighiaceæ* and *Acanthaceæ*, and amongst *Leguminosæ* in the *Glycineæ*. The genus *Viola* is remarkable in this respect; it is rich in cleistogamic species except in the section *Melanium*, to which *V. tricolor* belongs. In this species, besides conspicuous flowers adapted for self-fertilisation, smaller and less conspicuous flowers adapted for self-fertilisation are produced. These are not closed, but, as Mr. Darwin points out, "they approach in nature cleistogamic flowers," and though they differ in being produced on distinct plants they are perhaps destined to be as completely modified as the self-fertilising flowers of other sections of the genus.

The question as to the causes predisposing to the production of cleistogamic flowers is one of very great interest. In the first place Mr. Darwin points out that the larger proportion of known cases belong to plants with irregular flowers, that is, to plants whose flowers have been adapted for insect cross-fertilisation. Cleistogamy in this light is a resource to fall back upon when the elaborate adaptations for making insects do their work fail, as they seem to do more or less in *Viola*. It is a remarkable contrast that in heterostyled flowers, which are absolutely dependent upon insects for their legitimate fertilisation, irregular flowers are extremely exceptional, the adaptation, as far as it goes, being so complete that anything further in that direction is superfluous.

Four cleistogamic genera are normally wind-fertilised, and this shows that the cause alluded to above must be a subordinate one. Mr. Darwin urges with much force as the most potent agency, the unfavourable influence of climatic changes. From the time of Linnæus, it has been observed that exotic plants may be fertile, though their flowers have never attained proper expansion, that is to say, for the nonce they have become cleistogamic and self-fertile. The same thing occurs on a large scale with *Juncus bufonius*, in Russia, which in some districts never bears perfect flowers, while in Liguria, *Viola odorata* never bears cleistogamic ones. It is perhaps, however, doubtful whether winter-flowering plants are absolutely sterile, since the well-known *Chimonanthus*, whose name records its habit, is known to fruit, though sparingly, in this country. The evidence is, however, strong enough to render it highly probable that plants which are normally cross-fertilised, are driven into the abasement of cleistogamy when their geographical limits are extended beyond the limits not favourable to their receiving visits from appropriate insects, or to their properly expanding their flowers.

[1] *Journ. Lin. Soc.*, Bot xi. p. 256.

Here our comments must cease, content for our part if they attract a few more readers to a most fascinating research.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Elements of Articulate Speech

As a corollary to the interesting observation with the phonograph recorded by Prof. Fleeming Jenkin and Mr. Ewing in the last number of NATURE, will you allow me to point out that every capital letter of the Greek alphabet except Γ and Ρ is actually (either as written or when turned through an angle of 90°) a reversible or a reduplicate symbol.

With regard to gamma, although the capital is not, the small letter (γ) *is* reversible; and as to Ρ (or R, which is another ancient Greek form of it), many facts seem to show that by itself it does not as a rule represent a complete part of articulate speech; witness its frequent reduplication in Greek, the aspirate so often employed with it both in Greek and Latin, and the way in which it is frequently omitted, as if of no importance, from Latin words ordinarily spelt with it. The French or Italian pronunciation of this letter amounts to a reduplication in the English ear, while the English pronunciation of it amounts to its omission altogether in the ear of a Frenchman, an Italian, or a Scotchman.

In the Roman alphabet F, G, L, P, and R, are the exceptions; much might be said about each of these, but I will content myself by saying that L is obviously only an apparent exception, as it is easily derived from Λ. W. H. CORFIELD

10, Bolton Row, Mayfair, March 30

Phoneidoscopic Representation of Vowels and Diphthongs

I HAVE just obtained the two following results with the phoneidoscope[1]:—

1. If a vowel be steadily sung on a single note, a constant colour-figure is produced; but if the vowel be spoken in the ordinary conversational tone, a change of figure occurs before the sound ceases. The slurring alteration of pitch which takes place in pronouncing a single vowel is thus rendered perceptible by the eye.

2. When a diphthong is slowly intoned, two distinct figures successively present themselves, which are found on trial to be those corresponding to its constituent vowel-sounds. The two-fold nature asserted in the word "diphthong" receives by this experiment a visible illustration. SEDLEY TAYLOR

Trinity College, Cambridge, April 1

The Southern Drought

YOU ask in last week's NATURE (p. 436) for information respecting the drought in the southern hemisphere. A few days ago I received letters from Samoa and the Gilbert Islands telling me of its severity there. Droughts are of frequent occurrence in the Gilbert Islands, but my correspondent (a native of Samoa) tells me they have had an extraordinary one there, which commenced in 1876, and which continued up to the date of his letter—December 4, 1877. He says many of the people have died from starvation in consequence.

A letter from a missionary who has been forty years in Samoa contains the following:—"We have had the greatest drought I have ever known." The Samoan Islands are wonderfully fertile, and even during what is called the dry season it is rarely that more than a fortnight passes without rain. The atmosphere is always full of moisture, and there are very heavy dews at night, so that the vegetation never gets burnt up, except the drought be very extraordinary. Now, however, my correspondents speak of scarcity of food in those most fertile islands.

Blackheath, March 29 S. J. WHITMEE

[Can our correspondent favour us with the date of the last drought or series of droughts?—ED.]

[1] See NATURE, vol. xvii. p. 426, note 2.

Cumulative Temperatures

In reference to my letter upon the above subject, which was published in your columns of February 21 last, I have received from Prof. de Candolle, of Geneva, a communication dated March 11, in which he calls attention to the fact that in his "Géographie Botanique raisonné," which was published as far back as the year 1855, he recorded the suggestion (made by himself some ten years previously) of the employment of an uncompensated pendulum fitted with a suitable registering apparatus for the determination of cumulative temperatures in connection with the application of meteorology to agriculture and to the geography of plants.

In the above work (vol. i. pp. 58 and 59) the following passage occurs:—

"Les chiffres les plus importants à connaître pour les applications de la météorologie à l'agriculture et à la géographie botanique sont, pour chaque localité, les sommes de température au dessus de + 1°, + 2°, de + 3°, etc., par année, saison, mois ou fraction de mois.

"Pourrait-on obtenir ces valeurs directement par un instrument spécial, qui dispenserait de recourir à des calculs compliqués, souvent impracticables, dans le système actuel des observations météorologiques? C'est une question que je soumets aux physiciens. Elle m'a préoccupé depuis longtemps, mais je suis loin de posséder les connaissances théoretiques et pratiques nécessaires pour arrivir à une solution. J'entrevois la possibilité de construire deux sortes d'instruments qui répondraient aux conditions désirées; je les mentionne sans pouvoir indiquer les détails d'exécution.

"L'un de ces instruments serait la pendule-thermomètre de M. Edmond Becquerel, modifié de telle sorte que les battements par une température inférieure à 0°, ou ceux inférieures à + 1°, à + 2°, etc., ne seraient pas comptés."

"Un autre système serait celui de thermomètrographes marquant les températures supérieures à tel ou tel degré, et seulement celles-là."

To this the following foot-note was added:—"Il y a plus de dix ans je fit des démarches auprès de deux astronomes, M. Gautier, à Genève, et M. Arago, à Paris, pour appliquer la pendule à la mesure des températures. Je proposais une pendule aussi dilatable que possible sous l'action de la température et un compteur adapté à l'instrument. . . . Les honorables savants auxquels je m'etais adressé penserent qu'il serait trop difficile de soustraire l'instrument à diverses causes d'erreurs."

From the above, which was written twenty-three years ago, it is clear that to the eminent botanist must be accorded the merit of priority not only of the suggestion but also of the publication of the idea of the method of averaging temperatures by observations of the pendulum, while to Mr. Stanley must be given the credit of embodying that idea in a practical form and constructing an instrument based upon the principle.

St. Leonards-on-Sea, March 16 — Conrad W. Cooke

The Wasp and the Spider

I had anticipated in my own mind Mrs. Hubbard's suggestion, and only the great pressure on your space prevented my meeting it in my previous letter. In the first place, my recollection is that the spider was of a kind that spins no web; like our own grey hunting spider, familiar in the summer on walls and palings. In the next place no species of spider, except the gossamers, habitually leaves this fine line behind it. It is in all cases a voluntary act, preceded by a perceptible pause, and pressure downwards of the extremity of the body to attach the end, whether for suspension, or in the process of forming the web. Even the gossamers are no exception to this rule; only in their case the line, in summer and autumn, is more continuously run out as a point of departure for their mysterious aërial flights. A house-spider, for instance, as he runs across the floor or across your hand, leaves no fine line behind him. The tiny gossamer has an amazing command of the material, but in the larger, web-spinning kinds it is far from inexhaustible, and, at all events, an apparently useless waste is not in the ordinary economy of nature. Moreover, in the case in question the spider was keenly aware he was pursued, and would not willingly leave so fatal a clue on his track. Mr. Merlin, who is on the list for 1878 as our consul for the Piræus, is, however, a competent observer, and could settle the question. — Henry Cecil

Bregner, Bournemouth, March 23

SUN-SPOTS AND RAINFALL

By the overland mail which arrived here on January 12, I received, through the courtesy of Dr. W. W. Hunter, two copies of a pamphlet on "The Cycle of Drought and Famine in Southern India," a copy of the *Nineteenth Century* for November, and a copy of a letter on "The Rainfall in the Temperate Zone in Connection with the Sunspot Cycle," published in Nature (vol. xvii. p. 59).

Having previously read notices of the pamphlet and being desirous to see it, I requested its author to favour me with a copy. His rainfall cycle for Madras was, so far as I could learn from newspaper reports, identical with a cycle which I had discovered long before. In my official report for 1875, which was printed and circulated in 1876, I gave a *résumé* of the results at which I had arrived from 1872 down to the close of 1875, and stated that an examination of returns from 144 stations in different parts of the world, as well as of the variations in the levels of European rivers, had led me to the conclusion that there was a rainfall cycle of the same duration as the sunspot cycle and nearly coincident with it, both the sunspots and the rainfall attaining a minimum in the eleventh, first, and second years of the cycle, and a maximum in the fifth year. Hence when I learned from an abstract of Dr. Hunter's results for Madras that in his "cycle of eleven years both the sunspots and the rainfall reach their minimum in the group consisting of the eleventh, first, and second years, and that both the rainfall and the sunspots there increase till they both reach their maximum in the fifth year," I was curious to know how his cycle had been made out; for although I had not the Madras rainfall for each year from 1813 to 1872, yet from the falls in the years of maximum and minimum sunspots which I got in the *Proceedings* and *Transactions* of the Institution of Civil Engineers (vol. xxxii.), I inferred that the Madras rainfall was not quite so favourable to my hypothesis as the rainfalls of some other places. As, however, I might be wrong, I applied for a complete table of the Madras rainfall, but without success.

A remarkable rainfall cycle for Bombay, nearly coincident with the sun-spot cycle, had been previously ascertained, and a similar cycle, though not so well marked, had also been found by comparing the yearly mean rainfalls of Anjarakandy, Bombay, Calcutta, and Nagpur with Wolf's relative sun-spot numbers.

I have now the whole history of the Madras cycle before me. The author of the pamphlet says that after many experiments he hit upon a method of working out a cycle. This method consisted in commencing with 1876, taking backwards, as far as the register extended, periods of eleven years each, and then finding the mean rainfall for each series of years in the common period.

The results obtained for Madras by this method are to a considerable extent in conformity with those which I had found for different countries; but there are discrepancies, one of the most remarkable of which is that the rainfall in the second year of Dr. Hunter's cycle is greater, instead of less, than the mean rainfall. Still there is a certain amount of coincidence. But as the method used by Dr. Hunter—and I would call special attention to this point—is different from the one by which I found my cycle, his results and mine are not comparable.

The sun-spot cycle being one of about eleven years, and the maximum epoch occurring, on an average, 3·7 years after the previous minimum, and the next minimum 7·4 years after the maximum, I found by experience that the best way of comparing the rainfall and the sun-spots was to start either from a maximum or a minimum year, and then to take the proper number of years before and after the epochal year. Commencing with a maximum year, for instance, I took five years before it and seven

years after it, or thirteen years in all. Then, with the view of reducing the effects of the so-called non-periodic variations, I took a mean of the rainfall in the first and third of the thirteen series, and a mean of that mean and of the rainfall in the second series, and so on. This gave me eleven new means, which I called the "mean cycle." Again, starting with a minimum year, I took eight years before it and four years after it, and found eleven other new means in the same way. To each set of results, or to a combination of both of them, I then applied interpolation formulæ, and found a well-marked coincidence between the sun-spot and rainfall variations.

As the sun-spot cycles are not all of the same length, it is evident that by starting from any one year and going backwards over a long period, always using the same fixed number, a maximum and a minimum year might fall into the same group.

Let me, by an example, explain my method more fully. I take the Madras rainfall :—

TABLE I.—*Rainfall Cycle at Madras.*

Years.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.
	in.	in.	in.	in.	in.	in. max. years.	in.	in.	in.	in.	in.	in.	in.
1811–23	—	—	45·11	32·41	56·00	41·16	63·56	76·25	36·33	70·01	47·13	59·61	26·62
1824–36	33·72	56·05	60·71	88·41	37·89	36·87	32·43	44·35	18·45	37·11	39·00	41·47	44·76
1832–44	18·45	37·11	39·00	41·47	44·76	49·26	52·33	53·07	58·65	58·32	36·48	50·28	65·36
1843–55	50·28	65·36	38·05	79·81	80·99	54·76	39·81	36·88	64·32	72·69	35·82	43·20	32·32
1855–67	32·32	46·99	52·95	48·50	55·14	27·64	37·19	38·18	54·61	47·23	41·64	51·39	24·37
Means	33·69	51·38	47·17	58·12	54·96	41·94	45·06	49·75	46·47	57·07	40·01	49·19	38·69
Mean Cycle	—	46·12	50·96	54·59	52·49	45·97	45·55	47·75	49·94	50·15	46·57	44·52	—
Rainfall Variation	—	− 2·48	+ 2·36	+ 5·99	+ 3·89	− 2·63	− 3·05	− 0·85	+ 1·34	+ 1·55	− 2·03	− 4·08	—
Sun-spot Variation	—	− 32·3	− 19·2	− 1·1	+ 30·2	+ 40·0	+ 29·8	+ 11·3	− 1·2	− 12·8	− 21·1	− 23·6	—
Years of Cycle...	—	1	2	3	4	5	6	7	8	9	10	11	—

It will be seen that the individual years of maximum sunspots, 1816, 1829, 1837, 1848, and 1860, are all in the same vertical column, and that all the years of minimum sun-spots, except 1810, contribute to the formation of the beginning and end of the cycle. No doubt it would have been better to have placed 1836 farther in advance, but this would have altered the position of the maximum year 1829.

The results given by the above method show a *double* oscillation of the Madras rainfall during the sun-spot period, and I see (NATURE, vol. xvi. p. 333) that Mr. J. Allan Broun has found such an oscillation for Trevandrum as well as for Madras, and this may be a characteristic of the rainfall of the whole of Southern India. We have, however, evidence of a single rainfall oscillation for other parts of India. Even Mr. Blanford now admits that there is a periodic variation underlying all irregularities and more or less coincident with the sun-spot variation.

In consequence of the method adopted by Dr. Hunter the years of maximum sun-spots, in place of being all in the same group, are spread over three or four of the groups from which he derived his mean cycle, and it is probably owing to this that he missed what, I think, is the real character of the rainfall cycle at Madras, as shown in Table I. The annual average rainfall for each year of his cycle, together with the deviations from the mean, is as follows, and it will be seen that his cycle is very different from the one given by my method :—

Years of Cycle.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
Average Rainfall ...	in. 49·15	in. 35·00	in. 49·08	in. 49·17	in. 58·33	in. 50·95	in. 50·37	in. 54·35	in. 52·88	in. 45·16	in. 37·03
Rainfall Variation ...	+ 0·84	− 13·31	+ 0·77	+ 0·86	+ 10·02	+ 2·64	+ 2·06	+ 6·04	+ 4·57	− 3·15	− 11·28

According to Dr. Hunter's cycle, the rainfall of Madras was in excess in the fifth year to the extent of *ten* inches, whereas, according to mine, it was nearly *three* inches in defect. He takes, it is true, the years 1868-76, which I do not take, preferring to wait till I get the rainfall of 1877; but although 1870, which he puts down in his fifth group, was a very wet one, the double oscillation still exists, one of the minima occurring soon after the sun-spot maximum.

I must now come to Dr. Hunter's letter in NATURE (vol. xvii. p. 59). But, first of all, I may be excused for saying that I do not think some remarks he made about a periodicity of cyclones in a former letter (vol. xvi. p. 455) were altogether calculated to put the matter in its proper light. He says (p. 456) :—" M. Poëy called the attention of the French Académie des Sciences to the subject five years ago, and published, as far back as 1873, a list of hurricanes in the West Indies from 1750 to 1873, in support of *his* views. Dr. Meldrum has worked the same question as regards the [East] Indian Ocean." Now if these words are meant to convey the impression that M. Poëy preceded me, and that I followed with the cyclones of the Indian Ocean, all I can say is that M. Poëy himself gave a different version of the matter.

In his second letter (vol. xvii. p. 59) Dr. Hunter states

(1) that the evidence with respect to the European rainfall may be considered as "against a well-marked periodicity," and (2) that the result of a "systematic inquiry (by himself) into the American rain-returns altogether fails to establish the existence of a common cycle, so far as concerns the temperate zone." On each of these points I beg to make a few remarks.

The evidence on which Dr. Hunter bases his statement as to the European rainfall is derived (1) from an examination by Mr. Baxendell, for a short period, of the rainfall at *one* English station, (2) from an examination by the late Dr. Jelinek of *fourteen* stations on the Continent, from 1833 to 1869, and (3) from a comparison of the levels of the Elbe, Rhine, Oder, Danube, and Vistula, with the sun-spots for six cycles, a comparison which Dr. Hunter ascribes to Herr Gustav Wex, but which I believe is due to another.

Now, the evidence is much more extensive. Instead of being based on one British station and fourteen stations on the Continent, it is based on more than fifty British stations and more than forty stations on the Continent, and, taken with the evidence furnished by the rivers, it is, in my opinion, conclusive.

As to the American rainfall, an examination of thirty-four returns has given me much more favourable results than those Dr. Hunter has got from twenty-two stations, and I have little doubt that he will, as he extends his investigations, also find favourable results; but I think he must first adopt a method different from the one he used for Madras.

In support of my conclusion that the rainfalls of Europe and America are subject to a periodicity which closely corresponds with the sunspot periodicity, I will for the present only submit to your readers two tables, the one showing the general results for Great Britain, the Continent of Europe and America, and the other the results for one station in each of these countries, namely Edinburgh, Paris, and New Bedford.

TABLE II.—*Comparison of the Variation in the Sun-spot Area with the Variations in the Rainfalls of Great Britain, Continent of Europe, and America, from* 1824 *to* 1867 *inclusive.*

Years of Cycle.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
Sun-spots	− 31·7	− 19·5	+ 3·5	+ 28·8	+ 39·5	+ 29·5	+ 10·4	− 4·9	− 14·8	− 21·2	− 19·4
Rainfall of Great Britain	− 2·22	− 1·67	+ 0·04	+ 1·16	+ 1·05	+ 1·46	+ 1·28	+ 0·89	+ 0·43	− 0·73	− 1·03
Rainfall of Continent of Europe	− 0·76	− 1·73	− 1·22	+ 0·44	+ 1·18	+ 0·97	+ 1·08	+ 0·60	− 0·45	− 0·36	+ 0·62
Rainfall of America...	− 2·73	− 0·88	+ 0·91	+ 0·53	+ 0·24	+ 1·34	+ 2·03	+ 0·84	− 0·27	− 0·66	− 1·24
Mean Rainfall Variation	− 1·90	− 1·43	− 0·09	+ 0·71	+ 0·82	+ 1·26	+ 1·46	+ 0·78	− 0·10	− 0·65	− 0·55

The above results have been obtained in the manner in which I obtained the rainfall variation in Table I. For Great Britain the number of stations is 54, for the Continent of Europe, 42, and for America, 32.

TABLE III.—*Comparison of Variation in Sun-spot Area with Variations in Rainfalls of Edinburgh, Paris, and New Bedford, from* 1824 *to* 1867 *inclusive.*

Years of Cycle.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
Sun-spots	− 31·7	− 19·5	+ 3·5	+ 28·8	+ 39·5	+ 29·5	+ 10·4	− 4·9	− 14·8	− 21·2	− 19·4
Rainfall of Edinburgh	− 3·02	− 1·66	− 0·64	+ 2·50	+ 3·45	+ 2·86	+ 0·64	− 0·27	− 0·91	− 2·36	− 0·56
Rainfall of Paris ...	− 0·25	− 0·61	− 0·30	+ 1·27	+ 2·22	+ 1·90	+ 1·49	− 0·84	− 1·42	− 0·94	+ 0·48
Rainfall of New Bedford	− 3·07	− 1·03	− 0·69	− 0·32	− 0·19	+ 1·38	+ 3·47	+ 2·87	− 0·32	− 1·42	− 0·68
Mean Variation of Rainfall...	− 2·11	− 1·10	− 0·54	+ 1·15	+ 1·83	+ 3·05	+ 1·87	+ 0·59	− 0·88	− 1·57	− 0·25

It will be observed (see Table I.) that the variation in the Madras rainfall is not nearly so favourable as the variations for Edinburgh, Paris, and New Bedford.

What I have given here is but a small portion of the evidence in favour of a rainfall cycle. Having worked at the subject for six years, I have concluded that the whole evidence is as satisfactory as the evidence of a connection between sun-spots and terrestrial magnetism.

C. MELDRUM

Mauritius, February 1

JULIUS ROBERT VON MAYER

SEVERAL years ago (vol. v. p. 117) we published a paper by Prof. Tyndall on the nature and value of Mayer's researches, in which they were so fully detailed that now, on the occasion of the death of the man whose labours have won for him an undying renown, we need only briefly remind our readers of the chief events in his scientific career without entering into or attempting to renew a painful controversy of which it may be said that the truth lay neither wholly with one side nor the other.

Julius Robert Mayer was born at Heilbronn, November

25, 1814. In the neighbouring University of Tübingen, he underwent the usual course of studies in the medical faculty; and after obtaining his degree as physician, passed some time in the hospitals of Münich and Paris. His entrance into professional life was as ship's surgeon on an East India vessel. While thus engaged he made an observation apparently unconnected with, but really the origin of, all his subsequent investigations. To quote from Prof. Tyndall's paper referred to:—

"In the summer of 1840, he was at Java, and there observed that the venous blood of some of his patients had a singularly bright red colour. The observation riveted his attention; he reasoned upon it, and came to the conclusion that the brightness of the colour was due to the fact that a less amount of oxidation sufficed to keep up the temperature of the body in a hot climate than in a cold one. The darkness of the venous blood he regarded as the visible sign of the energy of the oxidation. It would be trivial to remark that accidents such as this, appealing to minds prepared for them, have often led to great discoveries. Mayer's attention was thereby drawn to the whole question of animal heat. . . ."

It was the idea thus suggested which he worked out to its issue in his great generalisation. In 1841 he returned from Batavia, and settled in his native town. Here he devoted the spare hours from his professional duties to the consideration of various unsolved physical problems. Although almost entirely isolated from scientific companionship, with next to no opportunity for experimental research, and limited in time, he evolved in a short period a succession of theoretical views, which in point of originality, boldness, and comprehensive grasp of facts, stand among the foremost in the history of physics. Mayer's first contribution to scientific literature—"Ueber die Kräfte der unbelebten Natur"—appeared in Liebig's *Annalen* in 1842, and contained within the space of eleven pages the forecast of the mechanical theory of heat, as accepted at present. At this time the caloric theory still found numerous advocates, despite the classic experiments of Rumford, of Davy, and of others; and but a small minority ventured to defend, from one standpoint or another, the idea of an intimate connection between heat and motion. It was reserved for Mayer to sum together the scattered facts, and to mould from them definite views on the nature of heat. With his introduction of the expression "the mechanical equivalent of heat," and the clear exposition of the mutual interchangeability of heat and mechanical energy, he dealt the last blow to the old theory, and thus largely helped to place on a firm foundation the new doctrines of the conservation and transformation of energy. But in this Mayer did not stand alone, nor was he the only one who had a firm hold of the conceptions which have been so fruitful of result. The quiet sap of experiment was going on side by side with these daring reconnaissances beyond the borders of the known and proved, and our own Joule, whose work does not suffer because he was not the sole worker and thinker in the field, was conducting those researches which have earned for him also an undying name and fame.

Three years elapsed before the appearance of Mayer's next work in 1845, on "Organic Movement in Connection with the Transformation of Matter." In this *brochure* of 100 pages, he details at greater length the new theory, and with a most extensive, varied, and novel series of illustrations from every branch of natural science and natural history, establishes the principles that all the so-called forces are interchangeable forms of energy—the one sole force—that energy is never created or destroyed, and that all natural phenomena are accompanied by a change of the form of energy. The logical consequences of the mechanical theory of heat were followed to their uttermost limits in Mayer's work "On Celestial Dynamics," in 1848. Here he seeks to solve the difficult problems of determining the thermal effects of the movements in the universe, the maintenance of the supply of solar heat, &c. One chief source of the latter he considers to be the heat evolved by the fall of innumerable meteorites &c., into the sun.

His "Remarks on the Mechanical Equivalent of Heat," in 1851, was his last notable contribution to the development of this subject. It possesses the same fulness of original ideas as its predecessors, and in point of vividness and clearness of conception and definition, can only be rivalled by Tyndall's "Heat as a Form of Motion." A collected edition of his writings, under the title of "Die Mechanik der Wärme," appeared at Stuttgart, 1867, and a second edition in 1874; this was followed by "Naturwissenschaftliche Vorträge" (Stuttgart, 1871), and two papers under the title of "Die Torricelli'sche Leere" and "Über Auslösung" (Stuttgart, 1876). The controversy on the priority of his discoveries led to disturbances in the mental health of the great *savant*, which, however, was in time completely restored. Dr. Mayer was of an original and witty turn of mind, unrestrained in a small company, but otherwise modestly retiring within himself.

In measuring the value of Mayer's scientific achievements it must not be forgotten that he published his theory at an epoch when physicists were directing their attention especially to this very subject, and that in Denmark and England the experiments were well advanced, which led to the complete establishment of our present knowledge of the character of heat and energy. It is, however, difficult to believe that any of his rivals in this province could have developed and amplified the theory in the masterly manner shown by this obscure German physician. In perusing his works, one scarcely knows which to admire most, the wonderful powers of classification and breadth of knowledge exhibited in every page, or the charming simplicity, clearness, and aptness of illustration with which abstruse theoretical questions are put within the comprehension of a tyro in science. Certainly in view of his life and surroundings, the contributions of Mayer to the progress of physics occupy a unique position in the history of science. To quote Dr. Tyndall again—

"Mayer grasped the mechanical theory of heat with commanding power, illustrating it and applying it in the most diverse domains. He began, as we have seen, with physical principles; he determined the numerical relation between heat and work; he revealed the source of the energies of the vegetable world, and showed the relationship of the heat of our fires to solar heat. He followed the energies which were potential in the vegetable up to their local exhaustion in the animal. But in 1845 a new thought was forced upon him by his calculations. He then for the first time drew attention to the astounding amount of heat generated by gravity where the force has sufficient distance to act through. He proved, as I have before stated, the heat of collision of a body falling from an infinite distance to the earth, to be sufficient to raise the temperature of a quantity of water equal to the falling body in weight 17,356° C. He also found in 1845 that the gravitating force between the earth and sun was competent to generate an amount of heat equal to that obtainable from the combustion of 6,000 times the weight of the earth of solid coal. With the quickness of genius he saw that we had here a power sufficient to produce the enormous temperature of the sun, and also to account for the primal molten condition of our own planet. Mayer shows the utter inadequacy of chemical forces, as we know them, to produce or maintain the solar temperature. He shows that were the sun a lump of coal, it would be utterly consumed in 5,000 years. He shows the difficulties attending the assumption that the sun is a cooling body; for supposing it to possess the high specific heat of water, its temperature would fall 15,000° in 5,000 years. He finally concludes that the light and heat of the sun

are maintained by the constant impact of meteoric matter. I never ventured an opinion as to the accuracy of this theory; that is a question which may still have to be fought out. But I refer to it as an illustration of the force of genius with which Mayer followed the mechanical theory of heat through all its applications. Whether the meteoric theory be a matter of fact or not, with him abides the honour of proving to demonstration that the light and heat of suns and stars *may* be originated and maintained by the collisions of cold planetary matter."

His services were recognised by election to membership in the French Academy of Sciences and other foreign societies, and two years before his death the King of Würtemberg elevated him to the nobility. Mayer received the Copley Medal of the Royal Society in 1871.

OUR ASTRONOMICAL COLUMN

TOTAL SOLAR ECLIPSES.—The eclipse of the sun on July 29, in which the belt of totality traverses the North American continent from Behring's Strait to the Gulf of Mexico, is a return of the eclipse of June 16, 1806, which was observed in the United States by Bowditch and the well-known Spanish astronomer, Ferrer; in this year it was central, with the sun on the meridian in 65° 30′ W., and 42° 23′ N., and the duration of total eclipse exceeded five minutes. At its next return on June 27, 1824, it was total at apparent noon in 170° 4′ W., and 44° 42′ N., but the course of the central eclipse was almost entirely an ocean-track. In 1842, on July 8, the total phase passed over the south of Europe, and was observed by a great number of astronomers, amongst them by the Astronomer-Royal at the Superga, near Turin, and by Baily, at Pavia, and Arago, who was stationed at Perpignan, gave a graphic account of the circumstances attending the extinction of sunlight, which has been often quoted. At the ensuing return of the eclipse in 1860, the "Himalaya" expedition was organised, and numerous descriptions of the phenomenon are upon record; one of the best of them is that given before the Royal Society, as the Bakerian Lecture, by Mr. de la Rue. American observers will doubtless render good account of the eclipse in July next. Its last return in the present century will take place on August 9, 1896, when it will be total soon after sunrise in the north of Sweden and Norway, central at apparent noon in 112° 21′ E. and 65° 38′ N. between Nova Zemlia and the mainland of Asia, ending in 179° 3′ W. and 18° 35′ N. in the North Pacific.

We have already given in this column some particulars of the total phase as it will occur in the present year in the United States. The last total eclipse visible in that country took place on August 7, 1869, and is the subject of detailed description in a very interesting appendix to the *Washington Observations*. The line of totality in the eclipse of January 11, 1880, will just reach American ground before sunset; some fifty miles south of Monterey, in California, the eclipse will be total for about forty seconds, but the sun will be at less than 2° altitude, thus affording a similar case to that which some interpreters of Herodotus have supposed to have occurred in the eclipse of Thales at the site of the battle between the Medes and Lydians when "day was suddenly turned into night;" though of course a characteristic of every total eclipse, it does not frequently happen that after a long ocean track the total phase is just landed on the coast of a continent at sunset. But although January, 1880, will witness what is strictly the next total eclipse of the sun on the soil of the United States, it does not appear that there will be one favourably circumstanced for observation until the year 1923, on September 10. As it is possible some readers may be interested in seeing the particulars of this eclipse, in connection with the phenomenon in July next, or as the next following eclipse in which totality can be well observed in any part of the North American continent, we give elements, &c., here, derived upon a similar system of calculation to what has been applied to other eclipses in these columns.

G.M.T. of conjunction in R.A. 1923, Sept. 10, at 8h. 28m. 42s.

	°	′	″	
Right Ascension	168	6	55·4	
Moon's hourly motion in R.A. ...		34	53·8	
Sun's ,, ,, ,, ...		2	14·9	
Moon's declination	5	38	34·0	N.
Sun's ,, ...	5	6	14·5	N.
Moon's hourly motion in decl. ...		11	5·1	S.
Sun's ,, ,, ,, ...		0	56 8	S.
Moon's horizontal parallax		59	55·3	
Sun's ,, ,, ...			8·8	
Moon's semi-diameter		16	19·7	
Sun's ,, ,, ...		15	53·7	

Hence the central and total eclipse commences at 7h. 15·6m. in long. 154° 38′ E., lat. 48° 24′ N.; it falls with the sun on the meridian in 127° 54′ W., and 38° 5′ N., and ends at 10h. 15·7m. in 63° 25′ W., and 13° 50′ N. The following are also points upon the line of central eclipse:—

Long.	Lat.	Zen. Dist.	Long.	Lat.	Zen. Dist.
° ′	° ′	°	° ′	° ′	°
120 13 W. ...	34 0 N. ...	31·1	106 12 W. ...	26 22 N. ...	40·7
116 52 ...	32 18 ...	33·1	94 33 ...	20 55 ...	54·9
112 25 ...	29 47 ...	34·7	81 14 ...	16 41 ...	71·1

Calculating directly for the first of these positions which is near the N.W. point of the island of Santa Cruz, off the Californian Coast, we find

Totality begins at 0h. 46m. 22s. } Local mean times.
,, ends at 0h. 49m. 56s. }

The duration of the total eclipse on this coast will therefore be about 3½ minutes, with the sun at an altitude of nearly 60°.

GEOGRAPHICAL NOTES

CHINA.—Lieut. Gill, R.E., who, a short time back, arrived in British Burmah, after succeeding in traversing China from Shanghai to the Yünnan frontier, has furnished a Rangoon paper with an account of his journey. Lieut. Gill, in the first instance, proceeded, *viâ* Hankow, to Chung-king, in Szechuen, whence he made a trip to the northwards, visiting the fire-wells of Tsi-liu, at which place are also found brine-wells, from which good salt is made. Mr. Gill made his way to Sung-pao-ting, on the borders of the Koko Nor, and to Liang-ngan-foo, returning by a different route to Chêng-tu. Being joined by Mr. Mesney, the two began their long, perilous, and arduous journey to Burmah overland, in the course of which they passed Bathang, on the borders of Thibet, in about N. lat. 30°. Near Bathang ranges were crossed which were some 15,600 feet in height. The most common tree near Bathang is the pine, which in some places was seen in magnificent forests, and many of the trees were found to be about three feet in diameter.

A correspondent of the *North China Herald*, writing from Chefoo, says that a scheme is under consideration for working the coal-mines of the province of Shantung, which is well known to be rich in mineral wealth. Some 120 miles to the west of Chefoo there is a fine level plain, under which at no great depth is a bed of coal twenty feet thick. The natives have from time immemorial been digging holes and getting a little out here and there, but as they have no means of drainage, the pits have filled with water as soon as they reached the good coal. The Chinese Superintendent of Customs at Chefoo, has obtained permission to form a native company to carry out the work, with the aid of modern appliances. The scheme referred to is understood to include a tramway to the port of Chefoo. Another project on a larger scale has been formed under the auspices of

the famous Li Hung-chang, for developing the coal and iron districts to the north-west of Tientsin, in the Chihli province. Should the enterprises we have alluded to be successfully carried out, some hopes may be entertained of a gradual development of the hidden resources of the Chinese Empire.

ANGOLA.—We learn that a young and energetic collector, Mr. Alfred Heath, started on board the *Biafra*, which sailed from Liverpool last Saturday, for the purpose of exploring the interior of Angola, and obtaining objects of natural history. Mr. Heath will stay at Ambriz for a short time, and make collections on the River Loge and on the coast, after which he will proceed inland to Bembe, a place recommended by the late Mr. Monteiro as presenting an excellent field for the naturalist.

AN ORGAN-PIANO

WHEN recently in Paris I was shown—I believe at the pianoforte factory of M. Herz—a piano with appliances for producing prolonged sounds like those of an organ, which appeared to me to be based on thoroughly sound scientific principles, and which was so great a success that, although the invention had only been perfected a very few weeks before, the firm were receiving orders for the new instruments much faster than they could execute them. The attempt to combine organ sounds with those of a piano has often before, I believe, been made, but usually, if not always, I am told, by combination with the piano arrangements of real organ appliances, the result being, of course, extreme difficulty in obtaining perfect harmony between notes produced by two such totally distinct methods. In the present instance the organ as well as the piano notes are produced by precisely the same means, the principle consisting in producing the organ or prolonged sound by a succession of extremely rapid blows of a hammer upon the same strings as produce the piano note. It will not be difficult, I think (notwithstanding my want of familiarity with such subjects), to make your readers understand exactly how this is accomplished. They will observe that if the pianist were able, instead of merely holding down a key, to produce upon it an extremely rapid succession of blows, far exceeding in rapidity anything which the finger can possibly effect, a prolonged note would be produced, and especially so if the number of blows given was so great as to be practically inseparable by the ear. Now in the instrument of which I am speaking this object is accomplished by means of a series of additional hammers (one to each string) mounted upon watch-spring levers, all of which are carried by a bar of brass lying across, but above and clear of the strings. To this bar is attached a rocking lever which is set in very rapid motion by means of an apparatus worked easily by a pedal. I was not shown the exact nature of this apparatus, but there are so many forms of small engines worked with immense rapidity by compressed air, any one of which would answer the purpose, that no great importance attaches to this point. The *modus operandi* is simple enough: the pianist works the pedal, and thus sets the transverse bar with its series of hammers into excessively rapid vibration. By holding down any key of the instrument, the string belonging to it is brought within range of its corresponding hammer, and is struck with corresponding rapidity, giving out what sounds at a short distance like one prolonged note, which lasts as long as the pedal is worked and the key is kept down. It is easy to see that by this means it is in the power of the pianist to produce either piano or organ notes at will, and although while standing close to the instrument the mode of production of the note could be detected, at a short distance the effect was precisely that of combined piano and organ sounds with the immense advantage of absolute concordance and harmony between the two.

E. J. REED

THE COMING TOTAL SOLAR ECLIPSE

WE have received from Admiral Rodgers, the Superintendent of the U.S. Naval Observatory, the official circular which we were enabled to anticipate some little time ago. Owing to the endeavours of the American astronomers, the Pennsylvania Railroad Company have made arrangements for a reduced rate of fare to and from the East to Denver, Colorado, which is near the central line. These arrangements refer only to persons going in a private capacity, and not to members of expeditions sent out by foreign Governments.

Upon the order of the Superintendent of the U.S. Naval Observatory, Washington, the Company will furnish transportation to Denver and return *viâ* Pittsburg, Indianapolis, St. Louis, and Kansas City, or *viâ* Pittsburg, Chicago, and Omaha, at the following rates for the round trip:—From New York, 73.00 dols.; from Philadelphia, 71.00 dols.; from Baltimore or Washington, 62.50 dols.

The round trip ticket includes transportation (about 2,000 miles according to route) from New York to Denver and back again to point of starting; or in all, 4,000 miles of travel.

These arrangements allow the journey to Denver to be made by one route, and the return by another. The routes available are—

No. 1, *viâ* Pittsburg, Chicago, Omaha, Denver.
No. 2, *viâ* Pittsburg, Chicago, Omaha, Denver.
No. 3, *viâ* Pittsburg, Chicago, Omaha, Denver. (Different routes from Chicago to Omaha.)
No. 4, *viâ* Pittsburg, Chicago, Leavenworth, Denver.
No. 5, *viâ* Pittsburg, Chicago, Atchison, Pueblo, Denver.
No. 6, *viâ* Pittsburg, Indianapolis, St. Louis, Kansas City, Denver.
No. 7, *viâ* Pittsburg, Indianapolis, St. Louis, Kansas City, Denver. (Different routes from St. Louis to Kansas City.)
No. 8, *viâ* Pittsburg, Indianapolis, St. Louis, Kansas City, Pueblo, Denver.
No. 9, *viâ* Pittsburg, Indianapolis, St. Louis, Kansas City, Pueblo, Denver. (Different routes from Kansas City to Pueblo.)

Those intending to make observations of the eclipse, and desiring to take advantage of the courtesy extended by the Pennsylvanian Railroad Company, are requested to notify their intention by letter to the Superintendent of the U. S. Naval Observatory, asking that a letter certifying to their identity be forwarded to their consuls at the port they may select for arrival in the United States. This letter must, upon delivery, be countersigned by the consul of the applicant to prevent mistakes.

The Hon. Secretary of the Treasury has notified the collector of customs of the ports of Boston, Portland, New York, Philadelphia, and Baltimore, of the probable arrival of European observers, who will be identified by exhibiting the above letter, so signed and countersigned, to the collectors named. Orders have been issued to them as follows:—

"Upon the arrival at your port of any of the gentlemen mentioned, you will extend all proper facilities for the speedy delivery to them of the professional instruments in question free of duty and charges."

On presenting the same letter, so countersigned, to the agent of the Pennsylvania Railroad Company in New York, Philadelphia, Baltimore, or Washington, a round-trip ticket to Denver and return, will be issued to them by the Railroad Company at the rates before named.

From Denver railroad lines extend to points likely to be chosen for observing stations.

NOTES

DR. WARREN DE LA RUE, F.R.S., has just made a second donation of 100*l.* to the Research Fund of the Chemical Society, stipulating that the whole sum be devoted to a single object.

MR. ROMANES being prevented, by domestic affliction, delivering his lecture at the Royal Institution, on Friday, the 5th inst., as announced, the Hon. Sec., Mr. W. Spottiswoode, Treasurer of the Royal Society, has undertaken to lecture in his stead, on "Quartz; an old Chapter Re-written."

PROF. A. AGASSIZ has returned from his cruise in the Gulf of Mexico, and in spite of bad weather and the grounding of the *Blake*, he has done fully as much as anticipated. As we have already stated, he made use of steel rope for dredging; the rope, however, was only $1\frac{1}{8}$ inch circumference, not $1\frac{1}{16}$ inch in diameter as we were led to believe. This steel rope came up fully to his expectations, and he is of opinion that hemp rope is not likely to be again used for deep-sea work by any one who has no time to spare. Prof. Agassiz is preparing a preliminary report to the Superintendent of the Coast Survey of his trip.

THE Iceland mail brings intelligence of a great eruption of Mount Hecla. On February 27, at 5 P.M., several smart shocks of earthquake were felt at Reykjavik, and in the same evening flames were visible behind the mountains, in the direction of Hecla. The Rev. Gudmund Jonsson, who lives close to Mount Hecla, states that at 4.30 P.M. of that day, slight shocks of earthquake began to be felt, these gradually increased till about 5 P.M., when two severe shocks occurred, creating a good deal of alarm, but doing no real damage. At 8 P.M. a tremendous eruption of flames appeared on the northern side of Hecla, the flames gradually increasing in size till they appeared like gigantic columns, double the height of the mountain. When the mail left Iceland on March 22, the eruption still continued, but apparently with diminished violence.

COPIOUS rains have fallen in the eastern districts of Cape Colony, and hopes are now entertained of the speedy termination of the disastrous drought referred to in NATURE, vol. xvii. p. 436. The heat in the east of the colony during January last is described as the most intense ever known in even that region of recurring scorching droughts.

THE correspondent of the *Scotsman* at Ottawa describes a curious phenomenon which occurred in the end of February at Niagara Falls. In the vicinity of Table Rock the river-bed was dry for hundreds of yards towards the centre of the Horse-shoe Falls, whilst the river below the falls was about twenty-four feet below high-water mark. For three days the appearance of the river both above and below the falls led to the idea that the falls would entirely cease for a time. This extraordinary circumstance was attributed to incessant high winds from N.E. and an ice-gorge at the rapids above damming the waters of the river till its bed was nearly dry. The icicles which hung from the rocks over which the falls are wont to pour, added to the remarkable character of the scene.

A NOTE sent by the Portuguese Government to the French papers states that a recent law has established in Mozambique and Angola a central council of agriculture, a station for conducting experiments, and a professorship of agriculture. In each capital public lectures will be given by the Government professor on zootechny and scientific agriculture.

THE educational authorities of Berlin possess an enormous garden in one of the suburbs of the city for the purpose of supplying all the schools with fresh botanical specimens. The distribution takes place regularly after April 1, and over 4,000,000 plants are required for botanical instruction during the course of the year.

PROF. OSSIAN-BONNET has been appointed to the Chair of Astronomical Physics in the Paris Faculty of Science and Letters.

A NUMBER of Algerian Arab chieftains have decided to visit the Paris Exhibition, and establish there a complete camp. They will bring with them a variety of Arab coursers.

SOME time since we called attention to the opening of a village museum at Castleton, Derbyshire. The mode of its arrangement has attracted a good deal of attention, and we are glad to hear that it is doing good work in the neighbourhood. A series of scientific lectures in connection with it has been very successful.

THE construction of the Tuileries captive balloon is attracting much attention in Paris. The necessary excavations for the rope-winding roller, the steam-engines, pulley, &c., &c., have modified the appearance of the old Tuileries yard. A large wooden saloon has been erected for the sewing of the canvas, which is quite ready; not less than 100 girls will be required for about a month. The work of making the rope, which is almost finished, has been immense. The weight of the netting will be 3,000 kilograms more than the displacement of the largest balloon in use. Besides the netting, the other ropes connecting the car, &c., will weigh 2,000 kilograms, and the large rope for mooring the balloon to the steam winding apparatus will be 3,000 kilograms. Experiments will be made to show that the rope can bear a traction of 50,000 kilograms, although it is not intended to ascend when the effort to move the balloon will exceed 12,000 kilograms. The real steam power required will be 300 horse-power. The displacement of the balloon will vary according to its station; on the ground it will be 24,430 cubic metres, but, floating at 600 metres in the air, it it will be 25,000.

SIR GEORGE AIRY sends to the *Times* of Saturday a paper giving an account of the public standards of length now mounted, by authority of the Corporation of the City of London, in the Guildhall, and of the care that has been taken to insure their accuracy. The standards consist of a line of 100 feet divided into tens of feet, and a line of 66 feet divided into tens of links, with some smaller divisions, on the floor of the Guildhall; and measures of three feet, two feet, and one foot, with subordinate divisions, on the north wall of the Guildhall. The lengths have been verified with the most scrupulous care by the officers of the Standards Department of the Board of Trade, and there is reason to believe that even the longest is not in error to the amount of one-hundredth of an inch. Sir George Airy has inspected these standards, and is satisfied with the general excellence of their construction.

THE meteor which was observed by Mr. Elliot at Hawick (p. 425) on March 25, at 10·20 A.M., was observed in various parts of Scotland, at Dunbar, Dundee, Cupar-Angus, and different parts of Fife. It is described as "apparently" several feet in circumference, cone-shaped, and at Dundee was observed to burst into a thousand fragments when near the earth.

M. KRANTZ, the Director-General of the Paris Exhibition, has been elected the president of a society for scientific excursions and demonstrations at the Champ de Mars. A circular has been issued by this organisation, which contains among its members a large number of influential scientific, industrial, and literary men. It is intended to organise a number of tours in the several sections under the guidance of experienced and competent teachers, the number of auditors admitted to each tour being limited to thirty. The charges will be very low, the society expecting to obtain for its professors and tourists a diminution of the entrance fee. Any communication may be sent to M. La Motte, editor, the secretary of the Association d'Excursions Scientifiques, Quai des Augustins, Paris. This society has been

sanctioned by the Ministry of Public Instruction, and these excursions are quite distinct from the lectures which will be organised on a large scale, as we mentioned a few weeks ago.

THE museum in the Paris Jardin des Plantes has ately been enriched by two very valuable collections. The first includes a vast variety of anthropological and ethnographical objects gathered by M. Pinart during his voyages in Polynesia, among which might be mentioned more especially the ancient stone statues from Easter Island, executed by a race unknown to the present inhabitants. The second consists of over 40,000 specimens in natural history, collected by M. Raffray in New Guinea, chiefly birds and insects.

M. SOLEIL, the well-known optician of Paris, who invented and patented the optical saccharimeter, patronised by Arago, has died at St. Gratian. He was eighty years of age, and had retired for the last twenty years.

ONE of the newly-opened streets in the Luxembourg Gardens, Paris, close to the Observatory, has been called "Rue Herschel," as a compliment to English astronomy.

IN the February session of the Deutsche anthropologische Gesellschaft, Prof. Bastian gave an interesting address on the occurrence of similar weapons among widely-separated African tribes, describing more particularly a peculiar kind of javelin, found by Schweinfurth on the eastern coast, by Pogge in the Gaboon region, and by other explorers in the Fan tribe of the interior. On the Gaboon coast it is preserved at present as a fetish, being no longer used. This, as well as other examples, tends to show the common origin of all the African races. The remains of an art closely allied to that of ancient Egypt even, have been discovered on the western coast by Dr. Pogge, who has brought back images, on which the beard and coiffure were the counterparts of those decorating the Egyptian statues 3,000 years ago.

IT has been stated by Mr. Rodwell (NATURE, vol. ix. p. 8), that the ancient Egyptians were acquainted with the principle of the "rider" in the balance. According to M. Wiedemann (*Annalen der Physik*) who has examined over 100 representations of Egyptian balances, this is based on a mistake. The Egyptian balance is a simple equal-armed one; a hook on the upper part of the stand supports a cord with terminal weight, or a plumb-line. In representation (perspective being unknown to the Egyptians), the hook and weight, as seen from the side, were drawn in the plane of the balance, so that the weight, in badly made figures, seems to hang, not from the hook, but from the balance-beam.

WE have received the Report of the Registrar-General of the province of Ontario for 1876. To the usual tables with the Report is added an interesting appendix by Mr. T. H. Monk, on the influence of the weather on the mortality of Toronto. The results show, so far as can be looked for, from one year's mortality numbering only 1,664 deaths, a general correspondence with those obtained by Mr. Buchan and Dr. Arthur Mitchell in their large inquiry into the influence of weather on the mortality of London. We hope Mr. Monk's suggestions will be carried out and that the inquiry will be extended so as to embrace the whole province, the health as well as the mortality of the people, and the registrations of the more prominent, if not of all the diseases, be printed for each week, in order to test more decisively the connection between weather and health and how far changes in the health and mortality of the people and the spread of epidemics may be foretold, as well as changes of weather, now so efficiently carried on in North America.

SINCE Mr. Darwin demonstrated processes similar to digestion in the plant organism, attention has been largely given to the discovery of substances of the nature of ferment in plants. M. van der Harst, of Utrecht, has lately examined the seeds of the garden bean (*Phaseolus vulgaris*) in this respect. He finds in these, when in germination, a ferment which can be extracted by means of glycerine. It has the power of transforming albuminous matter into peptones, and starch meal into glucose. It occurs exclusively in the seed lobes.

A CORRESPONDENT sends us the following extract from a letter of one of the officers of the ship *Newcastle*, of London. It is dated Brisbane, Sunday, 30th December, 1877. "Last Friday (28th) in the afternoon, it came over very black, so we expected a thunderstorm. Well, it came on to blow from the south, and then to hail. At first the hailstones were about the size of a marble, but they continued to increase, until they became as large and exactly the shape of a tomato. The captain weighed three and found that the three together weighed one pound. I was on the poop, under the awning, but the awning was blown adrift, which compelled me to beat a hasty retreat. Nearly all the glass in our large saloon ports on the starboard side is broken. To-day, when I was on shore, the houses in Queen Street, facing the south, looked as though there had been a great fire, not a pane of glass left, and in many cases the frames gone altogether. Of course, the backs of the houses on the other side of the street must have suffered to the same extent. During the squall, which lasted about three-quarters of an hour, the river was one mass of foam, caused by the hailstones raining upon its surface in such numbers."

AN interesting archæological discovery has been made at Cancello in the neighbourhood of Naples, by the uncovering of the cemetery of the ancient city of Luessula. The excavations made thus far have brought to light an immense number of interesting objects of ancient Greek civilisation. At Clermont-Ferrand, also, in Southern France, an old Roman villa has been laid bare and found to possess a rich treasure in the way of ornaments, &c.

AN interesting geological discovery has recently been made at Donaueschingen (Baden). A complete and very well-preserved skeleton of the prehistoric musk-deer (*Cervus elaphus muscosus*) has been found in the neighbourhood of this little town. The horns are of gigantic size and show over forty ends; it is aserted that this skeleton is the first complete one known.

M. LUIGI PONCI describes, in *L'Elettricista*, a new electric battery of great simplicity. It consists of the usual glass jar and porous cylinder; the latter, however, is filled with a solution of ferrous chloride (35° Beaumé), and has for a pole an iron plate, while the external solution is of ferric chloride (also 35° B.), and contains a carbon pole. The electro-motive power is 0·9 of that of a Daniel cell.

A ST. PETERSBURG correspondent, "C. S.," desiring to purchase a dictionary of chemistry, writes that he would gladly avail himself of a critical comparison of existing works of the kind. He suggests that a comparative estimate might be given through the pages of this journal. At the same time one of our Paris correspondents writes us on the appearance in Paris of the 25th number of the French "Dictionnaire de Chimie pure et appliquée," edited by Prof. Würtz; closing with the article on Vanadium. This important work was commenced by Prof. Würtz in 1869, assisted by a corps of twenty-five leading French chemists, and although delayed materially by the war and its results, has been pushed forward vigorously, until it is now on the eve of completion. It will form altogether five volumes, numbering nearly 5,000 pages, and will be the first record of chemistry approaching completeness in the French language. The chemist is still dependent in a great measure on the English language, for the seven bulky volumes of Watts's "Dictionary," including its two supplements, form the most extensive as well as most recent

compendium of chemical knowledge. Although Germany takes the lead in regard to chemical discovery, she is far behindhand in this respect. The new edition of the "Handwörterbuch," based on the well-known work of Liebig, Wöhler, and Kolber, now edited by Prof. Fehling, was commenced in 1871, but has progressed at a snail's pace, being only half way through the letter E, and the second of the six volumes which it will compose, not being yet completed. The Italian chemists have recently issued a dictionary of chemistry on a somewhat smaller scale than those alluded to above, but well edited and written.

THE additions to the Zoological Society's Gardens during the past week include two Pudua Deer (*Cervus humilis*) from Chili, a Black-faced Spider-Monkey (*Ateles ater*) from East Peru, deposited; an Arabian Gazelle (*Gazella arabica*) from Arabia, presented by Mr. W. W. Webb.

FOG SIGNALS[1]

DURING the long, laborious, and, I venture to think, memorable series of observations conducted under the auspices of the Elder Brethren of the Trinity House at the South Foreland in 1872 and 1873, it was proved that a short $5\frac{1}{2}$ inch howitzer, firing 3 lbs. of powder, yielded a louder report than a long 18-pounder firing the same charge. Here was a hint to be acted on by the Elder Brethren. The effectiveness of the sound depended on the shape of the gun, and as it could not be assumed that in the howitzer we had hit accidentally upon the best possible shape, arrangements were made with the War Office for the construction of a gun specially calculated to produce the loudest sound attainable from the combustion of 3 lbs. of powder. To prevent the unnecessary landward waste of the sound, the gun was furnished with a parabolic muzzle, intended to project the sound over the sea, where it was most needed. The construction of this gun was based on a searching series of experiments executed at Woolwich with small models, provided with muzzles of various kinds. The gun was constructed on the principle of the revolver, its various chambers being loaded and brought in rapid succession into the firing position. The performance of the gun proved the correctness of the principles on which its construction was based.

Coincident with these trials of guns at Woolwich gun-cotton was thought of as a possibly effective sound-producer. From the first, indeed, theoretic considerations caused me to fix my attention persistently on this substance; for the remarkable experiments of Mr. Abel, whereby its rapidity of combustion and violently explosive energy are demonstrated, seemed to single it out as a substance eminently calculated to fulfil the conditions necessary to the production of an intense wave of sound. What those conditions are we shall now more particularly inquire, calling to our aid a brief but very remarkable paper, published by Prof. Stokes in the *Philosophical Magazine* for 1868.

A sound wave consists essentially of two parts—a condensation and a rarefaction. Now air is a very mobile fluid, and if the shock imparted to it lack due promptness, the wave is not produced. Consider the case of a common clock pendulum, which oscillates to and fro, and which therefore might be expected to generate corresponding pulses in the air. When, for example, the bob moves to the right, the air to the right of it might be supposed to be condensed, while a partial vacuum might be supposed to follow the bob. As a matter of fact, we have nothing of this kind. The air particles in front of the bob retreat so rapidly, and those behind it close so rapidly in, that no sound-pulse is formed.

The more rapid the shock imparted to the air, the greater is the fractional part of the energy of the shock converted into wave motion. And as different kinds of gunpowder vary considerably in their rapidity of combustion, it may be expected that they will also vary as producers of sound. This theoretic inference is completely verified by experiment. In a series of preliminary trials conducted at Woolwich on the 4th of June, 1875, the sound-producing powers of four different kinds of powder were determined. In the order of their sizes they bear the names respectively of Fine-grain (F.G.), Large-grain (L.G.), Rifle Large-grain (R.L.G.), and Pebble-powder (P.). The charge in each case amounted to $4\frac{1}{2}$ lbs., four 24-pound howitzers being employed to fire the respective charges. There were eleven observers, all of whom, without a single dissentient, pronounced the sound of the fine-grain powder loudest of all. In the opinion of seven of the eleven the large-grain powder came next; seven also of the eleven placed the rifle large-grain third on the list; while they were again unanimous in pronouncing the pebble-powder the worst sound-producer. These differences are entirely due to differences in the rapidity of combustion.

These are some of the physical reasons why gun-cotton might be regarded as a promising fog-signal. Firing it as we have been taught to do by Mr. Abel, its explosion is more rapid than that of gunpowder. In its case the air-particles, alert as they are, will not, it may be presumed, be able to slip from places of condensation to places of rarefaction with a rapidity sufficient to forestall the formation of the wave.

As regards explosive material, and zealous and accomplished help in the use of it, the resources of Woolwich Arsenal have been freely placed at the disposal of the Elder Brethren. Gen. Campbell, Gen. Younghusband, Col. Fraser, Col. Maitland, and other officers, have taken an active personal part in the investigation, and in most cases have incurred the labour of reducing and reporting on the observations. Guns of various forms and sizes have been invoked for gunpowder, while gun-cotton has been fired in free air, and in the foci of parabolic reflectors.

On February 22, 1875, a number of small guns, cast specially for the purpose—some with plain, some with conical, and some with parabolic muzzles, firing 4 oz. of fine-grain powder, were pitted against 4 oz. of gun-cotton, detonated both in the open and in the focus of a parabolic reflector. The sound produced by the gun-cotton, reinforced by the reflector, was unanimously pronounced loudest of all. With equal unanimity, the gun-cotton detonated in free air was placed second in intensity. Though the same charge was used throughout, the guns differed considerably among themselves, but none of them came up to the gun-cotton either with or without the reflector. A second series, observed from a different distance on the same day, confirmed to the letter the foregoing result.

Meanwhile, the parabolic muzzle-gun, expressly intended for fog-signalling, was pushed rapidly forward, and on March 22 and 23, 1876, its power was tested at Shoeburyness. Pitted against it were a 16-pounder, a $5\frac{1}{2}$-inch howitzer, $1\frac{1}{2}$ lb. of gun-cotton detonated in the focus of a reflector, and $1\frac{1}{2}$ lb. of gun-cotton detonated in free air. On this occasion, nineteen different series of experiments were made, when the new experimental gun, firing a 3-lb. charge, demonstrated its superiority over all guns previously employed to fire the same charge. As regards the comparative merits of the gun-cotton fired in the open, and the gunpowder fired from the best constructed gun, the mean values of their sounds were found to be the same. Fired in the focus of the reflector, the gun-cotton clearly dominated over all the other sound-producers.[1]

The whole of the observations here referred to were embraced by an angle of about 70°, of which 50° lay on the one side and 20° on the other side of the line of fire. The shots were heard by eleven observers on board the *Galatea*, which took up positions varying from 2 miles to $13\frac{1}{2}$ miles from the firing-point. In all these observations, the reinforcing power of the reflector, and of the parabolic muzzle of the gun, came into play. But the reinforcement of the sound in one direction implies its withdrawal from some other direction, and accordingly we find that at a distance of $5\frac{1}{4}$ miles from the firing-point, and on a line, including nearly an angle of 90°, with the line of fire, the gun-cotton in the open beat the new gun; while behind the station, at distances of $8\frac{1}{2}$ miles and $13\frac{1}{2}$ miles respectively, the gun-cotton in the open beat both the gun and the gun-cotton in the reflector. This result is rendered more important by the fact that the sound reached the Mucking Light, a distance of $13\frac{1}{2}$ miles, against a light wind which was blowing at the time.

Theoretic considerations render it probable that the shape of the exploding mass would affect the constitution of the wave of sound. I did not think large rectangular slabs the most favourable shape, and accordingly proposed cutting a large slab into fragments of different sizes, and pitting them against each other. The differences between the sounds were by no means so great as the differences in the quantities of explosive material might lead one to expect. The mean values of eighteen series of

[1] "Recent Experiments on Fog Signals." Abstract of paper read at the Royal Society, March 21. By Dr. Tyndall, F.R.S., Professor of Natural Philosophy in the Royal Institution.

[1] In this case the reflector was fractured by the explosion.

observations made on board the *Galatea* at distances varying from $1\frac{3}{4}$ mile to 4·8 miles, were as follows:—

Weights	4-oz.	6-oz.	9-oz.	12-oz.
Value of sound ...	3·12	3·34	4·0	4·03

These charges were cut from a slab of dry gun-cotton about $1\frac{3}{4}$ inch thick; they were squares and rectangles of the following dimensions:—4 oz., 2 inches by 2 inches; 6 oz., 2 inches by 3 inches; 9 oz., 3 inches by 3 inches; 12 oz., 2 inches by 6 inches.

It is an obvious corollary from the foregoing experiments that on our "nesses" and promontories, where the land is clasped on both sides for a considerable distance by the sea,—where, therefore, the sound has to propagate itself rearward as well as forward—the use of the parabolic gun, or of the parabolic reflector might be a disadvantage rather than an advantage. Here gun-cotton, exploded in the open, forms the most appropriate source of sound. This remark is especially applicable to such lightships as are intended to spread the sound all round them as from central foci. As a signal in rock lighthouses, where neither syren, steam-whistle, nor gun could be mounted, and as a handy fleet-signal, which dispenses with the lumber of special signal-guns, the gun-cotton will prove invaluable. But in most of these cases we have the drawback that local damage may be done by the explosion. The lantern of the rock-lighthouse might suffer from concussion near at hand, and though mechanical arrangements might be devised, both in the case of the lighthouse and of the ship, to place the firing-point of the gun-cotton at a safe distance, no such arrangement could compete, as regards simplicity and effectiveness, with the expedient of a *gun-cotton rocket.* Had such a means of signalling existed at the Bishop's Rock Lighthouse, the ill-fated *Schiller* might have been warned of her approach to danger ten, or it may be twenty, miles before she reached the rock which wrecked her. Had the fleet possessed such a signal, instead of the ubiquitous but ineffectual steam-whistle, the *Iron Duke* and *Vanguard* need never have come into collision.

It was the necessity of providing a suitable signal for rock lighthouses, and of clearing obstacles which cast an acoustic shadow, that suggested the idea of the gun-cotton rocket to Sir Richard Collinson, Deputy Master of the Trinity House. That idea was to place a disk or short cylinder of the gun-cotton, which had proved so effectual at low levels, in the head of a rocket, the ascensional force of which should be employed to carry the disk to an elevation of 1,000 feet or thereabouts, where by the ignition of a fuse associated with a detonator, the gun-cotton should be fired, sending its sound in all directions vertically and obliquely down upon earth and sea. The first attempt to realise this idea was made on July 18, 1876, at the firework manufactory of the Messrs. Brock, at Nunhead. Eight rockets were then fired, four being charged with 5 oz. and four with $7\frac{1}{2}$ oz. of gun-cotton. They ascended to a great height, and exploded with a very loud report in the air. On July 27, the rockets were tried at Shoeburyness. The most noteworthy result on this occasion was the hearing of the rockets at the Mouse Lighthouse, $8\frac{1}{2}$ miles E. by S., and at the Chapman Lighthouse, $8\frac{1}{2}$ miles W. by N.; that is to say, at opposite sides of the firing-point.

On December 13, 1876, and again on March 8, 1877, comparative experiments of firing at high and low elevations were executed. The gun-cotton near the ground consisted of $\frac{1}{2}$-lb. disks suspended from a horizontal iron bar about $4\frac{1}{2}$ feet above the ground. The rockets carried the same quantity of gun-cotton in their heads, and the height to which they attained, as determined by a theodolite, was from 800 to 900 feet. The day last-mentioned was cold, with occasional squalls of snow and hail, the direction of the sound being at right angles to that of the wind. Five series of observation were made on board the *Vestal* at distances varying from three to six miles. The mean value of the explosions in the air exceeded that of the explosions near the ground by a small but sensible quantity. At Windmill Hill, Gravesend, however, which was nearly to leeward, and $5\frac{1}{2}$ miles from the firing-point, in nineteen cases out of twenty-four the disk fired near the ground was loudest; while in the remaining five the rocket had the advantage.

Towards the close of the day the atmosphere became very serene. A few distant cumuli sailed near the horizon, but the zenith and a vast angular space all round it were absolutely free from cloud. From the deck of the *Galatea* a rocket was discharged, which reached a great elevation, and exploded with a loud report. Following this solid nucleus of sound was a continuous train of echoes, which retreated to a continually greater distance, dying gradually off into silence after seven seconds' duration. These echoes were of the same character as those so frequently noticed at the South Foreland in 1872-73, and called by me "aerial echoes."

On March 23, the experiments were resumed, the most noteworthy results of this day's observations being that the sounds were heard at Tillingham, 10 miles to the N.E.; at West Mersea, $15\frac{3}{4}$ miles to the N.E. by E.; at Brightlingsea, $17\frac{1}{2}$ miles to the N.E.; and at Clacton Wash, $20\frac{1}{2}$ miles to the N.E. by $\frac{1}{2}$ E. The wind was blowing at the time from the S.E. Some of these sounds were produced by rockets, some by a 24-lb. howitzer, and some by an 8 inch Maroon.

In December, 1876, Mr. Gardiner, the managing director of the Cotton-powder Company, had proposed a trial of this material against the gun-cotton. The density of the cotton, he urged, was only 1·03, while that of the powder was 1·70. A greater quantity of explosive material being thus compressed into the same volume, Mr. Gardiner thought that a greater sonorous effect must be produced by the powder. At the instance of Mr. Mackie, who had previously gone very thoroughly into the subject, a Committee of the Elder Brethren visited the cotton powder manufactory, on the banks of the Swale, near Faversham, on June 16, 1877. The weights of cotton powder employed were 2 oz., 8 oz., 1 lb., and 2 lbs., in the form of rockets and of signals fired a few feet above the ground. The experiments throughout were arranged and conducted by Mr. Mackie. Our desire on this occasion was to get as near to windward as possible, but the Swale and other obstacles limited our distance to $1\frac{1}{2}$ mile. We stood here E.S.E. from the firing-point while the wind blew fresh from the N.E. The cotton-powder yielded a very effective report. The rockets in general had a slight advantage over the same quantities of material fired near the ground. The loudness of the sound was by no means proportional to the quantity of the material exploded, 8 oz. yielding very nearly as loud a report as 1 lb. The "aerial echoes," which invariably followed the explosion of the rockets, were loud and long-continued.

On October 17, 1877, another series of experiments with howitzers and rockets was carried out at Shoeburyness. The charge of the howitzer was 3 lbs. of L.G. powder. The charges of the rockets were 12 oz., 8 oz., 4 oz., and 2 oz. of gun-cotton respectively. The gun and the four rockets constituted a series, and eight series were fired during the afternoon of the 17th. The observations were made from the *Vestal* and the *Galatea*, positions being assumed which permitted the sound to reach the observers with the wind, against the wind, and across the wind. The distance of the *Galatea* varied from three to seven miles, that of the *Vestal*, which was more restricted in her movements, being from two to three miles. Briefly summed up, the result is that the howitzer, firing a 3-lb. charge, which it will be remembered was our best gun at the South Foreland, was beaten by the 12-oz. rocket, by the 8-oz. rocket, and by the 4-oz. rocket. The 2-oz. rocket alone fell behind the howitzer.

On the following day, viz., October 18, we proceeded to Dungeness with the view of making a series of strict comparative experiments with gun-cotton and cotton-powder. Rockets containing 8 oz., 4 oz., and 2 oz. of gun-cotton had been prepared at the Royal Arsenal; while others, containing a similar quantity of cotton-powder, had been supplied by the Cotton-powder Company at Faversham. With these were compared the ordinary 18-pounder gun, which happened to be mounted at Dungeness, firing the usual charge of 3 lbs. of powder, and a syren.

From these experiments it appeared that the gun-cotton and cotton-powder were practically equal as producers of sound.

The effectiveness of small charges was illustrated in a very striking manner, only a single unit separating the numerical value of the 8-oz. rocket from that of the 2-oz. rocket. The former was recorded as 6·9 and the latter as 5·9, the value of the 4-oz. charge being intermediate between them. These results were recorded by a number of very practised observers on board the *Galatea.* They were completely borne out by the observations of the Coastguard, who marked the value of the 8-oz. rocket 6·1, and that of the 2-oz rocket 5·2. The 18-pounder gun fell far behind all the rockets, a result probably to be in part ascribed to the imperfection of the powder. The performance of the syren was, on the whole, less satisfactory than that of the rocket. The instrument was worked, not by steam of 70 lbs. pressure, as at the South Foreland, but by compressed air, beginning with 40 lbs. and ending with 30 lbs. pressure. The trumpet was pointed to windward, and in the

axis of the instrument the sound was about as effective as that of the 8-oz. rocket. But in a direction at right angles to the axis, and still more in the rear of this direction, the syren fell very sensibly behind even the 2-oz. rocket.

These are the principal comparative trials made between the gun-cotton rocket and other fog-signals; but they are not the only ones. On August 2, 1877, for example, experiments were made at Lundy Island with the following results. At two miles distant from the firing point, with land intervening, the 18-pounder, firing a 3 lb. charge, was quite unheard. Both the 4-oz. rocket and the 8-oz. rocket, however, reached an elevation which commanded the acoustic shadow, and yielded loud reports. When both were in view, the rockets were still superior to the gun. On August 6, at St. Ann's, the 4-oz. and 8-oz. rockets proved superior to the syren. On the Shambles Light-vessel, when a pressure of 13 lbs. was employed to sound the syren, the rockets proved greatly superior to that instrument. Proceeding along the sea-margin at Flamboro' Head, Mr. Edwards states that at a distance of $1\frac{1}{4}$ mile, with the 18-pounder gun hidden behind the cliffs, its report was quite unheard, while the 4-oz. rocket, rising to an elevation which brought it clearly into view, yielded a powerful sound in the face of an opposing wind.

On the evening of February 9, 1877, a remarkable series of experiments was made by Mr. Prentice, at Stowmarket, with the gun-cotton rocket. From the report with which he has kindly furnished me I extract the following particulars. The first column in the annexed statement contains the name of the place of observation, the second its distance from the firing-point, and the third the result observed:—

Stoke Hill, Ipswich ...	10 miles	Rockets clearly seen and sounds distinctly heard 53 seconds after the flash.
Melton	15 ,,	Signals distinctly heard. Thought at first that sounds were reverberated from the sea.
Framlingham	18 ,,	Signals very distinctly heard, both in the open air and in a closed room. Wind in favour of sound.
Stratford. St. Andrews..	19 ,,	Reports loud; startled pheasants in a cover close by.
Tuddenham. St. Martin	10 ,,	Reports very loud; rolled away like thunder.
Christ Church Park ...	11 ,,	Report arrived a little more than a minute after flash.
Nettlestead Hall	6 ,,	Distinct in every part of observer's house. Very loud in the open air.
Bildestone...	6 ,,	Explosion very loud, wind against sound.
Nacton	14 ,,	Reports quite distinct—mistaken by inhabitants for claps of thunder.
Aldboro	25 ,,	Rockets seen through a very hazy atmosphere; a rumbling detonation heard.
Capel Mills	11 ,,	Reports heard within and without the observer's house. Wind opposed to sound.
Lawford	$15\frac{1}{2}$,,	Reports distinct: attributed to distant thunder.

It is needless to dwell for a moment on the advantage of possessing a signal commanding ranges such as these.

The explosion of substances in the air, after having been carried to a considerable elevation by rockets, is a familiar performance. In 1873, moreover, the Board of Trade proposed a light-and-sound rocket as a signal of distress, which proposal was subsequently realised, but in a form too elaborate and expensive for practical use. The idea of the gun-cotton rocket with a view to signalling in fogs is, I believe, wholly due to the Deputy Master of the Trinity House.[1] Thanks to the skilful aid given by the authorities of Woolwich, by Mr. Prentice, and Mr. Brock, that idea is now an accomplished fact, a signal of great power, handiness, and economy, being thus placed at the service of our mariners. Not only may the rocket be applied in association with lighthouses and lightships, but in the Navy also it may be turned to important account. Soon after the loss of the *Vanguard* I ventured to urge upon an eminent naval officer the desirability of having an organised code of fog-signals for the fleet. He shook his head doubtingly, and referred to the difficult of finding room for signal-guns. The gun-cotton rocket completely surmounts this difficulty. It is manipulated with ease and rapidity, while its discharges may be so grouped and combined as to give a most important extension to the voice of the admiral in command.

I have referred more than once to the train of echoes which accompanied the explosion of gun cotton in free air, speaking of them as similar in all respects to those which were described for the first time in my report on fog-signals, addressed to the Corporation of Trinity House in 1874.[1] To these echoes I attached a fundamental significance. There was no visible reflecting surface from which they could come. On some days, with hardly a cloud in the air, and hardly a ripple on the sea, they reached us with magical intensity. As far as the sense of hearing could judge, they came from the body of air in front of the great trumpet which produced them. The trumpet-blasts were five seconds in duration, but long before the blast had ceased the echoes struck in, adding their strength to the primitive note of the trumpet. After the blast had ended the echoes continued, retreating further and further from the point of observation, and finally dying away at great distances. The echoes were perfectly continuous as long as the sea was clear of ships, "tapering" by imperceptible gradations to absolute silence. But when a ship happened to throw itself athwart the course of the sound, the echo from the broadside of the vessel was returned as a shock which rudely interrupted the continuity of the dying atmospheric music.

The day on which our latest observations were made was particularly fine. Before reaching Dungeness the smoothness of the sea and the serenity of the air caused me to test the echoing power of the atmosphere. A single ship lay about half a mile distant between us and the land. The result of the proposed experiment was clearly foreseen. It was this. The rocket being sent up, it exploded at a great height; the echoes retreated in their usual fashion, becoming less and less intense as the distance of the surfaces of reflection from the observers increased. About five seconds after the explosion, a single loud shock was sent back to us from the side of the vessel lying between us and the land. Obliterated for a moment by this more intense echo, the aërial reverberation continued its retreat, dying away into silence in two or three seconds afterwards.

I have referred to the firing of an 8-oz. rocket from the deck of the *Galatea*, on March 8, 1877, stating the duration of its echoes to be seven seconds. Mr. Prentice, who was present at the time, assured me that, in his experiments with rockets, similar echoes had been frequently heard of more than twice this duration. The ranges of his sounds alone would render this result in the highest degree probable.

There is not a feature connected with the aërial echoes which cannot be brought out by experiments in the laboratory. I have recently made the following experiment:—A rectangle 22 inches by 12, is crossed by twenty-three brass tubes, each having a slit along it from which gas can issue. In this way, twenty-three low, flat flames are obtained. A sounding reed, fixed in a short tube, is placed at one end of the rectangle, and a "sensitive flame" at some distance beyond the other end. When the reed sounds, the flame in front of it is violently agitated, and roars boisterously. Turning on the gas, and lighting it as it issues from the slits, the air above the flames becomes so heterogeneous that the sensitive flame is instantly stilled by the aërial reflection, rising from a height of 6 inches to a height ot 18 inches. Here we have the acoustic opacity of the air in front of the South Foreland strikingly imitated. Turning off the gas, and removing the sensitive flame to some distance behind the reed, it burns there tranquilly, though the reed may be sounding. Again lighting the gas as it issues from the brass tubes, the sound reflected from the heterogeneous air throws the sensitive flame into violent agitation. Here we have imitated the aërial echoes heard when standing behind the syren-trumpets at South Foreland. The experiment is extremely simple and in the highest degree impresssive.

THE IRON AND STEEL INSTITUTE

THE ninth annual meeting of the members of the Iron and Steel Institute was commenced on Thursday in the rooms of the Institution of Civil Engineers in Westminster. The chair was occupied by Dr. C. W. Siemens, F.R.S., the President of the Institute, and the proceedings were commenced by the reading of the Annual Report of the Council, which stated that the total number of members now exceeds 900, while a steady accession of new members continues, there being 47 proposed for election at the present meeting. The Council referred to the increase of foreign members, which shows the interest taken in the institute by Continental and American metallurgists. An invitation received from M. Tresca on behalf of the Société des Ingénieurs Civils, to visit Paris in the ensuing summer and the concurrent holding of the International Exhibition in that city,

[1] I have proposed that it should be called the "Collinson Rocket."

[1] See also *Philosophical Transactions* for 1874, p. 183.

have induced the Council to recommend that the next autumn meeting should be held in Paris. A sum of 2,318*l.* has been raised by the Institute for the widow and family of Mr. Jones, the late general secretary.

The President stated that the Bessemer medal had been awarded to Prof. Tunner, of Leoben, Austria, in consequence of the great distinction that gentleman had earned for himself in his researches in that branch of science which the Institute so specially represented.

One of the most important papers read was by Mr. I. Lowthian Bell, M.P., F.R.S., on the separation of phosphorus from pig iron. In this paper Mr. Bell detailed his further experiences in his endeavours to eliminate the phosphorus from the iron, its presence having a weakening effect on the metal. Fully five-sixths of the pig iron manufactured in Great Britain is made from ores which, when smelted, give a product containing from three-tenths of a unit to nearly 2 per cent. of phosphorus. When, however, this element exists in pig iron to the extent of much more than one-tenth of a unit per cent. it is unfit for the Bessemer converter—at all events when ordinary spiegel iron, containing 10 or 12 per cent. of manganese, is used for its final purification. Bessemer steel rail-makers are, therefore, obliged to reject iron which formerly sufficed for the manufacture of iron rails, an iron comparatively free from phosphorus being necessary. That, therefore, affected the prosperity of the mines which formerly supplied the rail makers with ore, as well as the blast furnaces which produced the pig iron from that ore. Mr. Bell explained that at the high temperature of the Bessemer converter, while the carbon was removed by the air during its passage through the metal, the phosphorus was not affected. This he stated was also the case to a certain extent in the ordinary refinery furnaces; with a more moderate temperature, however, the conditions which bound carbon and phosphorus with iron were materially changed. The iron was more or less oxidised, and the oxide of iron so formed acted on the carbon and phosphorus. When the phosphorus is removed its loss is accompanied by a separation of the carbon contained in the pig iron. Loss of carbon, however, deprives the metal of its susceptibility of fusion at the temperatures at which the operation of refining and puddling are carried on, and when once the metal is solid the further elimination of phosphorus is very difficult, if not impossible. Mr. Bell expressed the opinion that a lower temperature probably weakened the affinity of phosphorus for iron, as they existed in the crude metal, or strengthened the affinity between oxide of iron and phosphoric acid. A third condition involved in the mere condition of heat might be a diminution of the power possessed by oxide of iron in attacking the carbon, that element which enabled the crude metal to maintain fluidity when moderately heated. The author said that whichever one or more than one of the three conditions was required, the fact remained that melted crude iron might be maintained in contact with melted oxide of iron, and still retain carbon enough to prevent it solidifying, while the phosphorus rapidly disappeared. Instances were given of 95 per cent. of phosphorus being removed, while only 10 per cent of the carbon had been dissipated. The process consists in the more rapid agitation of the two substances while in a liquid condition. The iron so heated may be puddled for the production of malleable iron, or used for the manufacture of steel. Specimens of steel of the highest quality which had been so produced at the Royal Arsenal, Woolwich, were exhibited.

Dr. Percy, F.R.S., gave some particulars as to the manufacture of Japanese copper. Bars of this metal present a beautiful rose-coloured tint on their surface, which is due to an extremely thin and pertinaciously adherent film of red oxide of copper or cuprous oxide. This tint is not in the least degree affected by free exposure to the atmosphere. Dr. Percy placed before the meeting bars which he had possessed for thirty years, and which had undergone no change, although freely exposed to the atmosphere. The secret of this result lies in casting the copper under water, the metal being very highly heated and the water being also made hot. Dr. Percy stated that he had succeeded in casting copper in this way, and had produced similar results to those shown in the Japanese metal.

Other papers read were:—"On some Recent Improvements in the Manufacture of Iron Sponge by the Blair Process," by Mr. J. Ireland; "Statistics on the Production and Depreciation of Rails," by Mr. Charles Wood; "On Steel-casting Apparatus," by Mr. Michael Scott; "On Railway Joints," by Mr. C. H. Halcomb; and "On the Manufacture of Bessemer Steel and Steel Rails," by Mr. C. B. Holland.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—The Council of the Senate recommend that the application of Prof. C. C. Babington for skilled assistance at the Botanical Museum be granted, and that an assistant curator of the Herbarium be appointed at a salary of 100*l.* per annum, the appointment to be made by the Professor with the consent of the Vice-Chancellor, and to be for a period of four years. It is in contemplation to appoint a non-collegiate student.

BALTIMORE.—The Anniversary of the Johns Hopkins University was celebrated on February 22, when addresses were given by some of the professors and others. So far the progress of the University has been thoroughly satisfactory. One of its principal aims is to encourage original research, both among professors and students, and fellowships are granted to those who show aptitude for such work. Prof. Remsen, in his address, showed that a lofty idea of what original research really is, is entertained at the University; it is not merely the establishing of an isolated fact, the devising of a new piece of apparatus, the simple analysis of a new mineral, the discovery of an extra tooth in some abnormal animal; it is, rather, a systematic attempt to solve a definite problem, involving the use of a variety of methods peculiar to the special branch in which the attempt is made. In the three laboratories, biological, physical, and chemical, a variety of important work is being carried on, and altogether, both in the kind and amount of work which is being done under the auspices of the university, the trustees and professors show that they have a thorough appreciation of the spirit of the founder's legacy.

SOCIETIES AND ACADEMIES

LONDON

Mathematical Society, March 14.—Lord Rayleigh, F.R.S., president, in the chair.—Mr. Artemas Martin, Erie, Pa., was proposed for election.—The Secretary communicated a paper by Prof. J. Clerk Maxwell, on the electrical capacity of a long narrow cylinder and of a disc of sensible thickness. Prof. Cayley, Mr. J. W. L. Glaisher, Mr. Roberts, and the President made short communications.

Royal Astronomical Society, March 8.—Lord Lindsay, president, in the chair.—Mr. Neison read a paper on Hansen's terms of long period in the lunar theory. Mr. Proctor drew some diagrams referring to the position of the axis of Mars, and spoke upon Mr. Stone's paper of last January. Mr. Neison made some illustrative remarks thereon.—A paper by Mr. Plummer was read on the supposed influence of a mass of brickwork upon the errors of a transit instrument in its neighbourhood. Several Fellows commented upon this paper and described the lively behaviour of their transit-piers; Mr. Dunkin said there was nothing new about it.—A paper by Mr. Stone was read on telescopic observations of the Transit of Venus. Mr. Gill spoke on the difficulties concerning contacts, and some discussion followed.—A paper was announced by Prof. Sedley Taylor on Galileo's trial before the Inquisition in the light of recent researches; likewise an atlas of the ecliptic, by Heiss, of stars down to the fifth magnitude on Mercator's projection, made in order to get people to lay down the zodiacal light.—There were several other papers.

Entomological Society, March 6.—H. W. Bates, F.L.S., F.Z.S., president, in the chair.—Mr. John Woodgate was elected a Member of the Society.—Mr. F. Moore, at the request of Sir W. H. Gregory, late governor of Ceylon, exhibited a large series of drawings, executed by native artists, of the transformations of the lepidoptera of the island. These drawings were made under the direction of Dr. Thwaites, and represented, for the first time, the life-history of many species.—Mr. McLachlan exhibited some entomological parts of the great Russian work "Fedtschenkos' Travels in Turkestan."—Mr. H. Goss exhibited a small collection of fossil insects obtained by Mr. Gardner from the Bournemouth leaf beds (middle eocene). The collection comprised numerous elytra of coleoptera, and wings of neuroptera, &c.—Mr. J. Mansel Weale read some notes on South African insects. These referred to variation in *Pieris severina* and *Pieris mesentina*; to the secretion of formic acid in *Termes triniverius*, and the probable localisation of the same in a cephalic process, and also to the larvæ of some Hesperidæ in relation to the subject of protective resemblance.—Mr. Ed. Saunders read a paper entitled "Remarks on the Hairs of some of our British Hymen-

optera." From a microscopical examination the author found that the presence of branched or plumose hairs is characteristic of the Anthophila, whilst the hairs of the Fossores, of Heterogyna, and of the Diploptera, are all simple, or in some cases twisted.—Mr. A. G. Butler communicated a paper on the natural affinities of the lepidopterous family Ægeriidæ. From an examination of structural characters, Mr. Butler considered that these insects presented no resemblance to the Sphingidæ, with which they had hitherto been allied, but were more related to the Pyrales and the Gelechiidæ. The president, in favour of this view, remarked that the whole of the Ægeriidæ had been made to depart from their congeners in appearance, through the action of mimicry.—The Secretary read a paper, by Mr. A. H. Swinton, on the biology of insecta, as determined by the emotions. The paper dealt chiefly with cases of simple muscular contractions and secretions.—Mr. Peter Cameron communicated a paper on some new genera and species of Tenthredinidæ.

Zoological Society, March 5.—Prof. Newton, F.R.S., vice-president, in the chair.—Mr. Sclater exhibited and made remarks on a second collection of birds from Duke of York Island, New Britain and New Ireland, which he had received from the Rev. George Brown, C.M.Z.S.—Mr. Sclater exhibited and made remarks upon a specimen of *Athene variegatæ*, and upon the type-specimen of *Fulica gallinuloides* of King, belonging to the Museum of Science and Art, Edinburgh.—Prof. Newton, F.R.S., drew attention to the statement of Legaut that every Solitaire (*Pezophaps solitaria*) carried a stone in its gizzard, and exhibited one of three stones found by Mr. Caldwell, C.M.Z.S., associated with the remains of as many birds of that species in the caves of Rodriguez.—Mr. T. J. Parker described the stridulating apparatus of *Palinurus vulgaris*, which consisted in a peculiar modification of the second joint of the antennæ working against the lateral surface of the antennulary sternum.—A communication was read from Mr. C. Spence Bate, C.M.Z.S., containing an account of the crustaceans of the Coast of Coromandel, collected by Sir Walter Elliot, K.C.S.I.—Mr. A. Boucard, C.M.Z.S., read notes on some coleoptera of the genus *Plusiotis*, and gave descriptions of three new species from Mexico and Central America.—A communication was read from Mr. Arthur G. Butler, F.Z.S., containing an account of a small collection of lepidoptera, obtained by the Rev. J. S. Whitmee, at the Ellice Islands.—A communication was read from Mr. Edward J. Miers, F.Z.S., on the *Penæidæ* in the collection of the British Museum.—Mr. George French Angas read the description of a new genus of land shells belonging to the family *Cyclophoridæ*, for which he proposed the name of *Mascaria*.—Mr. Angas also read descriptions of nine new species of land and marine shells from various localities, amongst which was a new *Rostellaria*, proposed to be named *R. luteostoma*, and a new *Bulimus* from Madagascar, proposed to be called *B. watersi*.—A communication was read from Dr. G. E. Dobson, C.M.Z.S., containing additional notes on the chiroptera of Duke of York Island and the adjacent parts of New Ireland and New Britain.—A communication was read from Mr. Robert Collett, C.M.Z.S., containing an account of *Latrunculus* and *Crystallogobius*, two remarkable forms of gobioid fishes found in Scandinavia.

Institution of Civil Engineers, March 26.—Mr. Bateman, president, in the chair.—The paper read was on direct acting or non-rotative pumping engines and pumps, by Mr. Henry Davey, Assoc. Inst. C.E.

PARIS

Academy of Sciences, March 25.—M. Fizeau in the chair.—The following papers were read:—Experiments designed to imitate various forms of foldings, distortions, and ruptures presented by stratified rocks, by M. Daubrée. He used an apparatus in which vertical and horizontal pressure could be produced, with screws, on sheets of metal of various thickness (especially lead), also sheets of wax mixed with plaster, resin, turpentine, &c. Various effects of a geological character were obtained.—Craniology: the Tasmanian race, by MM. de Quatrefages and Hamy. This relates to the sixth volume of the author's "Crania Ethnica." The Tasmanians formed a race by themselves, and remarkably homogeneous. Their cranial capacity is considerably over that of the Nubian negroes, yet the latter are socially much above the former. On the whole, the Tasmanian cranium does not present marked signs of inferiority. M. Hamy's measurements were made on at least fifty-four osseous heads and six skeletons.—On the treatment of wounds by occlusion, by M. Ravaisson-Mollien. In the winter of 1869, suffering greatly from chapped hands, he filled the wounds with filaments of wadding and then covered them with collodion. This gave relief and speedy cure. He communicated the fact to M. Nelaton, who, with M. Guerin, was then studying the treatment of wounds with occlusion of air.—Observations on the nature of the plants united in the group of *Nœgerrathia*; generalities and type of *Nœgerrathia foliosa*, Sternb., by M. De Saporta.—On the origin of the Phylloxera discovered at Bades (Eastern Pyrenees), by M. Planchon. This outbreak is shown to be due to introduction of some 500 vine-stocks from Gard, in France, five years ago. It is a mistake to regard the American vines as alone pestiferous. M. Duval Jouve was elected correspondent for the Section of Botany, in room of the late M. Hofmeister.—On a map of the erratic blocks of the valley of the Arboust, ancient glacier of Oo (environs of Lachon, Haute Garonne), by MM. Trutat and Gourdon.—M. Dumas presented fascicle A of Measurements of the photographic negatives of the Transit of Venus.—M. Lecoq de Boisbaudran stated that he had prepared several anhydrous chlorides, bromides, and iodides of gallium. The atomic weight of gallium (according to two experiments) was 69·9.—Results of observations in 1877 on the sun's limb on the lines *b* and 1474 *k*, by M. Tacchini. The mean number of positions daily of these lines shows a minimum in agreement with that of the sun-spots. From maximum to minimum the diminution of visibility of *b* is greater than for 1474 *k*. Iron has an enormous preponderance at the base of the chromosphere; magnesium comes next. The other substances are of comparatively slight frequency, and they nearly disappear at the minimum of spots.—New considerations on the observation and reduction of lunar distances at sea, by MM. Beuf and Perrin.—On the effects of the rheostatic machine, by M. Planté. *Inter alia*, the difference in character of the electricity from the positive pole and that from the negative is more marked than with the electric machine or induction coil. (The forms of the sparks are described.)—On a *camera lucida*, by M. Pellerin. This describes an arrangement (copying M. Cornu's polariser) which gives two images of the same intensity visible at the same time by the whole pupil.—On a hydrate of ether, by M. Tanret. In filtering an etherised solution in free air, a crystallisation occurs at the upper part of the filter. This, got otherwise in larger quantity, was what the author examined, and found a true combination of ether and water of the nature of cryo-hydrates.—On the constitution of wool and some similar products, by M. Schutzenberger. Wool gives a fixed residue presenting the same elementary and immediate composition as that of albumen; the proportions of ammonia, carbonic acid, and oxalic acid are considerably higher than with albumen; acetic acid and pyrrol are in similar proportions.—On the formation of partitions in the stylospores of Hendersonias and Pestalozzias, by M. Crié.—On some new facts of perlitism of rocks, and on the artificial reproduction of perlitic fissures, by MM. Fouqué and Lévy. This reproduction is by treating hydrofluosilicic acid with excess of carbonate of lime, filtering the mixture (slightly diluted with water), receiving a drop of the liquid which passes on a piece of glass covered with Canada balsam, and letting dry.—On the period of rotation of solar spots, by Mr. Brown.—M. Gaiffe presented an apparatus which enables one to determine immediately, by a simple reading, the electromotive force of any electric generator.

CONTENTS

THURSDAY, APRIL 11, 1878

THE APPLICATION OF ELECTRICITY TO RAILWAY WORKING

The Application of Electricity to Railway Working. By William Edward Langdon, Member of the Society of Telegraph Engineers; Superintendent (Engineering Department) Post-Office Telegraphs; and late Superintendent of Telegraphs on the London and South-Western Railway. (London: Macmillan and Co., 1877.)

IF any proof were needed of the vast and important services that science has conferred upon man, no more eloquent example could be instanced than that great combination of the conceptions of Stephenson and of Volta—the locomotive and the voltaic battery—which combination in its elaborated form is known as the railway system of the present time.

Living as we do in the midst of conveniences of transit, the mere belief in the possibility of which would, fifty years ago, have made a man a fit inmate for a lunatic asylum, we are apt to lose sight of the complexity of the problem that has been solved and to forget the all-important part which science has played in rendering such a state of things not only possible, but an accomplished fact of so familiar a nature as to have become a necessary part of our very existence. But when it is remembered that upon most of the lines of railway in and around London several hundred trains are running daily[1] at intervals varying from three minutes to half an hour, that each of those trains requires a separate series of signals only to protect it from collision, and that interspersed with the regular traffic "specials," "light engines," and trains out of time have to be provided for and protected against (to say nothing of the goods traffic, or of shunting, crossing and junction operations), it will be readily understood that traffic management, holding in its hands the power of life and death, is no easy task; and that without some very elaborate combination of sound administrative organisation with scientific instrumental aid, the traffic of a single hour would soon become an inextricable tangle of confusion.

Notwithstanding the great importance of the subject, involving as it does the safety of millions of human lives, it is somewhat surprising that technical literature should hitherto have been devoid of a work upon the very essence of safety in railway working—the application of the electric telegraph and of electric signalling to traffic management. This need has now been very ably supplied by the work before us, every page of which bears upon its face the evidence of being written by a thoroughly practical master of the subject in all its details and ramifications, and at the same time by one who possesses an exceptional power of making the subject clear to his readers.

In a handbook of a particular application of electricity it is refreshing to find that no valuable space is occupied by matter to be found in every elementary text-book of physics, that neither Thales with his amber nor Galvani with his frogs are even mentioned, and that descriptions of the various forms of the voltaic battery find no place in the book. The author presupposes that the necessary elementary knowledge is possessed or can be obtained by his readers, and disposing in one page of a few necessary introductory definitions plunges at once into his subject.

The work is arranged in three principal divisions:—(1) Speaking telegraphs; (2) Block signalling; and (3) Miscellaneous appliances. Under the first division a chapter is devoted to descriptions of the various speaking instruments and of the methods by which they are worked. The second chapter treats of signalling regulations, and while being of special value to all professionally engaged in railway working must prove most instructive and interesting to outsiders, who are thereby let into some of the technical mysteries of telegraphy. Every one is familiar with blank spaces left at the head of the telegraph forms issued by the Post Office, against which are printed the words "Prefix," "Code time," "Words," &c., but comparatively few know their meaning. The *Prefix* to a telegram is a signal letter or abbreviation to indicate the character of the message which follows, and therefore the order of its precedence for transmission. The *Code time* is a similar abbreviation to indicate the exact time at which a communication is handed to the telegraph clerk for transmission; and the space marked "Words" is set apart for signalling to the distant station the number of words contained in a message which gives to the receiving clerk a check upon his correct reading of the signals by which the communication is transmitted.

In railway telegraphy the prefix D.R. (Danger) gives to the message precedence over all others, and should never be employed except in cases of great emergency. Other prefixes SP. (for special service), DB. (for ordinary traffic), and various others are employed in railway signalling, by which the degree of its urgency is indicated before the message itself is transmitted.

The system upon which the Code time is abbreviated is very ingenious, and will be readily understood by referring to Fig. 1, which we have borrowed from Mr. Langdon's book. Opposite the hour figures on the dial of a clock are placed the twelve letters, A, B, C, D, E, F, G, H, I, K, L, and M, and against the four minute divisions between the hour figures, are placed the letters R, S, W, X, which, as will be seen in the sketch, are repeated all round the dial A simple time-code is thus obtained, by which any hour or minute throughout the day can be expressed in from one to three letters; thus 2 o'clock would be signalled by B, 2.45 by B I, and 7.12 (the time shown in the figure) by G, B, S, that is, G for seven hours, B for ten minutes, and S for the remaining two minutes to make up the twelve.

The technical regulations for railway telegraphing and traffic management are treated very fully. In this the author's large experience from having had the superintendence of the telegraphs of one of the most important lines of railway in the country is most apparent and gives great weight to his remarks, which ought to be committed to memory by all concerned in the management of railways; for if rigidly enforced and carried out, railway accidents would become well-nigh impossible, except from failure of instruments, from the breaking-down of rolling-stock, or from damage to permanent way.

[1] During some portions of the day as many as seventy-five trains run through Clapham Junction Station in an hour, and between 900 and 1,000 is the daily aggregate average.

The second division of the book is devoted to the consideration of the block system, first conceived by Sir William Fothergill Cooke, and to the instruments and regulations by which that system which is the great guardian of the safety of railway travelling is carried out. It begins with a short historical notice of the subject, and, after explaining some of the elementary principles upon which the various instruments are constructed, proceeds to describe the different systems for carrying the block system into effect. The chapters devoted to this subject are embellished by a large number of excellent illustrations; each system being treated in a chapter to itself, which is a tolerably complete treatise on the subject.

FIG. 1.

The beautiful arrangements of Mr. Preece, in which the indications of the signalling instruments as well as their manipulation are identical with those of the outdoor signals, are clearly described, as well as the systems of Mr. Walker, of Messrs. Tyer, and of Mr. Spagnoletti, all of which are very extensively used in this country. The system of Messrs. Siemens Brothers so largely employed on the Continent, a description of which concludes this part of the book, is specially remarkable for the fact that in it batteries are dispensed with, the necessary electric currents for working the instruments being derived from small magneto-electric machines.

FIG. 2.

The various schemes that have been devised for making the train work its own signals, either by depressing "treadles" on the line, or by otherwise making electrical contacts, form a very interesting chapter, in which the systems of Mr. Imray, of London, of Mr. Rousseau, of New York, and of Dr. Whyte, of Elgin, are described and rendered clear by means of drawings and diagrams of the apparatus.

The essential principle of what is known as the block-system, is the insuring of there always being a certain distance between two trains travelling on the same line of rails. To carry this out the line is divided into a number of sections or "blocks," and the traffic is so regulated, that it is impossible for two trains to be in the same section at the same time. As a train enters one section, the signal behind it is set at danger, and is not lowered until the train has passed into the next section, which is similarly protected, and thus throughout the whole of its course a train cannot follow it at a distance less than the length of a section, or the distance between signal and signal. This is the one principle of the block system and all the various arrangements devised by different inventors differ only in the details by which it is carried out.

In Rousseau's arrangement, which may be taken as a

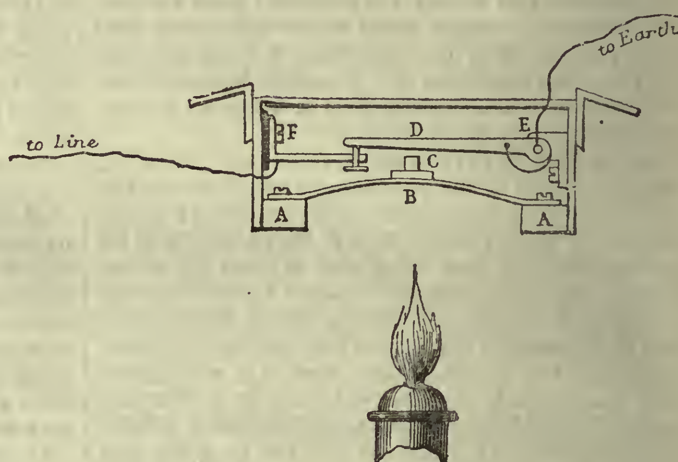


FIG. 3.

type of the automatic systems of block signalling, the train in its progress depresses treadles on the line, which, by making electrical contacts with suitable apparatus, set the signals at danger as the train enters one section, and releases them as it passes into the next. A general idea of this system may be obtained by referring to Fig. 2, in which A, B, and C represent three signals, and the spaces A B and B C two sections of the line; at *a* is a treadle by which A is set at danger, and at *a'* is another by which it is released; similarly a treadle at *b* sets the signal B at danger, and a

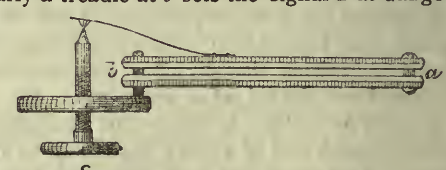


FIG. 4.

second at *b'* lowers it to the *all clear* position. A train, therefore, in passing *a*, which it does just before entering A B, will block that section against following trains by the signal A; travelling to B it will, in passing *b*, set B at danger, and not until it passes *a'*, when it is well out of the section A B, can the signal A be set at *all clear*, permitting a following train to enter A B.

In the system of M. Brunius, which is under trial on the state railways of Sweden, telegraphic communication is made between the stations and the engine of the train, so that not only can ordinary signals be transmitted to

the engine-driver, but he can receive instructions by telegraph.

Miscellaneous appliances employed in railway telegraphy are treated in the third division of Mr. Langdon's book, and an interesting chapter is devoted to the various "signal repeaters" and "light recorders." By the former, which were first employed by Mr. Preece, the position of the out-door signals is reproduced in miniature within the signal box, so that the signalman knows at once if the outside signals are correct. Light recorders are instruments which give warning, within the signal box, of the extinction of the light of the outside night-signals. Of these several forms have been devised but they all depend upon the expansion of metallic substances when subjected to the influence of heat and their subsequent contraction when that heat is removed. Fig. 3 is a cross-section of the transmitting portion of one of these instruments, in which B represents a concave disc of copper attached by its edge to the ring A A, a short distance above the flame of the lamp. When the light is out the lever D rests on the contact screw attached to the arm F (as shown in the figure) and the circuit is closed between the "line" and the earth, and an electric bell is set ringing in the signal box at the same time as an instrument indicates the words "*Light out*." When, however,

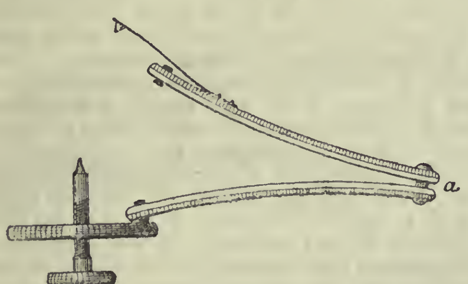


FIG. 5.

the plate B is heated by the flame below it, it becomes expanded, lifting the stud C, which, pressing against the lever D, lifts it off the contact screw, thereby breaking the circuit. The bell ceases ringing and the indicating instrument falls back to the signal "Light *in*."

Figs. 4 and 5 represent a different arrangement for producing the same result, in which the contact-maker consists of two compact bars of dissimilar metals, constructed after the manner of a metallic pyrometer, and united together at the end A with the similar metals facing one another. By this arrangement the arc of motion is largely increased and the instrument is in consequence rendered more sensitive. Under the influence of heat the bars curve in opposite directions, as shown in Fig. 5, but on the light becoming extinguished their differential contraction brings them to the position shown in Fig. 4, contact is established, the bell rings, and the signal "Light *out*," is transmitted to the receiving station.

The important subjects of the interlocking of points and signal levers, of level crossings, and the working of railway yards, all find their place in Mr. Langdon's book; and very interesting chapters are devoted respectively to the various kinds of electric bells, to lightning protectors for telegraphic instruments, and for the methods devised by different inventors and adopted by different railway companies for establishing intercommunication in trains.

It is not easy within the limits of the space at our disposal to do anything like justice to Mr. Langdon's most useful work, which is a thorough exposition of the subject in all its branches by one who not only has had a very large practical experience of the application of electricity to railway working, but who has the gift of clear description and a power of interesting his readers.

To all engaged in railway management whether directors, engineers, traffic managers, station-masters, signal-men, engine-drivers, or guards, Mr. Langdon's work will become a necessary text-book and book of reference, and the general scientific reader will find it most interesting and instructive. We must congratulate its author upon having put so much valuable information in so small a space, and its publishers upon having issued it in so cheap and attractive a form. C. W C.

TROLLOPE'S "SOUTH AFRICA"

South Africa. By Anthony Trollope. (London: Chapman and Hall.)

THERE are probably few of our Colonies the relations of which to one another are so little understood by the general public as those of South Africa, and none where events of so extraordinary a nature have occurred within the last few years. There are few Englishmen, therefore, by whom these charming volumes will not be read with delight and interest, coming as they do from a man of so much experience and of such liberal views as Mr. Trollope. The arrangement of the book is good and clear, each of the colonies being treated separately; a few chapters being devoted to a general introduction, and a few to the native tribes. The author has been led thereby into a somewhat unnecessary, possibly unconscious, repetition, when introducing each new district to his readers. This clearly arises from the fact that the origin of each colony is the same—the desire of the Boers to free themselves from British rule, their consequent occupation of new lands beyond the English border, and the necessity of our ultimately stepping in to govern them, both for their own good and for that of the natives. Mr. Trollope states that the objects of his interest are men and women, and it is to learn their condition, both socially and politically, that he visited South Africa.

Cape Colony, the oldest, largest, and most flourishing one, contains at present about 750,000 inhabitants, one third only of whom are white, and of the latter but one-third are English. These numbers indicate at once the very slow progress of the colony, and show that it is far from popular amongst emigrants, which Mr. Trollope thinks is due to the fact that here, and here only, the white labourer has to compete on equal terms with the native. The country seems closely to resemble the Riviera, though on a larger scale, both in scenery and capabilities. A great deal of the best lands, about 80,000,000 acres, is in private hands, of which only 550,000 acres are cultivated, being 1-145th of the private lands, and not one-fourteenth as Mr. Trollope has it. The great drawback to the country is the want of irrigation works when almost every European plant could be grown. Amongst other things, has not the cultivation of

the olive been tried? We have nowhere seen any account of such an attempt. One would suppose that it would grow well, and in that case could not fail to be very remunerative. The people are well-to-do, and the rate of wages is good. When one, however, compares what is done here and in the United States in the way of irrigation works, in the scientific investigation of the country with reference to mining and agricultural pursuits, and in the collection and examination of the objects of scientific interest, one cannot but feel that there is a sad lack of enterprise and energy in the colony. The Cape Town Museum seems to be in a semi-starved condition.

The white population of Natal is almost entirely English, the Dutch having withdrawn for the most part as soon as the English Government decided on interfering. Sugar seems likely to form the staple of the colony. It is cultivated with the aid of coolie labour, although the Zulus are to the white population as sixteen to one.

In the Transvaal and the Orange Free State the Dutch form the agricultural, the English the town and trading population. Mr. Trollope seems to possess that genial disposition which draws out the bright side of the people with whom he is brought in contact. Although, therefore, he finds the Boer wanting in cleanliness, education, sociability, and enterprise, he finds in him many good points, and is far from thinking him so bad or so hopeless as the author of "The Great Thirst Land." The Boer has improved of late years, and in some cases considerable pains are taken with the education of the children. As Mr. Trollope says, "The Dutch Boer is what he is, not because he is Dutch or because he is a Boer, but because circumstances have isolated him."

Three chapters are devoted to the diamond diggings, and a very interesting plan of the great Colesberg Kopje is given. The author has very little sympathy with diamond-digging, and the only satisfaction he finds there is the civilising influence which the employment of so many natives cannot fail in time to exert. Mr. Trollope has devoted considerable thought and attention to the native question. His opinion is one well worthy of attention, though it is not likely, he thinks, to be regarded with favour either by Exeter Hall or the Colonists whose lands lie uncultivated for want of labour. He visited several of the Missionary Institutions, all of which, with the exception of M. Esselin's self-supporting one at Worcester, seem to have been more or less failures. He thinks that work, steady and regular but voluntary, will be found to be the best and most effective civilising agents. Unfortunately the natives' wants are so few and so easily satisfied, that there is at present no spur to regular work.

The account of Bloemfontein as a sanatorium for consumptive people is that of a man of "heroic mould" equal to the feat of dining twice daily, such as Mr. Trollope must be, seeing that at his age he makes light of, and seems to have enjoyed, the rough travelling by mail-carts, cape-carts, and otherwise, of considerably over two thousand miles. One regrets that he has not mentioned whether there is here the same change between morning, midday, and evening climate as he observed at Pretoria; also whether he came across any consumptive people, and how they fared. He also forgets that deal benches and chairs constructed with an equal regard to human anatomy, judging from the fact that easy chairs cost 13*l.* 10*s.* each, are not the seats most likely to conduce to the comfort of an invalid.

An excellent map accompanies the book. The type, paper, and "get-up" are all that can be desired, and the number of misprints is small. W. J. L.

OUR BOOK SHELF

The Science of Language. By Abel Hovelacque. Translated by A. H. Keane. (Chapman and Hall, 1877.)

We have already had occasion to review at length the original French text of this work, which is now presented in an English dress. M. Hovelacque is one of the most distinguished representatives of the school of comparative philologists who would include their study among the physical sciences, and his book illustrates both the faults and the excellences of the view he upholds. In spite of the limitations thus introduced into the science of language, in spite, too, of the many inaccuracies which occur in his descriptions of the various groups of language at present existing in the world, the clearness and vigour of his style make his book one well worth translating, and it is satisfactory to see that it has been put into competent hands. Mr. Keane has added to the value of the work by a philological map, and a tabulated list of the languages described by M. Hovelacque, together with their characteristics and geographical position. From time to time, too, he has introduced foot-notes and even insertions in the text; many of these give fresh information or correct the statements of the author; others of them, however, had better been left unwritten. Thus his reference to Raabe's attempt to connect Aryan and Semitic grammar is not very happy, and he is unfair towards his author when he accuses him of inconsistency in being at once a Darwinian and a polygenist. No doubt "the impossibility of reducing man now to, say a mollusc, is no argument against the original identity of man with a mollusc" (or rather of his descent from the same form of life as a mollusc); but that is because there are intermediate links and stages of development between the mollusc and man, and M. Hovelacque believes—and with good reason—that such intermediate links do not exist between the manifold families of speech that are scattered over the world.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Age of the Sun in Relation to Evolution

It has been urged by Mr. Plummer (pp. 303 and 360) as a fundamental objection to the theory that sun-stars are formed from the collision of stellar masses, that if the theory be true there ought to be many of the stars moving with great velocities, which he affirms is not the case. But I am unable to understand upon what grounds he bases his assertion. I freely admit that if it could be proved that none of the stars has, as he seems to suppose, a proper motion of more than thirty or forty miles per second, it would at least be a formidable difficulty in the way of accepting the theory. For it would indeed be strange, as Mr. Plummer remarks, "that amid all the diversity of dimensions of the heavenly bodies, it should invariably happen that the resultant movement of the combined masses should be reduced to such insignificant figures as the above." But how does Mr. Plummer arrive at the conclusion that something like this must invariably

have taken place? I fear that before his objection can be fairly urged something more definite must yet be known as to the rate of motion of the stars.

All that we are at present warranted to affirm, I presume, is simply that of the comparatively few stars whose rate of motion has been properly measured, none has a motion greater than thirty or forty miles per second, while nothing whatever is known with certainty as to the rate of motion of the greater numbers of stars. Before we can ascertain the rate of motion of a star from its angular displacement of position in a given time we must know its absolute distance. But it is only of the few stars which show a well-marked parallax that we can estimate the distance, for it is now generally admitted that there is no relation between the apparent magnitude and the real distance of a star. All that we know in regard to the distances of the greater mass of the stars is little more than mere conjecture. Even supposing we knew the absolute distance of a star and could measure its amount of displacement in a given time, still we could not be certain of its rate of motion unless we knew that it was moving directly at right-angles to the line of vision, and not at the same time receding or advancing towards us; and this we could not determine by mere observation. The rate of motion, as determined from its observed change of position, may be, say, only twenty miles a second, while its actual velocity may be ten times that amount.

By spectrum analysis it is true we can determine the rate at which a star may be advancing or receding along the line of sight independently of any knowledge of its distance. But this again does not give us the actual rate of motion unless we are certain that it is moving directly to or from us. If it is at the same time moving transversely to the observer, its actual motion may be more than 100 miles per second, while the rate at which it is receding or advancing, as determined by spectrum analysis, may not be twenty miles a second. But in many cases it would be difficult to ascertain whether the star had a transverse motion or not. A star, for example, 1,000 times more remote than *a* Centauri, that is, twenty thousand billion miles, though moving transversely to the observer at the enormous rate of 100 miles per second, would take upwards of thirty years to change its position so much as 1″ and 1,800 years to change its position 1′. In fact, we should have to watch the star for a generation or two before we could be certain whether it was changing its position or not. And even after we had found with certainty that the star was shifting, and this at the rate of 1′ in 1,800 years, we could not, without a knowledge of its distance, express the angle of displacement in miles. But from the apparent magnitude or brilliancy of the star we could not determine whether its distance was ten times, 100 times, or 1,000 times that of *a* Centauri and consequently we could form no conjecture as to the actual velocity of the star. If we assumed its distance to be ten times that of *a* Centauri, this would give a transverse velocity of one mile per second. If we assumed its distance to be 100 times that of *a* Centauri, this would give ten miles a second as the velocity, and if 1,000 times, the velocity of course would be 100 miles per second.

As there are but few of the stars which show a measurable parallax and having no other reliable method of estimating their distances, it follows that in reference to the greater number of the stars neither by spectrum analysis nor by observation of their change of position can we determine their velocities. There does not therefore appear to be the shadow of a reason for believing that none of the stars has a motion of over thirty or forty miles per second. For anything that at present is known to the contrary, the majority of them may possess a proper motion enormously greater than that.

There is, however, an important point which seems to be overlooked in Mr. Plummer's objection, viz., that unless the greater part of the motion of translation be transformed into heat, the chances are that no sun-star will be formed. It is necessary to the formation of a sun which is to endure for millions of years, and to form the centre of a planetary system like our own that the masses coming into collision should be converted into an incandescent nebulous mass. But the greater the amount of motion left unconverted into heat, the less is the chance of this condition being attained. A concussion which would leave the greater part of the motion of translation untransformed would be likely as a general rule to produce merely a temporary star, which would blaze forth for a few years or a few hundred years, or perhaps a few thousand years, and then die out. In fact we have had several good examples of such since the time of Hipparchus. Now, although it may be true that according to the law of chances, collisions producing temporary stars may be far more numerous than those resulting in the formation of permanent stars, nevertheless the number of those temporary stars observable in the heavens may be perfectly insignificant in comparison to the number of permanent stars. Suppose there were as many as one hundred temporary stars formed for one permanent, and that on an average each should continue visible for 1,000 years, there would not at the present moment be over half-a-dozen of such stars visible in the heavens.

JAMES CROLL

The Age of the Earth

WITH reference to the ingenious suggestion by Mr. Preston, on the earth's orbit having been practically diminished by ethereal retardation, there are a few other points to be considered. 1. That the minor planets could never have passed the major planets, as they would be certainly caught by them during the immense number of revolutions in which their orbits would be nearly equal. Therefore the earth cannot have dropped in from much farther than Jupiter's present orbit; for if during its revolutions it came within one-sixth of the distance from Jupiter that it now is from the sun, it would be mastered by Jupiter. 2. By the retardation of Encke's comet it seems that if the comet had the same orbit as the earth, its distance from the sun would diminish about $\frac{1}{35000}$ per year. But for any appreciable lengthening of the earth's life-period, the earth must have started much more than one-tenth farther from the sun than it now is; that is to say, it must fall in much quicker than at the rate of its present distance from the sun in 10^8 years. This shows that the individual portions of Encke's comet must be much more than two miles in diameter, even supposing it to have as great a mean density as the earth, and to consist of a shower of solid meteors. Thus if the earth's history should be lengthened by any important amount from this cause, the nucleus of Encke's comet must consist of a shower of bodies of as great a density as the earth, and of a considerable size, each weighing very much more than 100,000,000 tons. And considering that there must be thousands of such bodies to compose it, the total mass would be greatly beyond what is considered possible. 3. If the earth had drawn much nearer to the sun, the asteroids must have come in from a very much greater distance; and yet, though they differ greatly in size, they are all grouped closely together, whereas we should find then sorted out very much more widely, and a vast quantity of them retained by Jupiter as satellites.

The solar system appears to be really a quinary system of stars; the major planets being analogous to the sun in their characteristics of density, distances and proportions of satellites, and other elements, the minor planets being the sun's satellites. Thus it is seen that the uniform law of satellites is to regularly decrease in volume both close to, and farthest from, their primaries; the series manifestly terminating in asteroids in the case of the sun and of Saturn.

In the whole of the present discussion of the earth's age, what is the reason why only one out of several different limits is considered? 1. The decrease of temperature in the earth. 2. Tidal retardation. 3. The cooling of the sun, which is recognised as being the weakest of the three. 4. A uniform diffusion of temperature in the earth, which gives a limit, not for life, but for the separate existence of the earth. The close agreement of the limits of life history given by these first three methods is a very strong argument in favour of each of them; for if there is even a possibility of 1 in 5 that each separately is wrong, it would be less chance than 1 in 100 that the concordance of all three was wrong.

Is there anything so stable and certain in geologic time—when we remember that levels permanently alter as quickly as ten feet per century—that rainfall (and therefore denudation) depends mainly on the almost unknown changes in the sun's heat, a slight increase of rainfall making much greater rapidity of denudation—and that accumulation of peat and stalactite might well become proverbial for its variability—when all these uncertainties are remembered, is there anything so indubitable as to warrant our throwing all the odium of incorrectness on the cosmical chronology, and seeking to square it with geological suppositions?

W. M. FLINDERS PETRIE

Bromley, Kent

The "Eurydice" Squall

THE loss of H.M.S. *Eurydice* on the 24th ult. may perhaps give a melancholy interest to a plain statement of the facts connected with the meteorology of that day.

The squall in which she capsized was one of a common class which occur when, after a long steady fall of the barometer, the mercury pauses for a few hours before commencing to rise. These squalls differ considerably from simple squalls, and are frequently complicated, as in this case, with small secondary cyclones.

Since the 20th inst. the general type of weather over our islands had been very uniform, an area of high pressure being constantly found over the west of Ireland, with a constantly low pressure near Stockholm giving cold north-west winds, conditions which are very common in the month of March. But while the general shape of the isobaric lines remained constant, the absolute pressure over the whole area had been diminishing rapidly till the 24th inst. On the morning of that day, the centre of a cyclone was near Stockholm, while no less than three secondary depressions were influencing Great Britain, and by 6 P.M. the whole system had gathered itself into two small cyclones whose centres were near Yarmouth and Bergen.

Such a development of secondaries with a north-west wind is not common, and is always associated with exceptionally wild and broken weather, of the kind which gives heavy local rainfall, with squalls, or violent cold thunderstorms, but not widespread or destructive gales.

In London the changes above described were well shown by a steady fall of the barometer from the 21st inst., which amounted to an inch at 3.45 P.M. on the 24th. As a heavy squall came on then, the barometer jumped up suddenly two-hundredths of an inch, as is often the case in squalls, and then fell slowly in about a quarter of an hour to its former level, where it remained stationary till about 9 P.M., after which it rose steadily. The squall, which lasted about twenty minutes, was followed by very threatening-looking weather, during which the wind perhaps backed a little to west-north-west, but at 4.40 P.M. it shifted to north-north-east and became strong, with heavy snow, till 5.20, when the weather moderated, the whole being evidently due to the complicated action of one of the secondary depressions before mentioned.

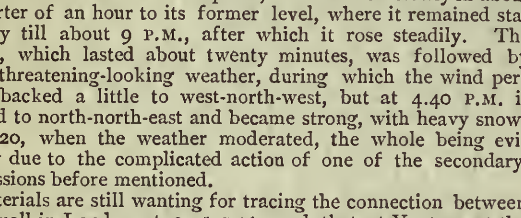
Materials are still wanting for tracing the connection between the squall in London at 3.45 P.M., and that at Ventnor at the same hour, but squalls often do occur simultaneously at distant places in connection with the trough of great non-cyclonic barometric depressions. The question of any such relation has not yet been worked out, and its solution presents great difficulties.

On the whole, then, the squall in which the *Eurydice* was lost, though of a common type, was somewhat exceptional in suddenness and violence. RALPH ABERCROMBY

21, Chapel Street, S.W., April 3

Leidenfrost's Phenomenon

A FEW days ago I was examining the "rosette" formed by a spheroid of water in a hot platinum capsule, and noticed that the outline was not a continuous curve, as is generally represented in books, but was "beaded" with re-entering angles as shown by the continuous lines in figures A_3, B_3, C_3, while the curve of



each bead could be distinctly traced within the drop, forming a "fluted" outline, shown by the dotted lines in the same figures. It was at once manifest that both the "beaded" and "fluted" figures were produced by the superposition of the retinal images of the drop in two extreme conditions of vibration; that, in the case represented by A_3, the drop was really vibrating like a bell which is sounding its first harmonic above its fundamental note, and therefore possesses six ventral segments, the extreme forms assumed being represented by A_1 and A_2 respectively, and that B_3 and C_3 represent the appearance of the drop when vibrating like a bell which is sounding its second and third harmonic respectively. To verify this a spheroid of about five-eighths of an inch in diameter was produced; and as soon as the beaded decagon, C_3, was steadily maintained, the room was darkened, and the spheroid illuminated by sparks from Holtz's machine. Immediately the curvilinear pentagons C_1 and C_2 were apparent, and frequently the vibrations continued perfectly steady for several seconds. When the drop had diminished in size the mode of vibration changed, and the crosses represented by B_1 and B_2 appeared when the sparks passed; on opening the shutters the beaded octagon B_3 appeared almost perfectly steady in the capsule. The figures A_1, A_2, and A_3 were obtained in the same manner, and with a larger spheroid twelve and sixteen beads were obtained, presenting respectively curvilinear hexagons and octagons when illuminated by the sparks. In one case a small spheroid presented a very large number of beads in its outline; but on examining it with sparks it was found to be produced by the crosses B_1 and B_2 rotating very rapidly about a vertical axis. Two or three particles of carbon introduced into a spheroid remained for a long time close to the surface of one "ventral segment," like lycopodium powder on a Chladni's plate, and when they escaped from it were ensnared by the next segment. The figures observed when the spheroids were illuminated by sparks were fully as exorbitant as those shown at A_1, A_2, B_1, B_2, C_1, and C_2.

If the spheroidal form be due to the combined action of gravity and surface tension, it is obviously to the latter force that we must look for the production of vibrations when, by any accident, the spheroid is disturbed. The amount of steam produced from the under-side of any "ventral segment" will, of course, be greater the greater the surface exposed; and when this is a fresh surface, will increase as the surface becomes heated by exposure. Hence the amount of steam escaping from beneath a "ventral segment" will be greater as it is contracting towards, than when it is moving from, the centre of the spheroid, thus supplying, on the whole, during each vibration an impulse in the direction of motion. It seems unnecessary to look farther for a supply of energy. WM. GARNETT

Cavendish Laboratory, Cambridge, March 15

Trajectories of Shot

HAVING observed a letter in NATURE, vol. xvii. p. 401, in which extracts from a paper of mine are commented upon by the Rev. F. Bashforth, I trust you will let me make a few remarks by way of explanation.

In the paper referred to I was trying to weigh against one another the merits of different methods of finding the trajectories of shot, the calculations being, of course, based upon Mr. Bashforth's tables; and the method which I liked the best did not contain the equation (a), which is the text of Mr. Bashforth's letter. Now without doubt the method I preferred had faults of its own, but it was a sort of argument in its favour if I could show that the other methods were not faultless, and in particular if I could show that the equation (a), which is the key of those other methods, had no merits of severe accuracy to set off against certain defects which I thought it might fairly be charged with.

The objections I had to the equation (a) are partly set forth in the first extract quoted by Mr. Bashforth; but one great objection to it is the tediousness of its application in practice. Mr. Bashforth appears to be greatly offended with my description of the way the equation is used, viz., that it is a process of guessing. But he cannot pretend that he has solved the equation according to any strict method; he has only guessed at a solution which falls in more or less with his tables. It seems to me he is here quarrelling about a mere name, because the process he describes and indeed illustrates is practically the process I describe, and it is idle on his part to give me the information contained in his letter, because I am very well aware that the second guess gives a better result than the first. But as regards the amount of accuracy belonging to the equation, I must still hold by the substance and tendency of my remarks on that subject, except in my unfortunate use of the epithet "dangerous," which I admit was extreme. I frankly confess that the force of the argument derived from discussing the values of $\frac{dk}{dv}$ is materially weakened when these values are numerically exhibited and compared with the tables. At the same time, when taken in connection with the peculiar way the equation is used, the numbers,

such as I make them roughly, do not convince me that the argument is without force. My chief criticism on the equation has two branches :—1. Mr. Bashforth has nowhere proved that he is entitled to use the k belonging to the mean velocity over the arc. 2. Granting that he may use that k, we have then to consider whether he has got v_β and k to accord. For my part, I do not feel the degree of certainty which Mr. Bashforth expresses about this, especially if the work is carried over a considerable arc. I will grant that his result comes near the truth, but assuredly he cannot be said to have determined v_β *accurately*, as he affirms.

I cannot help thinking that there is no real difference between Mr. Bashforth and myself, for all that I have said against the equation (a) can be said in another form against the method that I prefer, and I willingly indorse the statement in the last paragraph but one of his letter. I may be allowed to add that all methods hitherto proposed of calculating shot ranges seem to me too difficult for common use, and I believe what would really be a boon to the artilleryman is a book of trajectories drawn to scale. This might be accomplished very well by Mr. Bashforth's tables and methods in the hands of some one competent to use them, the simpler methods, as I think them, introduced by me, being also of some service. I trust this will be done when the resistance to shot moving with low velocities has been ascertained, as I hear it is to be, by a series of experiments under Mr. Bashforth's superintendence.

Allow me in conclusion to express my regret that I should seem to have been reviewing in a hostile spirit any part of the work done by Mr. Bashforth at Woolwich. I will only assure him that nothing could have been further from my thoughts than to do so.

W. D. NIVEN

Trinity College, Cambridge, March 30

The Daylight Meteor of March 25

A CORRESPONDENT in NATURE described the falling of a daylight meteor on Monday, March 25. I have received information respecting this meteor from five persons who witnessed its fall.

Mr. McIntyre, who saw it from near Dunston-on-Tyne; Mr. Wood, banker, who saw it whilst leaving his residence at Benton, near Newcastle-on-Tyne; Mrs. Hopper, from Gosforth, one mile north of Newcastle; Mrs. Lupton, who saw it from a railway carriage at Brampton, near Carlisle; and Mr. W. Clarke, of Newburn, who saw it at Wallbottle, four miles west of Newcastle. All these observers agree in the following particulars :—1. That the meteor was visible at 10.20. 2. That it was very luminous with a white light slightly coloured. 3. That it fell at a slight inclination from E. to N., and reached the horizon at or near the north point. 4. That the weather was clear and the sun shone brightly at the time the meteor was visible.

T. P. BARKAS

26, Archbold Terrace, Newcastle-on-Tyne

Meteor

ON the night of Tuesday, April 2, at about 7.55 o'clock, I was standing with two companions, facing the north, when we were surprised to observe the ground before us suddenly lighted up, and our three shadows sharply defined upon it. One of my friends exclaimed, "Why, there's the moon come out!" We turned round and beheld a wonderfully brilliant meteor descending almost perpendicularly from about 5° east of Betelgeux, in Orion, towards the most eastern of the three stars in the belt. Its course was slightly zig-zag, its colour yellow or orange, its apparent size about half the diameter of the full moon. It vanished noiselessly before reaching the belt, and left no visible remains. When we first saw it there appeared be a short trail of light behind it. About three minutes after its disappearance a rumbling sound was heard like distant thunder, from the same direction. Whether this was connected with the meteor I cannot tell. If so it would indicate a distance of about forty miles, and we ought to hear of this meteor from the neighbourhood of Warwick.

F. T. MOTT

Birstal Hill, Leicester

[The same meteor was seen by several *Times* correspondents. It made its appearance in Ursa Major, and after remaining stationary for a second or two between Orion's Belt and Sirius, fell at a comparatively slow rate and in a direct line to the horizon. It was pear-like in shape, seemed three or four times larger than Jupiter, and was intensely bright. Its colour changed from a silvery white to a pale red as it approached the horizon, where it disappeared behind a cloud, leaving a long track of light behind it.]

To Entomologists

As I have undertaken the section "Arthropoda" for the "*Jahresbericht für Anatomie und Physiologie*, of Hoffmann and Schwalbe," and find some difficulty in obtaining English scientific journals (specially the entomological ones) here in Naples, will you permit me through your columns to request such of your readers as may have published papers on the *anatomy*, *ontogeny*, and *phylogeny*, of the *Hexapoda*, *Myriapoda*, *Arachnoidea*, *Protracheata*, *Poecilopoda*, and *Crustacea* in 1877, or intend to do so in 1878 and the following years, to be kind enough to forward me a copy of them, or at least to inform me of the fact?

PAUL MAYER

Naples, Stazione Zoologica, March 31

GEOGRAPHICAL NOTES

ROYAL GEOGRAPHICAL SOCIETY MEDALS.—The Founder's Medal for 1878, of the Royal Geographical Society, has been awarded to Baron F. von Richthofen for his extensive travels and scientific explorations in China; also for his great work now in course of publication, in which the materials accumulated during his long journeys are elaborated with remarkable lucidity and completeness. The Patron's Medal has been given to Capt. Henry Trotter, R.E., for his services to geography, in having conducted the survey operations of the late Mission to Eastern Turkistan, under Sir Douglas Forsyth, which resulted in the connection of the Trigonometrical Survey of India with the Russian Surveys from Siberia, and for having further greatly improved the map of Central Asia. Mr. Stanley, being already a medallist, is disqualified from receiving a similar honour, but he has been elected an honorary corresponding member, and is to receive the thanks of the Council for his discoveries.

AFRICA.—With a view to facilitating the progress of the London Missionary Society's contemplated expedition from the East Coast of Africa to Lake Tanganyika, the Rev. Roger Price, who had had long experience of roads and waggons in South Africa, was despatched to Zanzibar in 1876, to make investigations respecting a new route and new mode of travelling into the interior. He made the experiment of using bullocks and waggons in the place of *pagazi*, and with so much success that it was resolved that the expedition should adopt that mode of conveyance for themselves and their goods, and a flourishing account of the new scheme was given before the Royal Geographical Society on February 26, 1877. Before the expedition arrived at Zanzibar in the summer of last year, Mr. Mackay, an agent of the Church Missionary Society, was reported to have cleared a road nearly, if not quite, as far as Mpwapwa, and it was supposed that the expedition would reach the Lake with great ease. Their hopes, however, have been grievously disappointed. The road has turned out to be no road at all, and most of the oxen have died from the effects of the climate. Mr. Price returned to England some little time back, convinced, we believe, of the present impracticability of his bullock-waggon scheme, and sad to relate, it has been found necessary to revert to the old *pagazi* system, the curse of African travel. By latest accounts the expedition had formed a camp at Kirasa, in Usugara, on the edge of the high plateau, and about forty miles east of Mpwapwa, and there they intend to remain till after the rainy season.—Lieut. J. B. Wathier has been appointed to join the Belgian expedition at Zanzibar, which recently lost two of its members, MM. Crespel and Maes. He has visited Dr. Nachtigall at Berlin, to obtain the advice of the experienced explorer, and left Brindisi for Zanzibar on the 5th inst. Dr. Nachtigall himself, as leader of the German expedition, is to start from St. Paul de Loanda, and it is hoped that the two expeditions may meet in the centre of Africa.

On the 5th inst. Lieut. de Semellé left Bordeaux for the purpose of setting out on his proposed journey across Africa from Senegambia. The Society of Algerine Catholic Missions has obtained from the Pope an authorisation to send two parties of priests into the interior of Africa; one, under the direction of Father Pascal, will establish a *vicariat apostolique* on the banks of Lake Tanganyika; the second party, whose head is said to be Father Livinzac, will establish a similar organisation in the region of the Nyanzas. The missions will be scientific as well as religious.

ARCTIC EXPLORATION.—A wealthy Russian merchant M. Sibiriakoff has offered the sum of 12,000 roubles to the Committee of the Dutch Arctic Expedition, on condition that the Committee should order that the Siberian coasts be specially explored by the Expedition. The Committee has, however, refused the offer, on the one hand because it was considered undignified to accept foreign help for a purely national undertaking, and on the other, because the expedition has a specifically scientific and not a commercial object. This offer of M. Sibiriakoff seems unnecessary, seeing that the Siberian coast is likely to be explored this summer by Prof. Nordenskjöld in the *Vega*. We may remind our readers that this expedition sets out in the beginning of July, for the purpose of forcing the North-East Passage from Europe to Behring's Straits. Prof. Nordenskjöld has made a thorough study of the records of Russian exploration along the north coast of Siberia, and concludes that in early autumn the ice retires from the coast as a rule, leaving a comparatively clear waterway. Even should the immediate aim of the expedition not be accomplished, we may expect large additions to our knowledge of the hydrography, geology, and natural history of these regions, which, from a scientific point of view, have been comparatively unexplored. Prof. Nordenskjöld conjectures that a line of islands separates the Siberian from the strictly Polar Sea, of which we only know Wrangell Land and New Siberia; he will endeavour to verify this conjecture. The proposal has been made in the first Chamber of the Swedish Reichstag to grant the sum of 22,000 Swedish crowns for the Arctic Expedition projected by Lieut. Sandeberg, the costs of whose exploration in Lapland, to which we have already referred, were defrayed entirely by himself.

CAIRO GEOGRAPHICAL SOCIETY.—At an extraordinary meeting of this society on February 16 the question of its existence was discussed; it had become almost extinct from want of funds. It was proposed to join it to the Egyptian Institute, thereby much diminishing its working expenses, and putting it in an advantageous position for carrying on its work. We hope the scheme will be carried out, as the situation of the society places it in an unusually favourable position for carrying on the work of African exploration.

SOUTH AMERICA.—Advices from Valparaiso state that Commander Paget, of Her Majesty's ship *Penguin*, communicated to the captain of the port of Coquimbo that while passing through Messier's Channel, on January 10, he saw a volcano in eruption, situated E. ¼ S. (Mag.) of the southern extremity of Middle Island, English Narrows. It is supposed that this volcano is the cause of the subterranean noises heard by an exploring party from the corvette *Magallanes*, near Lake Santa Cruz, in the middle of December, and is the same as that believed to exist by the Argentine explorer Moreno. *Apropos* of the foregoing, says the *Timpos*, one of the most remarkable discoveries made by Chilian explorers is the complete disappearance of the Andes chain at the southern extremity of the continent. Messrs. Rodgers and Ibar crossed from Brunswick Peninsula, situated, according to the Argentines, to the east of the Andes, to the Pacific, arriving at a place called by Fitzroy the plains of Diana, without meeting with vestiges of the Cordillera. There are plains, more or less inclined, but only plains.

ETHNOGRAPHY OF RUSSIA.—As Supplement 54 to Petermann's *Mittheilungen*, Col. Rittich's valuable and elaborate treatise on the ethnography of Russia has just been published, with a large map, coloured with the greatest care, and showing with wonderful clearness the many elements which go to make up the Russian population.

THE YENISSEI.—To the April number of the *Geographical Magazine* Mr. Seebohm contributes a paper on the Valley of the Yenissei, embodying some of the results of his recent voyage to that river.

THE WHANG-HO.—In the same number is the first instalment of an exhaustive paper on this river, with special reference to its double delta, by Mr. Samuel Mossman.

EDUCATIONAL VOYAGE.—A voyage around the world, designed for students, is being arranged at Paris. It will last eleven months, over six months being devoted to various land excursions. Books, collections, &c., will be taken, and the entire programme will have instruction, rather than sight-seeing, in view. It is intended to depart on June 15.

PARIS GEOGRAPHICAL SOCIETY.—The January *Bulletin* of this Society contains the first part of an important inquiry into the medical geography of the West Coast of Africa, by Dr. H. Rey, and also the first instalment of a narrative of a journey in Cilicia in 1874 by MM. C. Favre and B. Mandrot. M. Nogueira gives a translation of a paper from the Portuguese on the South African river Cunene.

The Council of the Society of Geography has issued its list of candidates for the high offices of the society. It proposes to the members to elect Admiral La Roncière le Nourry, who has been voted six or seven times almost without opposition. But a number of independent members are proposing, in opposition, the nomination of the present Minister of Marine.

DEPTHS OF LAKES.—*The Bavarian Courier* publishes an interesting comparative statement of the depths of lakes. Amongst European lakes the Achensee, in the Tyrol, heads the list. At some points the depth of this lake amounts to 772 metres. The greatest depth of the lake of Constance is about 300 metres, that of the Chiemsee about 141 metres, and that of the Walchen- and Königssee, 188 metres. The measurements made about 1870 at the Dead Sea showed that at its deepest part the depth is 565 metres, but if we consider that the level of this lake is already 429 metres below the level of the Mediterranean, then we find that the total depression in the soil here amounts really to 994 metres. The Lake of Tiberias is extremely shallow in comparison; on its eastern part the average depth is only eight metres, while on the western side it lies between six and seven metres. In Lake Baikal depths have been found which for a lake are truly astonishing. In the upper part of the lake the depth is 3,027 metres (about the height of Mount Etna), but downwards the bottom constantly descends, and near the opposite bank the depth amounts to 3,766 metres. This depth far exceeds that of the Mediterranean Sea, which at its deepest part measures only 2,197 metres.

GERMAN ALPINE CLUB.—The German and Austrian *Alpen Verein*, although comparatively young, has developed a most praiseworthy activity in a variety of directions. From the last general report we notice that it has a membership of nearly 7,000, and an annual income of 40,000 marks. The chief exertions are devoted to the erection of shelter huts in the Alps, and maintenance of communications over the passes. It is, however, rendering no slight service to the cause of geography, by the

gradual preparation of elaborate maps of the German and Austrian Alps. At present it is engaged on a map of the Salzkammergut, on a scale of 1 : 100,000.

A LUNAR LANDSCAPE

MESSRS. GAMMON AND VAUGHAN, No. 28, Old Bond Street, have at present on view a picture in which the artist, Olafs Winkler, of Weimar, has endeavoured to represent a lunar landscape. Prof. C. Bruhns, of Leipzig, has assisted him in the parts of the treatment which are directly scientific.

The painter has not trusted all to his imagination. He has, to the best of his knowledge and ability, sought to stick rigidly to truth, and to paint a lunar landscape such as it would appear, so far as human observation has hitherto ascertained, to a human eye, were it at all possible for a man to be transplanted to the moon and observe through his earthly eyes, only for a moment, nature as she manifests herself on the surface of our satellite. From the merely artistic point of view the artist fears his task may be a thankless one, for since the moon has no atmosphere, there is neither aërial perspective nor diffusion of light, but it is precisely this point which should make our artist all the more interested in this unique production. The shadow of a body in the foreground will appear quite as black as the sky itself which closes the landscape like a flat steep wall, broken only by the quiet light of the stars. All lights appear equally strong at a distance and close at hand, and this also holds with the local colouring. In a word, there is wanting in the lunar landscape that which lends to our earth perspective, richness or tone, modulation, softness, and temper. It is our atmosphere we have to thank for most of the multitudinous coloured phenomena of the terrestrial landscape—phenomena which in our satellite are impossible. The sunlight falls upon the hills with blinding brightness, and cuts sharply across the deep black shadows. Its intensity rivals the electric light, and light effects of such a kind are far beyond the reach of our palettes. We must resort to some expedient to be able to introduce a medium between the extreme contrast of light and shade, a sort of half-tone, which, at the same time, must be the chief tone of the picture; this Herr Winkler has sought in the light of the earth, the true "earthshine."

The artist has chosen the time of sunset, and the region he has selected lies in the northern part of the moon. The spectator is supposed to be on the front slope of a mountain, the continuation of which in the background comes out as a closed ridge. At his feet one of the numerous *maria* spreads out, filled up with rills, circular hills, and large and small craters, stretching away to the distant mountain referred to. Before, us in the black sky, hangs the moon's moon, our earth. She sheds her pale, ash-coloured light over the rent, desolate, dead stone-fields. Only the highest points of the mountain-tops still glow in the light of the setting sun, no longer red, as here, but dazzlingly white, in consequence of the absence of atmospheric absorption. The earth is at the period of her course between Sagittarius and the Scorpion, Antares being nearly in the middle of the picture. Against his persuasion he has been compelled to make the milky way very weak, and the stars somewhat large in proportion to the earth.

Herr Winkler, in a paper read at the last meeting of the German Association, stated that his first impulse to undertake the picture was derived from Nasmyth and Carpenter's work on the moon.

Our only criticism of the picture refers to the colour of the earth and of the true earthshine. We doubt whether the earth is quite red enough, especially at the edges, and we doubt again whether, with the earth as ruddy as it is, the colour of the lunar landscape itself should not be rather more in harmony with it, as it is the true light source.

The picture is an admirable performance, and the science of it is so true that, as we hinted before, those of our artists who care to have a natural basis for their depiction of natural phenomena will learn much from this attempt to deal with a new order of phenomena.

EDISON'S TALKING-MACHINE[1]

MR. THOMAS A. EDISON has recently invented an instrument which is undoubtedly the acoustic marvel of the century. It is called the "Speaking Phonograph," or, adopting the Indian idiom, one may aptly call it "*The Sound-Writer who Talks.*" Much curiosity has been expressed as to the workings of this instrument, so I purpose giving an account of it.

All talking-machines may be reduced to two types. That of Prof. Faber, of Vienna, is the most perfect example of one type; that of Mr. Edison is the only example of the other.

Faber worked at the source of articulate sounds, and built up an artificial organ of speech, whose parts, as nearly as possible, perform the same functions as corresponding organs in our vocal apparatus. A vibrating ivory reed, of variable pitch, forms its vocal chords. There is an oval cavity, whose size and shape can be rapidly changed by depressing the keys on a key-board. A rubber tongue and lips make the consonants; a little windmill, turning in its throat, rolls the letter *r*, and a tube is attached to its nose when it speaks French. This is the anatomy of this really wonderful piece of mechanism.

Faber attacked the problem on its physiological side. Quite differently works Mr. Edison: he attacks the problem, not at the source of origin of the vibrations which make articulate speech, but, considering these vibrations as already made, it matters not how, he makes these vibrations impress themselves on a sheet of metallic foil, and then reproduces from these impressions the sonorous vibrations which made them.

Faber solved the problem by reproducing the mechanical *causes* of the vibrations making voice and speech; Edison solved it by obtaining the mechanical *effects* of these vibrations. Faber reproduced the movements of our vocal organs; Edison reproduced the motions which the drum-skin of the ear has when this organ is acted on by the vibrations *caused* by the movements of the vocal organs.

Figs. 1 and 2 will render intelligible the construction of Mr. Edison's machine. A cylinder, F, turns on an axle which passes through the two standards, A and B. On one end of this axle is the crank, D; on the other the fly-wheel, E. The portion of this axle to the right of the cylinder has a screw-thread cut on it, which, working in a nut, A, causes the cylinder to move laterally when the crank is turned. On the surface of the cylinder is scored the same thread as on its axle. At F (shown in one-half scale in Fig. 2) is a plate of iron, A, about $\frac{1}{100}$ of an inch thick. This plate can be moved toward and from the cylinder by pushing in or pulling out the lever H G, which turns in an horizontal plane around the pin I.

The under side of this thin iron plate, A (Fig. 2), presses against short pieces of rubber tubing, X and X, which lie between the plate and a spring attached to E. The end of this spring carries a rounded steel point, P, which enters slightly between the threads scored on the cylinder C. The distance of this point, P, from the cylinder is regulated by a set-screw, S, against which abuts the lever, H G. Over the iron plate, A, is a disc of vulcanite, B B,

[1] The figures in this article are taken from "Sound, a Series of Simple, Entertaining, and Inexpensive Experiments in the Phenomena of Sound, for the Use of Students of every Age." By Alfred M. Mayer. Vol. ii. of "Experimental Science Series for Beginners." (Now in press and soon to be published by D. Appleton and Co.)

with a hole in its centre. The under side of this disc nearly touches the plate A. Its upper surface is cut into a shallow, funnel-shaped cavity, leading to the opening in its centre.

To operate this machine, we first neatly coat the cylinder with a sheet of foil, made to adhere by coating the edges with shellac varnish, then we bring the point, P, to bear against this foil, so that, on turning the cylinder, it makes a depressed line, or furrow. The mouth is now

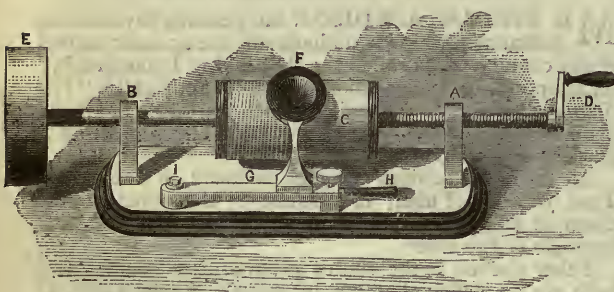


FIG. 1.—Edison's Talking-Phonograph.

placed close to the opening in the vulcanite disc, B B, and *the metal plate is talked to* while the cylinder is revolved with a uniform motion.

The plate, A, vibrates to the voice, and the point, P, indents the foil, impressing in it the varying numbers, amplitudes, and durations of these vibrations. If the vibrations given by the voice are those causing simple sounds, and are of a uniform, regular character, then similar, regular, undulating depressions are made in the

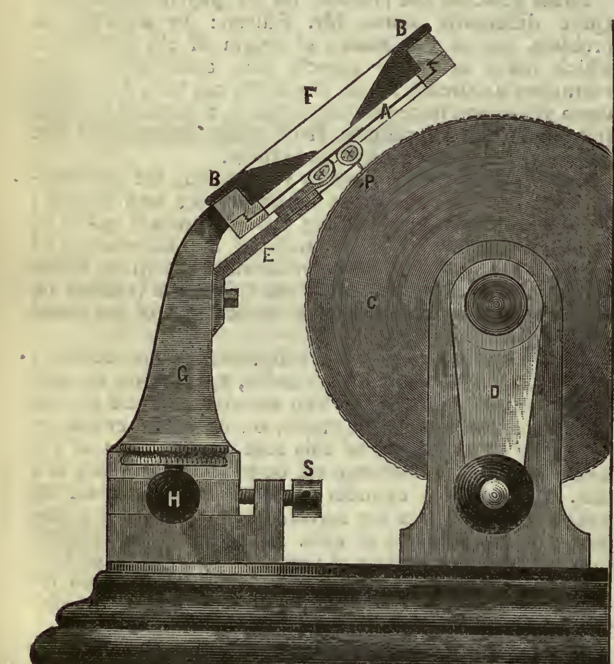


FIG. 2.

foil. If the vibrations are those causing complex and irregular sounds (like those of the voice in speaking), then, similarly, the depressions made in the foil are complex, having profiles like the curve, B, in Fig. 3. Thus the yielding and inelastic foil receives and *retains* the mechanical impressions of these vibrations with all their minute and subtile characteristics.

The permanent impressions of the vibrations of the voice are now made. It remains *to obtain from these impressions the aerial vibrations which made them.* Nothing is simpler. The plate A, with its point, P, is moved away from the cylinder by pulling toward you the lever, H G. Then the motion of the cylinder is reversed till you have brought opposite to the point P the beginning of the series of impressions which it has made on the foil. Now bring the point up to the cylinder; place against the vulcanite plate, B B, a large cone of paper or tin to re-enforce the sounds, and then steadily turn the crank, D. The elevations and depressions which have been made by the point, P, now pass under this point, and in so doing they cause it and the thin iron plate to make over again the precise vibrations which animated them when they made these impressions under the action of the voice. The consequence of this is, that the iron plate gives out the vibrations which previously fell upon it, and it *talks back to you what you said to it.*

By the following method we have just obtained several magnified traces on smoked glass of the contour, or profile, of the elevations and depressions made in the foil by the sonorous vibrations. On the under side of the shorter arm of a delicate lever is a point, made as nearly as possible like the point, P, under the thin iron plate, A. Cemented to the end of the longer arm of this lever is a pointed slip of thin copper-foil, which just touched the vertical surface of a smoked-glass plate. The point on the short arm of the lever rested in the furrow in which are the depressions and elevations made in the foil on the cylinder. Rotating the cylinder with a slow and uniform motion, while the plate of glass was slid along, the point of copper-foil scraped the lamp-black off the smoked-glass plate and traced on it the magnified profile of the depressions and elevations in the foil on the cylinder. I say expressly *elevations* as well as depressions in the foil, because, when the plate vibrates outward, the furrow in the foil often entirely disappears, and is always lessened in its depth by this outward motion of the point. One who has never made a special investigation of the character of the impressions on the phonograph, and forms his opinion from their appearance to his eye, might state that they are simply dots and dashes, like the marks on the filet of a Morse instrument

Another method of obtaining the profile of the impressions on the foil is to back it with an easily-fusible substance, and then, cutting through the middle of the furrows, we obtain a section, in which the edge of the foil presents to us the form of the elevations and depressions.

The instrument has been so short a time in my possession, that I have not had the leisure to make on it the careful and extended series of experiments which it deserves. I have, however, obtained several traces, and I have especially studied the characters of the trace of the sound of *bat.* As far as the few experiments warrant an expression of opinion, it seems that the profile of the impressions made on the phonograph and the contours of the flames of König, when vibrated by the same compound sound, bear a close resemblance.

In Fig. 3 we give on line A the appearance to the eye of the impressions on the foil, when the sound of *a* in *bat* is sung against the iron plate of the phonograph. B is the magnified profile of these impressions on the smoked glass obtained as described above. C gives the appearance of König's flame when the same sound is sung quite close to its membrane. I say expressly *quite close* to its membrane, for the form of the trace obtained from a point attached to a membrane vibrating under the influence of a compound sound depends on the *distance* of the source of the sound from the membrane, and the same compound sound will form an infinite number of different traces as we gradually increase the distance of its place of origin from the membrane; for, as you increase this distance, the waves of the components

of the compound sound are made to strike on the membrane at different periods of their swings.

For example, if the compound sound is formed of six harmonics, the removal of the source of the sonorous vibrations, from the membrane to a distance equal to $\frac{1}{4}$ of a wave-length of the first harmonic, will remove the second, third, fourth, fifth, and sixth harmonics to distances from the membrane equal respectively to $\frac{1}{2}$, $\frac{3}{4}$, 1, $1\frac{1}{4}$, and $1\frac{1}{2}$ wave-lengths. The consequence evidently is, that the resultant wave-form is entirely changed by this motion of the source of the sound, though the sonorous sensation of the compound sound remains unchanged.

The above facts are readily proved experimentally by sending a constant compound sound into the cone of König's apparatus, while we gradually lengthen the tube between the cone and the membrane next to the flame. This is best done by the intervention of one tube sliding in another, like a trombone. These experiments I have recently made with entire success, and they explain the discussions which have arisen between different observers as to the composition of vocal and other composite sound, as analysed by means of König's vibrating flames.

These facts also show how futile it is for any one to hope to be able to *read* the impressions and traces of phonographs, for these traces will vary, not alone with the quality of the voices, but also with the differently-related times of starting of the harmonics of these voices, and with the different relative intensities of these harmonics.

It is necessary to give to the cylinder a very regular motion of rotation while it receives and reproduces the vibrations made in singing; for even slight irregularities in the velocity of the cylinder destroy the accuracy of the musical intervals; and cause the phonograph to sing falsetto. Even the reproducing of speech is greatly

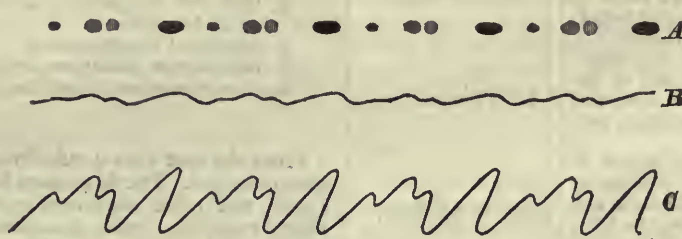


FIG. 3.

improved by rotating the cylinder by mechanism which gives it uniformity of motion. If you make the machine talk by giving it a more rapid rotation than it had when you spoke to it, the pitch of its voice is raised; and by varying the velocity of the cylinder the machine may be made to speak the same sentence in a very bass voice, or in a voice of a pitch so high that its sounds are really elfish and entirely unnatural.

Recent experiments seem to show that the nearer the diaphragm A approaches to the construction of the drumskin of the human ear by "damping" it, as the hammer-bone does the latter, the better does it record and repeat the sonorous vibrations; for the motion of a membrane thus damped is ruled alone by the aërial vibrations falling on it.

Mr. Edison has just sent me the following notes of the results of recent experiments:—

"That the size of the hole through which you speak has a great deal to do with the articulation. When words are spoken against the whole diaphragm, the hissing sounds, as in *shall, fleece, last*, are lost; whereas, by the use of a small hole provided with sharp edges, these words are reinforced and recorded. Also, teeth around the edge of a slot, instead of a round hole, give the hissing consonants clearer.

"That the best reading is obtained when the mouth-piece, B F B (Fig. 2), is covered with several thicknesses of cloth, so that the snapping noise on the foil is rendered less audible.

"I send you a sheet of copper-foil upon which I made records in Ansonia, Connecticut, that could be read 275 feet in the open air, and perhaps farther, if it had been tried."

Mr. Edison also states that impressions of sonorous vibrations have been made on a cylinder of soft Norway iron, and from these impressions have been reproduced the sonorous vibrations which made them.

ALFRED M. MAYER

THE OLD RED SANDSTONE OF WESTERN EUROPE[1]

PART I.

IN a historical introduction the author gives an outline of the progress of research into the history of the Old Red Sandstone of the British Area. This system is at present regarded as composed of three sub-divisions, Lower, Middle, and Upper, each characterised by a distinct suite of organic remains. From the absence of unequivocally marine fossils and from lithological characters, it has been inferred by Mr. Godwin Austen, Prof. Ramsay, Prof. Rupert Jones, as well as other observers, and is now very generally admitted that the Old Red Sandstone, as distinguished from the "Devonian" rocks, probably originated in inland sheets of water. The object of the present memoir was to endeavour to trace out in that geological system of deposits the changes of physical geography which took place over Western Europe during the interval between the close of the Upper Silurian and the beginning of the Carboniferous period.

After a sketch of the probable conditions of the region previous to the commencement of the Old Red Sandstone, the author proceeds to show how the shallowing Silurian sea was converted here and there into *salinas* or inland seas, by a series of subterranean movements which have left their indelible traces upon the upturned Silurian rocks. He divides his memoir into two parts, the first dealing with the Lower and the second with the Upper Old Red Sandstone. The present paper deals only with a portion of the first of these sections. It traces out the limits of the different basins in which the Old Red Sandstone of the British Islands were deposited, and for the sake of convenience as well as briefness of reference, proposes short geographical names for these basins, which are arranged as follows:—

[1] Abstract of paper by Prof. Geikie, F.R.S., read before the Royal Society of Edinburgh on April 1, 1878.

Area of the Basins.	Short reference names proposed to be applied to them.
1. The Old Red Sandstone tracts of the north of Scotland, embracing the region of the Moray Firth, Caithness, the Orkney Islands, the mainland of Shetland, and perhaps part of the south-western coast of Norway.	Lake Orcadie.
2. The central valley of Scotland between the Highlands on the north and the Silurian uplands on the south, including the basin of the Firth of Clyde, and ranging across the north of Ireland to the high grounds of Donegal.	Lake Caledonia.
3. A portion of the south-east of Scotland and north of England extending from near St. Abb's Head to the head of Liddesdale, and including the area of the Cheviot Hills.	Lake Cheviot.
4. A district in the north of Argyllshire extending from the mouth of the Sound of Mull to Loch Awe, and perhaps up into the southern part of the Great Glen.	Lake Lorne.
5. The Old Red Sandstone region of Wales and the border counties of England, bounded on the north and west by the older palæozoic hills, the eastern and southern limits being unknown.	The Welsh Lake.

Lake Orcadie.—After describing the limits of this basin, and giving a sketch of the labours of previous observers in the Old Red Sandstone tracts of the north of Scotland, the author proceeds to examine the evidence for the threefold arrangement of the Old Red Sandstone proposed by Murchison. He shows that nowhere are the three groups, Lower, Middle, and Upper, found in consecutive order; that this so-called "Middle" division occurs only in the north of Scotland, where it lies unconformably upon the older palæozoic rocks, and is itself unconformably overlaid by the Upper Old Red Sandstone, thus occupying a position exactly similar to that of the Lower Old Red Sandstone on the southern side of the Highlands. He further points out that while some species of fishes are common to the Old Red Sandstone on the two sides of the Highland barrier, the lithological differences between the deposits of the two areas are so great as to make it evident that the rocks were laid down in distinct basins and consequently that the fauna of each basin might be expected to be more or less peculiar, as in many analogous cases at the present day. As evidence that adjacent areas in the time of the Lower Old Red Sandstone were strongly marked off from each other in their faunas, reference is made to the contrast between the fishes and crustaceans of the Welsh region and those of Lanarkshire and Forfarshire, not a single species being common to the two countries though some of the genera are. Reasons are then given why the argument used by Murchison from the occurrence of many of the Scottish ichthyolites in Russia could not be regarded as establishing the existence of a "Middle" division of the Old Red Sandstone.

The conclusion arrived at by the author is that the Caithness flags or "Middle Old Red Sandstone" are probably the general equivalents of the Lower Old Red Sandstone of other regions, and that this system consists in Britain of two well-marked divisions only—a Lower, which graduates in some places into the Upper Silurian rocks and is separated by an unconformability from an Upper which in many districts passes up into the base of the Carboniferous system.

The various districts into which the area embraced under the term Lake Orcadie may be divided are then described *seriatim*. The detailed structure of Caithness has been worked out by the author (partly with the cooperation of his colleagues in the Geological Survey, Mr. B. N. Peach and Mr. John Horne) as affording the most complete sections of the Old Red Sandstone in the North of Scotland. Arranged in descending order, the various stratigraphical zones stand as in the subjoined table :—

	Thickness in feet.
9. John O'Groats Red Sandstone, Flagstones, and impure Limestones and Shales	2000
8. Huna Flagstones, Shales, and Limestones ...	1000
7. Gill's Bay Red Sandstones	400
6. Thurso or northern group of Flagstones, Shales, and Limestones	5000
5. Wick or eastern group of Flagstones, Shales, and Limestones passing down into Red Shales and Sandstones	5000
4. Dull Red Sandstones, Red Shales, and fine Conglomerates...	2000
3. Brecciated Conglomerates	300
2. Badbea Red Sandstones and Shales or Clays..	450
1. Coarse basement Conglomerates	50
	16,200 ft.

From the four lowest sub-divisions no fossils have yet been obtained. The flagstones have yielded to Mr. C. W. Peach, and other observers many land plants (some of which resemble forms described by Dawson from the Gaspé sandstones) as well as *Estheria membranacea*, *Pterygotus*, sp., and many ichthyolites. Availing himself of the list of localities furnished to him by Mr. Peach (to whom he cordially acknowledges his obligations) with the species of fish found at each, the author has constructed a table of the vertical distribution of the fossil fishes in Caithness. Some of the species range through almost the entire succession of beds. Some, however, are either peculiar to or very characteristic of one sub-division. Thus *Osteolepis arenatus* and *Dipterus Valenciennesi*, are not noted except from the group No. 5. In the Thurso and the higher flagstones (Nos. 5, 8, and 9) *Acanthodes*, *Parexus*, *Cheiracanthus*, *Diplacanthus*, *Pterichthys*, *Tristichopterus*, and *Holoptychius*—genera absent from the Wick beds—are found in greater or less abundance. These strata are further marked by peculiar species of genera which likewise occur among the older flagstones, as *Coccosteus pusillus* and *Osteolepis microlepidotus*.

The Orkney Islands are assigned to the higher subdivisions of the flagstone series, the protruding ridge of granite and gneiss which rises at Stromness and Gremsa being merely an indication of the irregular surface, on which the deposits of Lake Orcadie were accumulated, and of the slow progressive subsidence of the area. The fossils, for which these islands have long been famous, include most of those of the upper groups of Caithness, with the addition of others which have been regarded as distinct. In the determination of these fossils much skill is required to discriminate between the accidental differences of aspect resulting from the condition of fossilisation. The Orkney fishes, for instance, are preserved as black jet-like impressions which, often very perfect when first removed from the quarry, are apt to scale off, leaving in each case only an amorphous layer which, though it retains the contour of the fish, shows little or no trace of structure. On the shores of the Moray Firth, on the other hand, the organisms have been inclosed within calcareous nodules; their colours are sometimes brilliant, and their scales, plates, fins, and bones, are often admirably preserved and remain unchanged in the Museum. Want of experience in these different modes of preservation may have led to a reduplication of species, especially in the case of the Orkney and Moray Firth fishes. Among the most interesting Orkney fossils is a portion

of a *Pterygotus* (recognised by Dr. H. Woodward), now in the British Museum. The occurrence there of this characteristically Upper Silurian and Lower Old Red Sandstone genus supports the view contended for in this paper as to the true horizon of the Orkney and Caithness flagstones.

The Shetland Islands contain a portion of the shoreline of Lake Orcadie with its conglomerates and sandstones and the flagstones and shales of deeper water. Among these strata the Caithness *Estheria* occurs, with abundant stems and roots of large calamite-like plants with well-marked flutings but without observable joints. Some ichthyolites of the Caithness type are said to have been found in Bressay. The general lithological characters are quite those of the sandy parts of the Orkney and Caithness groups. On the west side of the mainland of Shetland interesting evidence occurs to show the existence of volcanic action contemporaneous with the accumulation of the Old Red Sandstone. Beds of amygdaloidal lavas and bands of tuff occur among the sandstones, the whole being pierced by masses of pink felsite.

The south-western and southern margin of this great northern basin of the Old Red Sandstone can still be traced nearly continuously from the confines of Caithness to the borders of Aberdeenshire, its position being marked by a zone of littoral conglomerates. Beyond the edge of that zone, however, there occur some interesting outliers which in some cases may represent long fjord-like indentations of the coast line; in others may mark what were really independent basins lying at the base of the Grampian Mountains. The author points out that probably most of the difficulty which has hitherto been experienced in understanding the sequence of beds along the southern shores of the Moray Firth and their parallelism with those of Caithness and Orkney is not to be attributed to the amount of detritus covering the country, but rather to the fact which has not heretofore been observed that the Upper Old Red Sandstone with *Holoptychius* and *Pterichthys major* really overlap unconformably upon the older nodular clays and conglomerates with *Coccasteus*, *Cheirolepis*, &c. This relation could be satisfactorily determined in Morayshire, and was now being worked out by Mr. John Horne in the course of the Geological Survey. The author traces in great detail from the Spey into Sutherlandshire, the development of the lower sandstone conglomerates and clays, which have been regarded as equivalents of the Caithness flagstones. He thinks that in no sense can this comparatively thin group of rocks (seldom 1,400 feet in depth) be regarded as a mere southward attenuation of the great Caithness series, as suggested by Murchison, for that neither lithologically nor palæontologically can that view be sustained. He has been led to the conclusion that the whole of these rocks from the borders of Sutherlandshire to those of Aberdeenshire represent only the higher portions of the great Caithness series, and that they were formed during a gradual depression of the ancient high grounds whereby the waters of Lake Orcadie were allowed to creep southward over the descending land. This movement is indicated by the character of the strata, and that it took place about the time of deposit of the later flagstones of Caithness is shown by the occurrence of the fossils of that division in the nodules, flags, and clays of the Moray Firth region, while those of the Lower division are absent.

Allusion is likewise made to the discovery of two localities where contemporaneous volcanic action has recently been observed in the Moray Firth area, the whole of the basin of Lake Orcadie being otherwise remarkably free from any trace of such action except on the northern margin in Shetland. The history of the area embraced by Lake Caledonia will form the subject of the next paper.

NOTES

We regret to have to announce the death of Dr. F. Brüggemann. Dr. Brüggeman was a native of Bremen and studied at Jena, where he was for several years assistant to Prof. Haeckel. His earliest publications were on entomological subjects, but later he published an account of the Amphibians and Reptiles of Bremen. He was especially interested in ornithology, and amongst other papers on this subject published two on the Birds of South-Eastern and Central Borneo (*Abhand. d. naturw. Vereins zu Bremen*, Bd. v. u. vi.). On the recommendation of Prof. Haeckel, Dr. Brüggemann was engaged last year by Dr. Günther to arrange and catalogue the collection of corals in the British Museum. Whilst in the midst of this undertaking he died suddenly at his lodgings on the night of Saturday last of hæmorrhage from the lungs. He had already named 1,500 species of corals in the collection, and had published two papers on undescribed forms in the *Annals and Magazine of Natural History*. He had in hand a revised list of all species of recent corals hitherto described, which was in an advanced state and which he had intended to have published. He was of an extremely amiable disposition and his loss is deeply regretted in London by the staff of the British Museum and other naturalists with whom he was acquainted. He was under thirty years of age at the time of his death.

On Thursday last the members of the General Council summoned to deliberate upon the improvements required in the organisation of the Paris Observatory waited upon M. Bardoux, the Minister of Public Instruction. They insisted upon the necessity of continuing the existing connection between astronomy and meteorology in accordance with the principles established by M. Leverrier himself, and developed the reasons which had led the majority to pass a resolution in favour of that system. A number of eminent scientific men had interviews with M. Bardoux, and have made a strong impression upon his mind. M. Bardoux has ordered all the letters from a number of departmental meteorological commissions to be summarised, and it has been found that not a single one has urged the disconnection of the two departments. We are in a position to state that according to every probability, during the present month, the Academy of Sciences and the new Council of the Observatory will be summoned to present each two candidates, between whom the Minister will exert his right of selection according to the provisions of the newly-published decree.

Captain Feilden, R.A., naturalist to the late British Expedition to the Arctic Regions, and Mr. De Rance, of H.M. Geological Survey, are announced to read a paper on the Geology of the Northern Lands visited, at the next meeting of the Geological Society of London, at which Mr. Etheridge will present a detailed report of the palæontology of the same area. We understand that the British Museum will probably be the destination of the very numerous collection of geological specimens made by Capt. Feilden, Dr. Coppinger, and other officers of the expedition.

M. Belgrand, Director of the Paris Sewers and Waterworks, died suddenly on the 8th inst. in his sixty-eighth year. To him Paris owes its network of sewers and its supply of water from the Dhuys, the Vanne, and the Somme Soude. He also devised the system of hydrological observations, by which floods are foreseen. As a connoisseur of water he is said to have had no rival.

It is stated that Prof. H. J. S. Smith, F.R.S., is to be a candidate for the representation of Oxford University in Parliament.

The coloured spherules discovered by M. Hannover in the cones of the retina of many birds are known to have three colours: a yellowish green, an orange yellow, and an intense

ruby-red. Lately, M. Capranica affirmed the identity of these different colouring matters and their close relation to visual red and the widely expanded lutein (found in the yolk of egg, adipose tissue, corpora lutea, the ovary of mammalia, &c.), and he cited various reactions as proving this relation. M. Kuhne has lately, in the *Centralblatt für die medicin. Wiss.*, opposed this view; he has succeeded easily in isolating the three colouring matters after they were freed from fat, and he affirms that as regards spectroscopic behaviour, reaction, and solubility, they may be clearly distinguished.

THE French Academy had proposed for the prize of eloquence in 1877 the *éloge* of Buffon, the celebrated naturalist, and not less than seventeen memoirs were presented. Two were found so excellent that in opposition to the traditions of the Academy, they were declared *ex æquo*, as having obtained the premium. When the sealed envelopes containing the names of the authors were opened, it was found that one of them had died before he had quite revised his work. The name of this posthumous laureate is M. Narcisse Michaud. M. Dumas has written a letter of sympathy and regret to the family in the name of the Academy.

M. DE WATTEVILLE, one of the chief secretaries of the French Minister of Public Instruction, has lately submitted a plan for the formation of a large scientific committee in Paris, which shall stand in direct communication with all existing learned societies. The project will be put into execution during the present month, and M. Bardoux, the Minister of Public Instruction, will be the first president of the committee.

ON April 4 was held at the Tuileries a meeting of the several committees which had been appointed in order to organise the series of congresses intended to take place in Paris during the Universal Exhibition. After having returned thanks to his numerous subordinates for their exertions, the Minister for Public Works read a list of eleven congresses which are completely organised, viz.:—1. Agriculture. 2. Metrical and monetary, for the adoption of a universal system. 3. Special congress for determining a universal measure of threads of every description used in textile fabrics. 4. For the protection of literary, artistic, and industrial property, patents, &c., &c. 5. For provident institutions, life, fire, agriculture, &c., insurances. 6. Philological. 7. A congress inaugurated by European economists. 8. Meteorological. 9. The French Alpine Club will call a congress of every similar institution. 10. Public hygiene. 11. A congress for the international regulation of measures against the propagation of epizootics. Other congresses are in preparation. The several regulations will be published very shortly, mentioning the dates, the space of time allotted to them, the several programmes, the places of meeting, the conditions of admission, and the composition of initiative commissions.

HERR ACHENBACH, the Prussian Minister of Commerce, has lately issued an order that during the Paris Exhibition arrangements shall be made at the Berlin School of Mines to put at the service of those desiring to study the mineral wealth of the kingdom, all possible cartographical and literary requisites, as well as information as to the best means of reaching all points of interest in the mining regions; this disposition is made more especially for the benefit of American scientific visitors in recognition of the courtesies extended by them in this direction two years ago.

A GUIDE for the approaching Exhibition at Paris has just been published under the title "Guide de l'Exposition Universelle et de la Ville de Paris." (Paris: Bureau de la Publicité.) It contains no less than fifty-four maps and plans.

THE Institute of Naval Architects commences its annual session to-day; the meetings will be continued to-morrow and Saturday. A large number of papers on subjects of great importance are down for reading.

THE agents of the Paris Acclimatisation Society are engaged in organising, at Marseilles, a zoological garden which will be considered as an annexe to the Parisian establishment. A certain number of animals have already arrived but have not yet been placed in the cages which are being built for them.

A PAPER on "State Aid to Music at Home and Abroad" was read by Mr. Alan S. Cole, at the Society of Arts on Wednesday evening, March 27. Allusion was made to the constitution of foreign Conservatoires, which, to a considerable extent, depend upon the support given to them by the governments of the countries in which they are established. Government support gives an element of stability to these foreign Conservatoires, and Mr. Cole endeavoured to show that in the United Kingdom there is an absence of stability in respect of the different music schools which exist. Our academies and schools of music have been founded by private enterprise, and their existence, depending upon the fluctuations of subscriptions and amateur fee-paying students, seems to have no guarantee of permanence. In regard to freely established classes for promoting science and art, the prospect of their becoming permanent is assisted by the offer of national payments for ascertained results of instruction. In elementary day schools the education department makes a payment of one shilling per child who attends a school where singing is taught. These shilling payments amount to 96,000*l.* a year. As, however, the Inspector of Music, Mr. John Hullah, reports that the musical proficiency of the children is bad, it may be inferred that not only is the instruction of the children in music bad, but the payment also of so large a sum as 96,000*l.* per annum is of little use in securing for national benefit an adequate return. The supply of duly qualified teachers in the art and science of music may probably tend to diminish the disproportion between the annual expenditure and the insufficient return of results in musical instruction. Accepting the general features common to Conservatoires abroad as the outlines for similar institutions at home, Mr. Cole called attention to the Royal Academy of Music and to the New National Training School for Music at Kensington. The Royal Academy is not a Conservatoire according to the definition given. The constitution of the National Training School is similar to that of the chief Conservatoires. The tendency of individual or private enterprise seems to direct itself towards the training of singers and peformers; and it was stated that the Kensington School was at present training nearly a hundred scholars of this class. The duty of the Government is to provide qualified teachers, the results of whose instruction shall be of value to the country at large, and therefore properly to be paid for out of the exchequer. The form of State aid which it was suggested might be given was the payment of the fees of instruction of a certain number of students whose aim is to be teachers in elementary schools, in local classes, and music schools throughout the country. Such payment of fees would be made to that academy or training school whose proved methods of instruction seemed to be the best, and the work promoted by this kind of State aid would not compete with that part of national culture which is at present dependent upon the support given according to the whims of the givers, and therefore of an uncertain, spasmodic, unbusiness-like character.

M. CAZIN, the eminent French physicist, whose premature death we noticed a few months since, left a manuscript on Spectrum Analysis. This has just been published by Gauthier Villars in his "Actualités Scientifiques."

THE Annual Meeting of the Cumberland Association for the Advancement of Literature and Science will be held at Cockermouth on Easter Monday and Tuesday. A varied and interesting

programme is arranged for the two days, one of the items being a lecture by Sir George Airy on "The Probable Condition of the Interior of the Earth."

FOR Easter Monday and following day the Geologists' Association have arranged what promises to be an interesting excursion to Chipping Norton. Provincial field societies are now also issuing programmes of their summer excursions; the Manchester Field Naturalists and the Leeds Naturalists have sent us well-arranged programmes of this kind.

EXCAVATIONS in the neighbourhood of Merten, in Lorraine, have uncovered the remains of an old Roman temple, and brought to light a variety of weapons, busts, coins, &c. The indications all point to the existence of a large settlement here under the Roman rule, and arrangements have been made for a series of widely extended excavations.

THE archæologists of Rome are busied over the latest discovery, the uncovering of a cellar containing a thousand vessels for various commercial purposes, two hundred of which are covered with inscriptions throwing no small light on the business terms of the ancient Romans.

IN the course of a report, which has just been published by order of the Inspector-General of Maritime Customs in China, Dr. F. Wong gives us some curious particulars respecting a strange remedial agent employed by the Chinese in cases of *Cynanche Tonsillaris*. The disease they term *Ngo-how*, or "Goose-throat," and the remedy in question is called *How-tsao*, a soft stone not unlike biliary calculus in appearance. It is expensive, being worth twenty times its weight in silver, and is said to come from Siam. Twenty or thirty grains of this in powder, taken in water, is thought to be very efficacious. Dr. Wong mentions having seen a case where this remedy was given, and where it certainly appeared effective, after gargles and astringents had been applied in vain. The specimens of the stone which have come under his notice appear like animal concretions, and are of various sizes, some being smaller than pigeons' eggs, while others are as large as hens' eggs. The story goes that, when a monkey is wounded, the animal, from its natural instinct, picks out the proper medicinal herbs, masticates and applies them to the wound, so that successive layers are in this way laid on so as to form a mass. In time the wound heals, and the lump of dried herbs falls off; it is then picked up by the Siamese, found by them to possess peculiar virtues, and sent in small quantities to China as a drug.

NEWS from Berlin states that Prof. Mommsen has again started upon a scientific expedition to the south of Italy, from which he intends to return to Berlin at the end of May.

IN the south of France no rain or snow has fallen since the beginning of the winter, and the prevailing drought resulting from this peculiar absence of atmospheric moisture has well-nigh assumed the proportions of a real catastrophe. The authorities have been obliged to take in hand the distribution of drinking water to the inhabitants. Between Marseilles and the Italian frontier certain railway stations are completely without water, and waggon-cisterns had to be constructed which are kept filled by water brought by train. The soil in the district is so hard that all agricultural work is impossible, and the crops are, of course, in a most miserable condition.

FROM Leipzig and its vicinity heavy rains are reported causing severe inundations in that neighbourhood.

DR. A. HARTMANN describes in the *Proceedings* of the Berlin Physiological Society for the present year, a new application of the telephone for the purpose of testing the hearing. It rests upon the fact that when the magnet of the receiving instrument is excited by a galvanic stream, the intensity of the tone transmitted can be altered at will by the introduction of various resistances or of Du Bois-Reymond's compensator into the circuit. By this means it is easy to measure comparatively in different persons the limits of hearing, by applying the telephone to the ear, and noticing the amount of resistance necessary in order to extinguish the same sound.

THE American Chemical Society closes its second year with a membership of 300. Its *Transactions*, instead of appearing at irregular intervals, are to be published twice a month, and efforts are being made to concentrate in them all that America produces in the line of chemical research. The Society has chosen Prof. Johnson, the familiar authority on agricultural chemistry, for its president during 1878, and has elected to honorary membership Professors Frankland and Williamson, of London, Bunsen and Wöhler of Germany, Berthelot of Paris, Boutlerow of St. Petersburg, and Cannizzaro of Rome.

AN earthquake was felt at Liesthal, in the canton of Bâle during the night of March 28–29. This phenomenon was probably connected with another commotion which was registered at Strasburg Observatory by Winnecke, and was observed on March 29 at 8h. 52m. 27s. in the morning. The duration of the commotion was only $\frac{1}{2}$s., and would have escaped notice if a registering apparatus had not been kept at the observatory. A violent earthquake was felt at Kaltenbrunn, in the Kaunser Valley (Tyrol) on March 16 at 5 A.M.

MR. A. O. THORLACIUS, the observer for the Scottish Meteorological Society at Stykkisholm, in the north-west of Iceland, reports the occurrence, on March 4, of the severest thunderstorm ever experienced in that part of Iceland. Thunder and lightning continued without interruption from 5.30 A.M. to 8 A.M., accompanied at intervals with rain and hail. For the past thirty-three years, during which Mr. Thorlacius has observed, nearly all the thunderstorms have occurred during the winter months. At 7 A.M. a very fine meteor passed over the village of Stykkisholm and exploded into innumerable fragments over the harbour, unaccompanied, however, with any audible report, and shortly after another fine meteor passed over the village and disappeared without being observed to explode. It is added that this is the first time such meteors have been observed by any one at Stykkisholm.

WITH regard to the fact stated by M. Forel, that frequently during distinct shocks of earthquake, the lakes show neither waves nor *seiches*, while at other times shocks produce large movements, M. De Rossi writes to *La Nature*, from Rome, that the lakes probably act according to the law of pendulums. Thus in Italy shocks of earthquake have frequently occurred without the pendulum seismograph showing any sign of movement, whereas, again, the pendulum may swing violently without the shock being perceived by any one. M. De Rossi has, with others, experienced a distinct shock of earthquake, and on immediately examining with a microscope eight pendulums of different lengths, could not detect the slightest motion. The fact evidently depends, he says, on the relation between the length of the pendulum and the rapidity of the earth-vibrations. When the seismic wave is *synchronous* with the natural oscillation of the pendulum, the latter enters into motion; when it is *dissynchronous*, the pendulum refuses to move.

IT will be seen from our advertising columns, that pending the erection of the permanent buildings of the Channel Islands' Zoological Station, St. Helier's, Jersey, arrangements have been made for placing private rooms with tables and apparatus at the disposal of a limited number of naturalists and students, with every assistance in obtaining subjects for investigation.

THE additions to the Zoological Society's Gardens during the past week include two Persian Gazelles (*Gazella subgutturosa*) from Persia, presented by Mr. R. W. Inglis; a Macaque

Monkey (*Macacus cynomolgus*) from India, presented by Mr. Francis Pym; a Common Squirrel (*Sciurus vulgaris*), European, presented by Madame Hante; a Vulpine Phalanger (*Phalangista vulpina*) from Australia, presented by Capt. F. Ayling; a Pudu Deer (*Cervus humilis*), a Naked-eared Deer (*Cervus gymnotis*) from Chili, a Maned Goose (*Bernicla jubata*) from Australia, purchased; an Egyptian Gazelle (*Gazella dorcas*) from Egypt, deposited; a Frazer's Squirrel (*Sciurus fraseri*) from Ecuador, a Black Sternothere (*Sternothœrus niger*) from West Africa, received in exchange.

UNDERGROUND TEMPERATURE[1]

OBSERVATIONS on a very elaborate scale have been received from the important mining district of Schemnitz, in Hungary. A request for observations was sent by the Secretary, in 1873, to the Imperial School of Forests and Mines at Schemnitz, and on the receipt of two thermometers a Committee was formed to plan and carry out observations. The leading part in the observations has been taken by Dr. Otto Schwartz, Professor of Physics and Mathematics, who has furnished an elaborate report of the results obtained. This is accompanied by a geological report drawn up by Prof. Gustav von Liszkay and by a geological map with plans and sections of the mines.

The two thermometers sent being deemed insufficient for the numerous observations which were contemplated, twenty-five large thermometers were ordered from a local maker (T. T. Greiner), and the ten best of these, after being minutely compared with one of the two thermometers sent—which was non-registering and had a Kew certificate—were devoted to the observations. Three of them were divided to tenths and the others to fifths of a degree Centigrade, and all had bulbs of thick glass to ensure slowness of action. They were found not to change their indications during the time requisite for an observation.

The observations were for the most part taken by boring a hole in the rock to a depth in the earlier observations of ·422, and in the later ones of ·79 of a metre, then filling the hole with water, and after leaving it in some cases for a few hours, in others for several days, to plunge a thermometer to the bottom of the hole, and after thirty or forty-five minutes take it out and read it. The tenths of a degree were read first, and there was time for this to be done before the reading changed. As a rule three observations were taken in each gallery, two of them in bore-holes to give the temperature of the rock, and the third in the air of the gallery at an intermediate position. Pyrites and also decaying timber were avoided as being known to generate heat, and as far as possible currents of air and the neighbourhood of shafts were avoided also.

A table, which forms part of Dr. Schwartz's report, contains observations made in no fewer than thirty-eight galleries. Besides the temperatures, it gives the depth of the place of observation beneath the shaft-mouth and the height of the latter above sea-level. Dr. Schwartz takes exception to a few of the observations in the table, as being vitiated by the presence of pyrites or by currents of air.

All the galleries mentioned in the table are classified according to the shafts with which they are connected, and there are for the most part six of these galleries to each shaft. In the final reductions, Dr. Schwartz compares the temperature in the deepest gallery of each shaft with the assumed mean annual temperature of the ground at the shaft-mouth. For determining this latter element the following data are employed.

The mean temperature of the air at the School of Mines, from twenty years' observation, is 7°·2 C. at the height of 612·6 metres above sea-level. The shaft-mouths are at heights of from 498 to 763 metres above sea-level, and it is assumed that the temperature of the air falls 1° C. for 100 metres of elevation. It is further assumed that the mean temperature one metre deep in the soil is, in these particular localities, 1° C. higher than the mean temperature of the air. The reasons given for this last assumption may be thus summarised:—

1. Observations in various localities show that in sandy soils the excess in question amounts on the average to about half a degree Centigrade.

2. In this locality the surface is a compact rock which is highly heated by the sun in summer and is protected from radiation by a covering of snow in winter; and the conformation of the hills in the neighbourhood is such as to give protection against the prevailing winds. Hence the excess is probably greater here than in most places, and may fairly be assumed to be double of the above average.

Omitting one shaft (Franz shaft), in which, owing to the presence of pyrites, the temperatures are abnormal, the following are the principal results:—

	Depth in metres.	Increase of temp. Cent.	Quotient, or metres per 1° C.	Feet per 1° F.
Elizabeth shaft	417	8·5	49·1	89·5
Maximilian ,, ...	253	6·4	39·5	72·0
Amalia ,, ...	285	8·1	35·2	64·2
Stefan ,, ...	218	7·2	30·3	55·2
Siglisberg ,, ...	414	8·1	51·1	93·2
Sums, &c.	1587	38·3	41·4	75·5

The best mode of combining the results from these five shafts is indicated in the last line of the above table, where the sum of the depths is compared with the sum of the increments of temperature. We have thus a total increase of 38°·3 C. in 1,587 m.; which is at the rate of 1° C. in 41·4 m., or 1° F. in 75·5 feet.

As these results depend on an assumption regarding the surface-temperature, it seems desirable to check them by a comparison of actual observations, namely, by comparing the deepest with the shallowest observation in each mine. We thus obtain the following results:—

	Difference of depth, metres.	Difference of temperature, Cent.	Quotient metres per deg. Cent.	Feet per deg. Fahr.
Elizabeth shaft	145·2	4·6	31·6	57·6
Maximilian ,, ...	191·6	3·9	49·1	89·5
Amalia ,, ...	228·2	5·1	44·8	81·7
Stefan ,, ...	82·0	4·7	17·4	31·7
Siglisberg ,, ...	400·3	8·0	50·0	91·2
Sums, &c.	1047·3	26·3	39·8	72·5

Combining these results in the same manner as the others, we have a total difference of 26°·3 C. in 1047·3 metres, which is at the rate of 1° C. in 39·8 metres, or 1° F. in 72·5 feet.

The near agreement of this result with that obtained from comparison with the assumed surface-temperature is very satisfactory. The mean of the two would be 1° F. in 74 feet.

The rocks consist, for the most part, of trachyte and greenstone.

Dr. Schwartz concludes his report with the suggestion that the heat developed by the decomposition of pyrites and galena in seams which are not altogether air-tight and water-tight, may possibly be utilised as a guide to the whereabouts of metallic lodes; and that "we shall thus obtain, by means of the thermometer, scientific information which the ancients sought by means of the divining-rod."

Thanks are due to M. Antoine Péch, Ministerial Councillor, and Director of the Mines, and to Herr Edouard Pöschl, Director of the School, for energetic co-operation in this extensive and valuable series of observations.

Mr. Lebour, having been requested to supplement the above *résumé* of the Schemnitz observations by an account of the connection (if any) between the geological and thermal conditions of the several mines, as indicated by a comparison of the reports of Dr. Schwartz, and Prof. von Liszkay, remarks:—

"The rock at all the mines except Franzschacht is green

[1] Report of the British Association Committee on Underground Temperature, by Prof. Everett.

hornblende-andesite (*in German* Grünstein-trachyt), a compact fine-grained crystalline, more or less vitreous rock, containing crystals of oligoclase and hornblende, but *no quartz or sanidine*. This rock is a good heat-conductor, with a conductivity probably nearly approaching that of 'Calton trap rock.'

"The Franzschacht is sunk in rhyolite (a highly siliceous vitreous trachyte), a rock, the conductivity of which would presumably be nearly the same as that of hornblende-andesite, probably a little greater. Elements of temperature-disturbance are, however, present in the form of thermal springs, and, possibly, in the proximity of a basaltic cone. This last element of disturbance is, I should imagine, a very doubtful one indeed, although Councillor A. Péch appears to think it of importance. The rate of increase, as deduced from observations in the rhyolite here, was 1° C. for 40·55m., or about 1° F. for 74 feet.

"The report brings out strongly the important variations of rock-temperature which may be, and are occasionally, generated by the decomposition of metallic sulphides, a point which I think is here prominently mentioned for the first time."

At the request of Mr. Lebour, observations have been taken by Mr. Matthew Heckels, Manager of Boldon Colliery, between Newcastle and Sunderland, in holes bored upwards to a distance of ten feet from some of the deepest seams.

The mine is described as "perfectly dry," and those parts of it in which the observations were made are quite free from currents of air. The surface of the ground is tolerably level, and is ninety-seven feet above Trinity high-water mark.

Hole No. 1 is bored up from the roof of the Bensham seam. The thermometer—one of the new slow-action instruments, not self-registering—was placed at the end of the hole (so as to be ten feet within the rock) and protected by air-tight plugging. The surrounding strata consist of arenaceous shale, known as "grey metal." The distance of the thermometer from the surface of the ground overhead was 1,365 feet.

The hole had been standing idle for some time when the thermometer was inserted, April 5, 1876. The first reading was taken April 26, and was 75°, the surrounding air being at 75½°, and almost stagnant. The readings were repeated during four consecutive weeks, without change of the indications.

Hole No. 2 is in the same vertical with No. 1, and is bored up (also to the height of ten feet) from a deeper seam—the Hutton seam. The same thermometer was employed, and in the same manner. The surrounding strata consist of a close, compact sandstone known as "hard post." The distance of the thermometer from the surface of the ground overhead was 1,514 feet. Immediately after the drilling of the hole, June 6, 1876, the thermometer was inserted, and on July 4 the first reading was taken, namely, 81°. On July 24 it had fallen to 79½°, and on August 1 to 79°. Readings taken on August 15 and 29 and September 1 also showed 79°, the surrounding air having never altered from the fixed temperature, 78½°. It would therefore appear that the first observation in this hole was 2° too high, owing to the remains of the heat generated in boring, notwithstanding the lapse of four weeks which had intervened. Four readings have since been taken at regular intervals, ending with July, 1877, and the same temperature, 79°, continues to be shown.

Assuming 48° as the mean annual temperature of the surface, we have the following data for calculating the rate of increase downwards:—

Surface	48°
1,365 feet	75°
1,514 feet	79°

For the interval of 149 feet between the two holes we have an increase of 4° F., which is at the rate of 1° F. in 37 feet.

For the whole depth of 1,514 feet from the surface to the lower hole we have an increase of 31°, which is at the rate of 1° F. in 49 feet.

In explanation of the length of time required for the heat of boring to disappear in the second hole, Mr. Heckels remarks that "it required two men sixteen hours with a hand-boring machine to drill this hole, so hard is the stratum." He further says: "The tool by which this hole was bored, on being drawn out, was too hot to allow it being touched with the hand, so that the temperature of the hole, on being finished, must have been considerable; and no doubt it would be when we consider the immense pressure required to bore holes in such strata as this." With respect to the permanent temperature, 78½°, of the surrounding air, Mr. Heckels remarks: "The air of this district is almost stagnant, and what circulation there is will have travelled a distance of three miles underground; and hence it may be expected to be itself pretty near the temperature of the rocks through which it is circulating."

The dryness of the mine, the absence of currents of air, and the great depth render these observations extremely valuable for the purpose which the Committee have in view, and their best thanks are due to Mr. Heckels and the proprietors of the colliery for the trouble and expense which have been incurred in procuring them. Observations will shortly be taken in another bore in the same colliery.

During the past year the first observations have been received from India. They were taken by Mr. H. B. Medlicott, M.A., of the Geological Survey, in bores made in search of coal, and have been published by him in the "Records of the Geological Survey of India," vol. x., part 1. The instrument employed was a "protected Negretti" thermometer sent by the secretary of this Committee to Dr. Oldham, the director of the Survey. A Casella-Miller thermometer was used to check the observations, but was found much less sensitive and steady, and its readings, though placed on record, are therefore left out of account by Mr. Medlicott in his reductions.

The observations were taken in three bores, at places named Khappa, Manegaon, and Moran; but the observations at Moran were made only four hours after the boring tool had been at work, and the Khappa bore exhibited a strong bubbling, besides other marks of convection. The results obtained at these two bores must therefore be discarded; but in the Manegaon bore everything was favourable for satisfactory observation. "It was closed on April 24, 1875, so that it had been at rest for twenty months. There is only one guide-pipe ten feet long at the top of the bore, there never having been any pressure of water in the hole. The position is low, and the water had always stood at or near the mouth of the tube. There was no difficulty in removing the plug. The very equable series of temperatures is the natural result of these conditions. The observations were taken in the evening of the 5th and morning of the 6th of December. At 5 P.M. the air-temperature was 72°; at 8 P.M., 59°; at 8 A.M., 65°; at 11 A.M., 84°. The slight decrease of temperature in the top readings is a good proof of the perfectly tranquil conditions of observation. It is no doubt due to the excess of summer heat not yet abstracted; and it is apparent that that influence reaches to a considerable depth—quite to sixty feet." The following are the observations:—

Depth, feet.	Temperature, Fahr.	Depth, feet.	Temperature, Fahr.
10	81·15	150	82·7
20	81·1	200	83·3
40	81·0	250	84·0
60	81·0	300	84·65
80	81·3	310	84·70
100	81·8		

This last observation was in mud, the hole, which had originally a depth of 420 feet, having silted up to such an extent that 310 feet was the lowest depth attainable. The increase from 60 feet downwards is remarkably uniform, and the whole increase from this depth to the lowest reached is 3°·7, which is at the rate of 1° F. for 68 feet.

The elevation of Manegaon is estimated at 1,400 feet. It lies "in an open valley of the Satpuras, traversed by the Dudhi River, south of the wide plains of the Narbada Valley, about halfway between Jabalpur and Hoshungabad, which are 150 miles apart." Jabalpur is 1,351 feet above sea-level, and has a mean annual temperature of 75·2. Hoshungabad is 1,020 feet above sea-level, and has a mean annual temperature of 78·3.

"The geological conditions of the position are favourable for these observations. The rocks consist of steady alternations, in about equal proportions, of fine softish sandstones, and hard silty clays of the upper Gondwana strata, having a steady dip of about 10°. . . Strong trap dykes are frequent in many parts of the stratigraphical basin; but there are none within a considerable distance of these borings. There are no faults near, nor any rock-features having a known disturbing effect upon the heat-distribution."

Mention was made in last report (p. 209) of two methods which had been suggested by members of the Committee for plugging

bores to prevent the convection of heat. Mr. Lebour, at the request of the Committee, has conducted experiments during the past year on both forms of plug. He reports that:—

"In accordance with Sir W. Thomson's suggestion, discs of india-rubber fixed to the lowering wire above and below the thermometer have been tried. The chief difficulty met with was the unwieldiness of the armed portion of the wire, which could not be wound and unwound from the drum, owing to the fixed disc-holders. This difficulty prevented the placing of the discs anywhere but at the extremity of the wire, whereas it would be very desirable to have a large number of them at intervals along the greater part of its entire length. Discs for a 2½-inch bore were found to work well with a diameter of 2¼ inches. The lowering, and especially the raising, of the wire armed with the disc-plugging were very slow operations, owing to the resistance opposed by the water to the passage of the discs.

Experiments with the form of plug devised by Mr. Lebour himself were continued with a set of better made plugs. "The great disadvantage of this system of plugging is the necessity for using two wires, one to lower the thermometer and plug as usual, and the other to let down weights upon the upper ends of the plugs, when they are to be expanded, and to remove them when they are to be collapsed. This necessitates not only the ordinary drum for the first wire, but also an independent reel for the second. With care, however, and after some practice, the apparatus was found to work well; but it certainly is extremely inconvenient for rapid work, as it requires a good deal of setting up."

Experiments were made with both forms of plug at the depth of 360 feet, in a bore of the total depth of 420 feet. In the one case, eight india-rubber discs were employed, four above and four below the thermometer; in the other, two collapsible plugs, one above and the other below. The experiments had chiefly in view the mechanical difficulties of the subject, and are not decisive as to the sufficiency of the plugs to prevent convection.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

PROPOSED NEW UNIVERSITY.—A movement has for some time been on foot for the establishment of a new university in the north of England, and on Tuesday last week a deputation, which included the Rev. Dr. Gott (Vicar of Leeds), Mr. Edward Baines, Prof. Thorpe, Prof. Rücker, and Mr. R. Reynolds, waited upon the Mayor of Bradford, Mr. B. Priestly, with the object of inducing the Corporation of Bradford to adopt a memorial to the Privy Council in favour of the proposal. The Mayor intimated that the matter would be referred to the Finance and General Purposes Committee of the Corporation for consideration.

FRANCE.—A commission of twenty-two members has been appointed by the Chamber of Deputies of the French Republic, to prepare a general law on primary instruction.

Two new professorships of botany have been created in the faculties of Lille and Rennes.

PARIS.—The medical course at the University is attended at present by 23 ladies, including 12 Russians, 6 English, and 5 French. Since 1865, 30 ladies have studied medicine at Paris, 9 of whom have received the doctor's diploma.

HIGHER FEMALE EDUCATION.—The subject of the admission of female students to the universities is exciting at present an unusual degree of discussion in Germany as well as in England. In this connection we notice the publication of a letter from Prof. G. H. Meyer, of the medical faculty of Zurich, in which he states, as the result of the experience of a number of years with female students, that he can detect no difference in the average amount of talent and application shown by the representatives of the two sexes under his charge. From a social as well as a professional standpoint, the advanced position taken by the University of Zürich in this direction, during the past few years, is shown to be justified.

KÖNIGSBERG.—The university is attended at present by 655 students, including 42 in the theological faculty, 174 in the legal, 134 in the medical, and 305 in the philosophical. But 42 are from outside of Prussia. The corps of instructors numbers 40. The university possesses a library of 155,000 volumes, an observatory, the zoological museum founded by von Baer, and numerous clinics. On February 2 the eminent philosopher, Herr Rosenkranz, celebrated the fiftieth anniversary of his receiving his doctor-diploma. The German Emperor, the Crown Prince, and all the German Universities, sent congratulatory telegrams and addresses.

HALLE.—On February 27 the 150th anniversary of the establishment of an agricultural chair was celebrated at the Halle University. At the same time the fifteenth anniversary of the opening of the Halle Agricultural Institute, under the direction of its founder, Prof. Kühn, was solemnised. A torchlight procession and banquet were followed by the laying of the foundation-stone for a new geological museum, which is principally destined to contain a geognostical collection of the most important formations in their natural form and succession.

MUNICH.—The rapid increase in the attendance shows that this young University is taking a leading position in Germany. At present the students number 1,360, an increase of over 200 on 1876–77. The philosophical faculty contains 400, and the medical 340. Countries outside of Bavaria are represented by 346. The corps of instructors number 114.

GIESSEN.—The university is attended at present by 315 students, of whom 237 are natives of Hesse. There are but 16 students of chemistry, a striking contrast to the numbers which were wont to flock from all quarters to Liebig's laboratory.

MARBURG.—The number of students in attendance on the university during the past winter was 415. They were divided among the faculties as follows:—Theology 51, law 85, medicine 100, philosophy 179. The Prussian students numbered 263.

BONN.—The professorship of geology and palæontology in this university has been offered to the well-known geologist, Prof. von Seebach, of Göttingen.

KIEL.—The vacant chair of botany is to be filled by Prof. A. Engler, of Munich.

DRESDEN.—A congress of representatives from all the German technical institutions is to take place at Dresden shortly after Easter.

LEIPZIG.—A young lady has taken here, for the first time, the degree of Doctor of Jurisprudence in the legal faculty.

PRUSSIA.—The number of legal students in the various universities has increased so rapidly of late years that they now form three-tenths of the total number.

GERMANY.—From statistical results published by the *Neue Deutsche Schul Zeitung*, it is shown that 60,000 schools with 6,000,000 pupils are in existence in Germany, for a population of about 40,000,000 inhabitants.

MADRID.—The Royal School of Mines has recently celebrated its 100th anniversary and published a handsome historical work in commemoration of the event.

UPSALA.—The University is attended at present by 1,370 students, consisting of 331 in the theological faculty, 145 in the legal, 181 in the medical, and 713 in the philosophical. The corps of instructors numbers 110, including 30 ordinary and 9 extraordinary professors.

SCIENTIFIC SERIALS

Reale Istituto Lombardo di Scienze e Lettere, Rendiconti, vol. xi., fasc. i. and ii.—On some propositions of Clausius on the theory of potentials, by M. Beltrami.—On the composition of cheeses, and on the emanation of fat from their albuminoid substances during maturation, by MM. Musso and Menozzi.—On determination of the nitrogen in milk and its products, by M. Menozzi.—On the resistance of the helices of telegraphic electro-magnets, by M. Ferrini.—Experimental researches on heterogenesis; on the limit of productivity of organic solutions (third communication), by MM. Maggi and Giovanni.—Chemical manures, the agrarian industry, and funded property, by M. Gaetano.—On a reaction of substances reductive in general, and in particular of glucose, by M. Pollacci.—On granite in the serpentine formation of the Apennines, by M. Torquato.

Morphologisches Jahrbuch, vol. iv. part 1, commences with a paper of 111 pages by Max Fürbringer on the comparative anatomy and development of the excretory organs of vertebrata. Nearly fifty figures are given to illustrate the early stages of these organs in the common frog and salamander, a full *résumé* is given of all observations on those of other vertebrates; together with a discussion on their homologies, and on their indications of relationship to the segmental organs of worms.—A careful description of the anatomy of *Isis neapolitana*, n.sp., is given by

G. von Koch.—Dr. H. von Ihering's contribution to the anatomy of Chiton deals chiefly with the sexual apparatus, the kidney, and the muscles. He show that in Chitonidæ the sexes are undoubtedly separate, and that the ova are fertilised in the ovary.—Observations on the formation, fertilisation, and segmentation of the animal egg, by Oscar Hertwig, part 3, 20 pages, 3 plates. This part deals with the ova of the star-fish, Asteracanthion.

Zeitschrift für wissenschaftliche Zoologie, vol. xxx. part 2.—Contribution to the knowledge of the flagellate infusorians and some related organisms, by O. Bütschli, 78 pp. 5 plates, describing or criticising a great number of species.—On the lungs of *Birgus latro* (land crab), by C. Semper.—The copulatory organs of plagiostomes, by K. R. Petri, 48 pp. 3 plates.—The central nervous system of the alligator, by Rabl-Rückard, 38 pp. 2 plates.

SOCIETIES AND ACADEMIES

London

Royal Society, March 28.—"Measurements of Electrical Constants. No. II. On the Specific Inductive Capacities of Certain Dielectrics," by J. E. H. Gordon, B.A. Camb. First Series. Communicated by Prof. J. Clerk Maxwell, F.R.S. (Abstract.)

The author has, under Prof. Clerk Maxwell's directions, carried out some measurements of specific inductive capacities by a new method.

The author finds that all his results are much lower than those obtained by previous experimenters, and suggests that the fact may perhaps be explained on a supposition that the specific inductive capacity of dielectrics increases from an inferior to a superior limit during the first small fraction of a second after the commencement of the electrification. He discusses this question at some length in his paper.

"On the Thermo-Electric Properties of Liquids," by G. Gore, LL.D., F.R.S.

In this communication the author has described an improved apparatus for examining the thermo-electric properties of liquids, by the use of which, with the precautions stated, all sources of error in such experiments appear to be removed; he has also described a number of experiments he has made with it, and the results obtained.

By employing a sufficient number and variety of electrically-conducting solutions, of acids, salts, and alkalies, in those experiments, he has discovered several exceptions to the usual effect he had formerly obtained, viz., that acid liquids are thermo-electro-positive, and alkaline ones thermo-electro-negative, and has sketched a diagram representing the thermo-electric behaviour of heated platinum in three of the exceptional liquids.

Reasoning upon the satisfactory results obtained, he concludes:—(1) That the electric currents are not produced by chemical action; (2) Nor by a temporary disassociation of the constituents of the liquid; (3) Nor by the action of gases occluded in the metals; (4) But that they are produced purely and solely by the heat, and that heat disappears in producing them; (5) That they are immediate or direct effects of the heat, and that aqueous conducting liquids, therefore, possess true thermo-electric properties; (6) That the current is a result of a difference of thermic action at the surfaces of the two pieces of metal; (7) That it is a product of a suitable molecular structure of the liquid, a change of such structure resulting from alteration of temperature, and a direct conversion of heat into electricity; and (8) That the circumstance which is most influential in enabling heat to produce the currents, and most determines their direction and amount, is a suitable molecular structure of the *liquid*.

By means of the apparatus and process described, he has discovered irregular molecular changes in several of the liquids examined; and as molecular changes are the bases of various physical and chemical alterations, he suggests the use of this apparatus and method as a new one for discovering anomalous molecular alterations, and other coincident physical and chemical ones, in electrically conducting liquids; and for detecting differences of electric potential between metals and liquids at different temperatures.

By reasoning upon the different results obtained, he concludes also as probable, that when a piece of metal is simply immersed in a suitable liquid, a change of temperature occurs; and this (if correct[1]) is a parallel fact to that of the production of electricity by simple contact only. The results also support the contact theory of voltaic electricity.

The paper concludes with several suggestions of new lines of research suggested by the experiments, one of which is the construction of a new thermo-electro-motor.

Chemical Society, March 30.—Anniversary meeting.—Dr. Gladstone, president, in the chair.—The following is a brief summary of the president's address:—The bye-laws have been thoroughly revised. Successful efforts have been made to expedite the publication of the *Journal*, and a sub-editor, Mr. C. E. Groves, has been appointed. The Research Fund now amounts to 4,000*l.*, and already two papers have resulted from the assistance rendered by it to investigators. The President hopes that many chemists, especially those to whom the pursuit of chemistry has become a source of wealth, will contribute to this important fund. During the past year an independent body, the Institute of Chemistry of Great Britain and Ireland has been formed and incorporated; its objects, which are quite distinct from those of the Chemical Society, are the encouragement of the study of chemistry and the maintenance of the profession on a sound and satisfactory basis. Sixty-five papers have been read during the past session, and two lectures have been delivered. There are at present 965 Fellows. The Society has lost by death one eminent foreign member, M. Regnault, and, besides, Messrs. R. Apjohn, J. J. Griffin, W. Gossage, T. Hall, E. L. Koch, M. Murphy, Dr. Noad, and E. F. Teschemacher. After several votes of thanks, &c., the following officers were elected for the ensuing year:—President—J. H. Gladstone, Ph.D., F.R.S. Vice-presidents—F. A. Abel, C.B., Sir B. C. Brodie, W. De la Rue, E. Frankland, A. W. Hofmann, W. Odling, Lyon Playfair, A. W. Williamson, T. Andrews, W. Crookes, F. Field, N. S. Maskelyne, H. E. Roscoe, R. Angus Smith. Secretaries—W. H. Perkin and H. E. Armstrong. Foreign Secretary—Hugo Müller. Treasurer—W. J. Russell. Council—Lothian Bell, M. Carteighe, A. H. Church, W. N. Hartley, C. W. Heaton, D. Howard, G. Matthey, E. Riley, W. A. Tilden, R. V. Tuson, R. Warington, C. R. A. Wright. During the meeting it was announced that Mr. Warren De la Rue had presented the Research Fund with the sum of 100*l.* on the condition that it should be devoted to any one important research.

Anthropological Institute, March 12.—Mr. John Evans, D.C.L., F.R.S., president, in the chair.—Prof. A. Graham Bell read a paper on the natural language of the deaf and dumb. The author stated that in most cases dumbness was merely a consequence of deafness, and does not arise from any deficiency in the vocal organs, but merely from the inability to acquire articulate language, from want of means of imitating it. This can be supplied by teaching. The dogma, "without speech, no reason," is not well founded. Deaf-mute children think in pictures. Thence they form a language of signs which, as contractions of it become understood, develops into a conventional language, but its extent is very limited. No deaf-mute has been found who had formed the idea of a Supreme Being. About the commencement of the present century the Abbé de l'Epée opened an institution for the education of deaf-mutes. The tendency of education was to render the language more and more conventional by means of contractions. Of this Mr. Bell gave many interesting examples. The result of systematic education has been to enable the deaf-mutes to form a community among themselves, using a real language, representing abstract ideas as well as mere objects. Not only so, but the language has idioms of its own; for example, the objective case comes first—thus, "the boots made the bootmaker." This is a difficulty, and perhaps a mistake in the education; it affords, however, a useful subject for anthropological inquiry into the analogy with the development of spoken language. In illustration, Mr. Bell delivered the Lord's Prayer in the sign language. The North American Indians have a sign language, the same in character, but less developed, than that of the deaf-mutes. The language of the deaf-mutes is beginning to split into dialects.

Photographic Society, March 12.—J. Glaisher, F.R.S., president, in the chair.—Papers were read by Dr. van Monckhoven on the fading of carbon prints, and the suppression of

[1] Since writing the paper he has proved, by experiment, that when a sheet of platinum is immersed in various saline, alkaline, and acid liquids, a slight rise of temperature takes place; the solutions already employed, in which such a result occurs, are enumerated.

bichromates in carbon printing, and by Edwin Cocking, on non-converging perpendiculars in architectural photographs. Dr. Monckhoven, in his paper, asserts that neither hot water nor alum fix carbon prints, and although excess of bichromate of potash is removed, still the chromic salt, which has rendered the gelatine insoluble, not only remains, but undergoes a change by subsequent exposure to light, and thus accelerates the action of light upon the organic colour of the pigment, which fades rapidly. He suggests a new fixing agent, bisulphite of soda, and for colour some of the oxides of iron, mixed when moist, with glycerine and gelatine, which he states are absolutely unalterable by exposure to light.

EDINBURGH

Royal Society, March 8.—Sir William Thomson in the chair.—Prof. Tait read a paper on thermal conductivity, the result of experiments during the last ten years. His results for iron are much the same as those obtained by Principal Forbes. He had solved the following problems:—1. That, with the exception of iron, in no case as yet tried does a pure metal diminish in thermal conductivity as the temperature rises. 2. That different specimens of the same metal, as, for instance, two kinds of copper differ much the same relatively in thermal and in electric conductivity. 3. A substance which is pretty constant as a conductor of electricity is also pretty constant in thermal conductivity. Among the difficulties encountered was the alteration of the zero point of the thermometers used—Kew standards—after being heated to a high temperature. This affects only the absolute values slightly, but not the general character of the results. Another difficulty was the oxidation, during heating, of the short bars employed to measure the heat lost by radiation and convection at different temperatures. This was almost completely overcome.—Prof. Fleeming Jenkin and Mr. J. A. Ewing communicated a paper on the wave forms of articulate sounds obtained by the aid of the phonograph. Their results show that Helmholtz's theory of vowel sounds, viz., that for the production of any one vowel certain fixed notes are necessary, is not tenable, as they obtained vowel sounds under circumstances which rendered the presence of some of these notes impossible. They have also made out that every vowel and every consonant is reversible. This is true also of such single sounds as *ng*, *th*, *ch*, &c. A number of curves were exhibited showing the form of the indentations on tinfoil produced by various articulate sounds, multiplied about 400 times by means of a system of levers.—A paper by Mr. George M'Gowan on the action of the chlorides of iodine on acetylene and ethylene, was read by Mr. J. Y. Buchanan.

PARIS

Academy of Sciences, April 1.—M. Fizeau in the chair.—The following papers were read:—On some applications of elliptic functions (continued), by M. Hermite.—Parameters of elasticity of solids, and their experimental determination, by M. de Saint-Venant.—On the specific heats and the heat of fusion of gallium, by M. Berthelot. The liquid specific heat was found to be 0·0802; the solid, 0·079. Referred to 69·9 as the atomic weight, the heat of fusion was 1·33 cal. As with mercury, lead, tin, and bismuth, the solid and liquid specific heats, taken at the same temperature, are closely alike. The specific atomic heat of gallium (liquid 5·59, solid 5·52) is about the same as that of aluminium (5·53) and that of glucinium (5·64).—Action of oxygen on acid chlorides, bromides, and iodides; compounds of aluminium, by M. Berthelot.—On the movements of storms, by M. Faye.—On the whirlpools of watercourses, by M. Belgrand. He notices some phenomena of streams as illustrating M. Faye's theory.—Observations on the nature of the plants collected in the group of *Nœggerathia*; types of *N. flabellata*, Lindl. and Hutt., and *N. cyclopteroides*, Gœpp., by M. de Saporta.—The conidia of *Polyporus sulfureus*, Bull, and their development, by M. de Seynes.—Action of the sun on the magnetic and electric fluids of the earth, by M. Quet. The subject is treated mathematically.—On the linear differential equation which connects with the modulus the complete function of the first species, by M. Tannery.—On the kinematics of continuous figures on curved surfaces, and, in general, in plane or curved varieties, by M. Levy.—Actinometric measurements made in Algeria during the summer of 1877, by M. Violle. These were partly made in the dry Saharan climate of Laghouat, 466 kilom. south of Algiers, partly at Fagrait, a height of 993 m., and at Khanza, 740 m. lower. The method was the same as M. Violle used on the top of Mont Blanc two years ago. The numbers obtained for the solar constant in the former case, by Pouillet's and Forbes' formulæ, were 2·40 and 2·42; both less than 2·54, the value got on the top of Mont Blanc. The ratio of the intensities of solar radiation in the plain and on the mountain was 0·915.—On astronomical refraction, by M. Makarevitsch.—On the physical properties and the specific heat of glucinium, by MM. Nilson and Pettersson. They obtained large quantities of crystalline glucinium by heating to a red heat a cylindrical mass of iron, containing, in a hole closed with a screw, some of the chloride and some sodium freed from its oil of naphtha. The compound of marine salt and glucinium found after cooling, is washed with water, and the reduced metal (impure) appears in bright spangles, or dendrites, or small globules. The density of pure glucinium is calculated to be 1·64; specific heat 0·4084. The atomic weight Be = 13·8, and the formula for the oxide Be_2O_3 (assigned by Berzelius) are confirmed.—On a reaction peculiar to some polyatomic alcohols, by M. Klein. It is shown that all the ethers of mannite and its derivatives possess rotatory power.—On a new method of separation of arsenic from other metals, by MM. De Clermont and Frommel. This is based on the fact that while a large number of hydrates of sulphides are dissociated at 100° into sulphuretted hydrogen and oxide, sulphide of arsenic is the only one which gives a soluble oxide, arsenious acid. Hence, if a mixture of sulphide of arsenic and other sulphides be brought to boiling, the sulphides will all be oxidised, and remain insoluble in the water, except arsenious acid, which may then be easily isolated.—On melilotol, by Mr. Phipson. This is a new oily product got by distilling with water, dried *Melilotus officinalis*, then treating the distilled water with ether which dissolves the substance, so that it is got very pure after evaporation. To it is due the odour of melilot and hay.—Telephone employed as galvanoscope, by M. D'Arsonval. The worst constructed instrument is found to be at least 100 times more sensitive than the nerve for revealing weak electric tensions. It is very well adapted for studying the electric tetanus of muscle.—On anthrax in the horse and the dog; phlogogenic action of anthracic blood, by M. Toussaint. The phlogogenic matter accompanying the bacterides is more or less active according to the source whence these latter come.—On the epoch of formation of the cloaca in the embryo of the common fowl, by M. Cadiat.

GÖTTINGEN

Royal Society of Sciences, January 5.—On a class of differential equations which are integrable by Abel's or elliptic functions, by M. Fuchs.—On the affinities and systematic significance of *Ceroxylon andicola*, by M. Drude.—Some words on the origin of language, by M. Benfey.

CONTENTS

THURSDAY, APRIL 18, 1878

THE COMING TOTAL SOLAR ECLIPSE

THERE is no doubt whatever that the eclipse which will sweep over the United States next July will be observed as no eclipse has ever been observed before. The wealth of men, the wealth of instruments, and the wealth of skill in all matters astronomical, already accumulated there, makes us Old Country people almost gasp when we try to picture to ourselves what the golden age will be like there, when already they are so far ahead of us in so many particulars.

Draper, Hall, Harkness, Holden, Langley, Newcomb, Peters, Peirce, Pickering, Rutherfurd, Trouvelot, and last, but not least, Young, are the names that at once run easily off the pen to form a skeleton list, capable of considerable expansion with a little thought, when one thinks of the men who will be there. One knows too that all the enthusiasm of devoted students and all the appliances of modern science—appliances in the creation of which many of those named have borne so noble a part—will not be lacking. So that we may be sure that not only all old methods but all possible new ones will be tried to make this year one destined to be memorable in the annals of science side by side with 1706, 1851, 1860, and other later years.

Thank Heaven, too, there is no necessity that the thankless task of organising an "Eclipse Expedition" from this country should fall on any unfortunate individual, among other reasons because—and this is a very hopeful sign of increasing general interest taken in scientific work—Messrs. Ismay, Imray and Co., the owners of the White Star Line, have expressed in the warmest manner their desire to aid English observers by a considerable reduction of fares, and the directors of the Pennsylvanian Railway Company, as the readers of NATURE have already been made aware, have done the like in the case of observers coming from Europe in their individual capacity.[1]

The progress in that branch of knowledge which requires the aid of eclipse observations has been so rapid during the last few years that the eclipse of 1868, though it happened only ten years ago, seems to be as far removed from the present as the Middle Ages are in regard to many other branches of culture. The work done by the spectroscope since that year, when in the hands of Janssen, Pogson, Herschel, and others, it added so enormously to our knowledge, has gradually covered larger and larger ground, and each successive eclipse in 1869, 1870, 1871 and 1875, has seen some variations in its use, so that its employment has proved the most novel, if not the most powerful, side of the attack.

Young's work of 1869 will no doubt form the key-note of much that will be done this year so far as the coronal atmosphere is concerned. It will be remembered that Young in 1869 observed a continuous spectrum, while Janssen in 1871 observed a non-continuous one, for he recorded the presence of the more prominent Fraunhofer lines, notably D. This positive observation from so distinguished an observer demands attention, not only on its own account, but because of the question which hangs upon it, which is this: Does the corona reflect solar light to us or does it not, and if it does, *where* are those particles which thus act as reflectors? On this point the photographs taken in Siam in 1875 are silent, as the method employed was not intended to discriminate between a continuous and a discontinuous spectrum.

But although this point remains, how greatly has the ground been cleared since 1869. That wonderful line, "1474," is more familiar to us now! and yet there has been almost a chapter of accidents about it. In the first place, with regard to this line above all others, there appears to be a mistake in Ångström's map; the solar line at 1474 is not due to iron at all; with the most powerful arc there is no iron line to be seen there. Then Secchi attributed it to hydrogen, though I am not aware on what evidence. But whatever be its origin, the fact remains that we now know by its means that the solar hydrogen is traversed and enwrapped by the substance which gives rise to the line to an enormous height, so that it forms the highest portion of the atmosphere which is hot enough to render its presence manifest to us by spectral lines. Here, so far as I know, only one point of difference remains. In 1871 I most distinctly saw the line trumpet-shaped, that is, with the base broadening as the spectrum of the photosphere was reached, while Janssen saw it stopping short of the spectrum of the photosphere. The importance of this point is that supposing one of us to be mistaken and one or other observation to represent a *constant* condition, then, if the line broadens downwards till the sun is reached we are dealing with a gas lighter than hydrogen, capable of existing at a high temperature, which thins out as the other gases and vapours do in consequence of its vapour density being below that of hydrogen; or, on the other hand, if the line stops short as a constant condition, it represents a substance which is probably dissociated at the lower levels, and is therefore probably a compound gas; and then the question arises whether it has not hydrogen as one of its constituents.

Perhaps I may conveniently refer to a paper of mine which was read at the Royal Society last Thursday in this connection, because it may be that the solar regions most worthy of the closest study at the present time are precisely these higher reaches of the sun's atmosphere. There is little doubt, I think, that around the sun's visible atmosphere matter exists at a temperature low enough not to give us its autobiography in the bright line manner, and there is evidence that matter existing under such conditions, absorbing as it must do some of the sun's light, will, if it remains elemental, give us an absorption of the fluted kind, or again will absorb only in the blue or ultra-violet region.

Now the more the chemistry of the reversing lower layer of the sun's atmosphere—that in which the upper level of the photosphere is bathed—is examined the more metallic is it found to be. For instance, my own work has enabled me to trace with more or less certainty eighteen metallic elements,[1] in addition to those recorded

[1] In fact Messrs. Ismay, Imray and Co. have just announced that they will take properly certified observers and bring them home again for the sum of 20*l*., which is rather less than 1st class single fare; so that English observers will be carried to Denver or the Rocky Mountains and back again for the sum of 34*l*.

[1] These are strontium, lead, cadmium, potassium, cerium, uranium, vanadium, palladium, molybdenum, indium, lithium, rubidium, cæsium, bismuth, tin, lanthanum, glucinum, and yttrium or erbium.

by previous observers; but of metalloids in this region I have traced none. The persistency with which metal after metal revealed itself to the exclusion of the metalloids led me to throw out the idea some time ago, that perhaps the metalloids lay as a whole above the metals, and shortly afterwards I obtained evidence which seemed to me of a very satisfactory nature as to the existence of carbon, its presence in the sun's atmosphere being rendered probable by fluted bands, and not by lines. There were two points, however, which remained to be settled before the matter could be considered to be placed beyond all doubt.

The first was to establish that the fluted bands generally present in the spectrum of the electric arc, as photographed, which bands vary very considerably in strength according to the volatility of the metal under experiment, were really bands of carbon—a point denied by Angström and Thalèn.

This point I have settled by two photographs, in which the carbon bands remain the same, though one spectrum is that of carbon in air, the other of carbon in dry chlorine.

The next point was to insure accuracy by the most positive evidence that there was absolutely no shift in the carbon bands. Such a shift is produced when the part of the arc photographed is not perfectly in the prolongation of the axis of the collimator of the spectroscope. Its effect is to throw the lines of iron, for instance, a little to the right or a little to the left of the Fraunhofer lines with which they really correspond.

I have now obtained a photograph which supplies such evidence. There are metallic lines close to the carbon bands which are prolongations of Fraunhofer's lines, while the lines which I have already mapped at W. L., 39·27 and 39·295, in the spectrum of iron, are also absolute prolongations. Therefore there is no shift in the carbon flutings, and the individual members of the fluted spectra in the brightest portion are absolute prolongations of a fine series of Fraunhofer lines in the ultra-violet.

Now how does this connect itself with observations of the upper parts of the solar atmosphere?

Angström has already shown that the true carbon *lines* which we get when a coil and jar are employed are not reversed in the spectrum of the sun, and I have already shown that the calcium spectrum in the sun is similar to the spectrum obtained when the spark, and not the arc, is employed. Accompanying the change from a high to a higher temperature, there is a change in the intensity of the lines—some thicken, others become thinner. We can only match the relative thickness of the solar calcium lines by employing a very powerful coil and jar—so powerful, indeed, that the lines, and not the flutings, of carbon would be visible in the spark given by it. It is fair then to say that if carbon were present with the calcium *in the sun's reversing layer*, we should get the lines of carbon when we get the calcium lines appearing as they do.

As we do not get this evidence, we are driven to the conclusion that the carbon vapour exists not only in a more complicated molecular condition (as is evinced by the flutings) than the metallic vapours in the sun's atmosphere, but at a lower temperature. It must, therefore, exist *above the chromosphere*, that is, in a region of lower temperature. Lower pressure, again, is indicated by the feeble reversal, so that everything points to a high level.

The question is, will this region be recognised during the coming eclipse?

Coming down lower we reach a level better known, and of which, perhaps, the interest during the eclipse will now be less, if we except the possibilities opened out to us by photography. One good photograph of the lines visible in the lower chromosphere will be of incalculable value. Attempts may be made on the cusps just before and after totality, and if only one of these succeeds we shall have the ordinary solar spectrum as a scale. If good pictures near H can be secured, enough information now exists for that region to enable us to determine the chemical origin of the bright lines photographed. These remarks apply to attempts made with spectroscopes furnished with slits in the ordinary way; there is little doubt, however, that the method utilised for the Siam eclipse in 1875, the method suggested by Prof. Young and myself for the Indian Eclipse of 1871, will also be taken advantage of; here the chromosphere itself becomes the slit. A dispersed series of spectral images of the thing itself, instead of the spectrum of a part of the image of it focussed on a slit is obtained, the position of each image in the spectrum enabling its chemical origin to be ascertained if only a comparison spectrum can be secured at the same time.

In 1875, in the expedition to Siam, the photographs of this nature were obtained by means of a prism, and the results obtained by that expedition led me to think that, possibly, this method of using the coronal atmosphere as a circular slit might be applied under very favourable conditions if the prism, or train of prisms, hitherto employed, were replaced by a reflection grating, with which the generosity of Mr. Rutherford has made many of us familiar, for the simple reason that while a prism only gives us one spectrum, a brilliant grating placed at right angles to an incident beam gives us spectra of different orders, so-called, on each side of the line, perpendicular to its surface. Of these two or three are bright enough to be utilised on each side, so that we can get six in all.

To test this notion I made the following experiment with a grating given to me by Mr. Rutherfurd. This magnificent instrument contains 17,280 lines to the inch, ruled on glass and silvered; its brilliancy is remarkable.

In front of the condenser of an electric lamp adjusted to throw a parallel beam, I placed a circular aperture, cut in cardboard, forming a ring some 2 inches in interior diameter, the breadth of the ring being about $\frac{1}{8}$ inch. This was intended to represent the chromosphere, and formed my artificial eclipse.

At some distance from the lamp I mounted a $3\frac{3}{4}$ inch Cooke telescope. Some distance short of the focus I placed the grating; the spectrum of the circular slit, illuminated by sodium vapour and carbon vapour was photographed for the first, second, and third orders on one side. The third order spectrum, showing the exquisite rings due to the carbon vapour flutings, was produced in forty-two seconds. The first order spectrum, obtained in the same period of time, was very much over-exposed. It is, therefore, I think, not expecting too much that we

should be able to take a photograph of the eclipse, in the third order, in two minutes. Similarly, we may hope for a photograph of the second order in two minutes, and it is, I think, highly probable also that a photograph of the first order may be obtained in one minute. To make assurance doubly sure, the whole of the totality may be used during the coming eclipse, but if there be several such attempts made it will certainly be worth while to try what a shorter exposure will do.

Now, by mounting photographic plates on both sides of the axis, one solidly mounted equatorial of short focal length may enable us to obtain several such photographs, with varying lengths of exposure. I insist upon the solidity of the mounting because, if any one plate is to be exposed during the whole of totality, the instrument must not be violently disturbed or shaken while the eclipse is going on. I think, however, it is quite possible to obtain more than one photograph of the lower order spectra without any such disturbance in this way. The same plate may be made to record three, or even four, exposures in the case of the first order in an eclipse of four minutes' duration, by merely raising or lowering it after a given time, by means of a rapid screw or other equivalent contrivance, so that a fresh portion of the same plate may be exposed. Similarly, the plates on which the spectra of the second order are to be recorded may be made to perform double duty.

If one equatorial thus mounted were to be devoted to each quadrant of the coronal atmosphere, it is certain, I think, that most important results would be obtained.

J. NORMAN LOCKYER

(*To be continued.*)

GIGANTIC LAND-TORTOISES

Gigantic Land-Tortoises, Living and Extinct, in the Collection of the British Museum. By A. C. L. G. Günther, M.A., M.D., F.R.S. Keeper of the Department of Zoology. (London: Printed by Order of the Trustees, 1877.)

THE recent and extinct gigantic land-tortoises in the collection of the British Museum has just received at the hands of Mr. A. C. L. G. Günther, Keeper of the Department of Zoology, an elaborate and exhaustive memoir and history. As early as 1872 Dr. Günther had made much progress in the elucidation of their structure, but in 1874 the osteology of the Mascarene tortoises had still more engaged his attention. Again in 1877 new matter arising from fresh materials imported into England from the Aldabra group of islands, Mauritius, Rodriguez, and the Albemarle and Abingdon Islands, enabled Günther to complete his memoir upon these gigantic land-tortoises, recent and fossil.

This important volume contains a description of the races of the Aldabra group, the extinct races of the Mascarene group (Mauritius and Rodriguez), and lastly, the Galapagos Islands races. Dr. Günther, at p. 10, gives a synopsis of the fossil and living gigantic land-tortoises. He bases his classification upon the presence or absence of the nuchal plate—frontal portion of the skull—condition of the pelvis as to nature of the symphysial bridge, and whether the gular plate is single or double. The Aldabra tortoises, or those of the Aldabra Islands, fall under the first group, or those with the nuchal plate present, gular plate double, and frontal portion of skull convex and with the pelvis having a narrow symphysial bridge. Four species of Testudo, all living, occur in the Aldabra group.

The second group, embracing the Mascarene and Galapagos tortoises, possess no nuchal plate; the symphysial bridge is broad, and the frontal portion of the skull is flat. The Mascarene species, four in number, are all extinct, and are found by Günther to have a single gular plate and short sternum, whereas the Galapagos tortoises have a double gular plate and rather large sternum, and all but one species (*Testudo ephippium*), from Indefatigable Island, are living.

These deductions arrived at by Dr. Günther after years of long and patient labour, greatly add to our knowledge of the structure of the Testudinæ greatly removed in space; he not only shows that the Aldabra species have definite and almost individualised structure, but that they are entirely different species from their nearest or Mascarene neighbours, a great fact in the distribution of life, over an area once continuous land, but now known to be one of depression, and yet geographically contiguous, the Island of Madagascar only separating them. Here, however, we have not a wide distribution in space, and yet no species seems common to the Mascarene and Aldabra Testudinæ—the living races of the Aldabra group being entirely different from the extinct races of the Mascarenes. Dr. Günther endeavours to show that in the absence of direct genetic relationship between the tortoises of the Galapagos Islands and the Mascarenes, that some "terrestrial tortoises" were transported through some agency ("stream or current") from the American continent to the Galapagos—and similarly that those of Madagascar or Africa migrated in a similar manner to the Mascarenes. The origin and geographical distribution of species especially terrestrial is always of the highest interest to earnest students of life in its various phases. The history and origin of species, and their distribution, is perhaps one of the most difficult problems now engaging the minds of naturalists, and Gunther refers to the reappearance of the "Indian, Mascarene, and Aldabra gigantic land-tortoises in the Galapagos," as one of these—not, he says, in "typical singularity, but with all the principal secondary modifications reproduced." The greater extension of this large Chelonian type at a former geological epoch seems manifest, when we find remains at Malta corresponding with those of the Galapagos tortoises, and the close affinity between the Galapagos and the Aldabra and Mascarene species, although separated by so vast a distance; we must grant a continuity of land over the region now covered by the Pacific, and which for ages has undergone, and is still undergoing depression. No one can doubt or fail to see the great changes that have taken place in the physical geography of South Africa, whose attenuation towards the south and eastern coasts is due to depression, thus causing the isolation of Madagascar, the Mascarene Islands, and the Seychelles, such severance and island making, through causes long-continued and not equally the same areally in equal times, has produced that specialised or peculiar fauna for which many of

these islands are noted, yet partaking largely of the Madagascar or parent types.

These gigantic land-tortoises which appear to have formerly occupied or inhabited the Mascarene Islands, are now only found at Aldabra, one of the Seychelles 1,000 miles further north. This isolation of Madagascar with its surrounding northern and eastern scattered islands and coral reefs alone indicate one continuous and extensive equatorial land. According to Wallace the Mascarene Islands were probably "earliest separated from Madagascar and before any carnivora had reached the country, hence the secure abode of groups of birds incapable of flight"; also to the same causes may be attributed in these islands the development of these gigantic land-tortoises, security, food, and time being three at least important factors for continuity of life; and surpassing as they did all others in size now living on the globe.

Dr. Günther formulates the races of tortoises indigenous to the Galapagos by the want of the nuchal plate, by the long neck and legs, and black shell, flatness of the crown of the skull, and thinness of the osseous carapace.

This diagnosis of the Galapagos races of tortoises shows them to be differentiated from the Aldabra races by the same structural characters as the Mascarene races—to which, however, they are closely allied—but differing in not possessing the double gular plate.

Dr. Günther in his elaborate notice of the extinct races of the Mascarene turtles, speaks of their being "sharply and structurally differentiated from the tortoises of the Aldabra group;" he has now ascertained through the possession of complete carapaces from Mauritius and Rodriguez, that there is an "absence of the suture which divides in most land-tortoises the gular plate of the sternum into two longitudinal halves." Again, he has proved that the Mascarene tortoises possess no nuchal plate. The solution of these structural differences of the races is due to recent researches and exploration in the Mauritius and Rodriguez, and they have resulted under Günther's determination in the three following deductions:—

1. That the specimens with a nuchal plate (and with double gulars) come from Aldabra.

2. That the specimens with simple gular (and without nuchal) come from the Mascarenes.

3. That the specimens without nuchal, and with double gular, are Galapagos tortoises.

Dr. Günther's researches conclusively show that the living gigantic tortoises of the Galapagos are more nearly allied or related to the extinct tortoises of the Mauritius, than those living in Aldabra. This generalisation of Dr. Günther's tends to show that there must have been several distinct groups and centres of Testudo ranging widely over the globe, and that some of each still survive in localities widely removed from each other; such being the Mascarene, Seychelles, and Galapagos, with remains found at Malta.

Elaborate osteological details accompany the descriptions of the species in the races of the Aldabra tortoises, the extinct races of the Mascarenes, and the Galapagos species. No less than fifty plates illustrate and accompany the letter-press to this learned memoir or monograph upon the gigantic land-tortoises (living and extinct) now in the collection of the British Museum. R. E.

OUR BOOK SHELF

Treatise on Modern Horology in Theory and Practice. By M. Claudius Saunier, Ex-Director of the School of Horology at Maçon, Chevalier of the Legion of Honour, Honorary Secretary to the Paris Society of Horologers, &c. Translated by Julien Tripplin, Besançon, Watch Manufacturer, and Edward Rigg, M.A., Assayer in the Royal Mint. (London: J. Tripplin.)

M. Claudius Saunier's treatise, though mainly intended for technical readers, contains a vast quantity of useful and instructive information, likely to be quite as interesting to amateur as to professional horologists. The work, moreover, is largely illustrated by beautiful coloured copperplate engravings, which, as models of accuracy and elegance, cannot be too highly praised. If in anything the book is perhaps scarcely up to the mark, as regards recent improvements in English clockwork; but no doubt such will be fully discussed in the appendix we understand M. Saunier has in hand, and which we hope will be published before the conclusion of the English series. So far as can be judged from the first number, the work of translation is being performed efficiently.

China. A History of the Laws, Manners, and Customs of the People. By John Henry Gray, M.A., LL.D., Archdeacon of Hongkong. Edited by W. G. Gregor. 2 vols. 140 Illustrations. (London: Macmillan and Co., 1878.)

Many books have been written on China and its puzzling people, and many attempts made to describe and account for the mode of life, the manners, and customs—to Europeans seemingly half-childish—of the latter. Hitherto, however, it is safe to say, the Chinese have not been understood. Their jealousy of foreigners, their unwillingness to admit the outside barbarian into the sanctity of their inner life is proverbial, so that the vaguest and most erroneous notions prevail concerning this remarkable people, combined with a sort of tacit conviction that their life in its various aspects is too trivial to be worth inquiring into. A perusal of Dr. Gray's work, we are sure, will greatly tend to dispel these mistaken notions. Dr. Gray evidently possesses an unusual power of winning his way into the friendship of all classes of Chinese, and this, combined with a liberal and tolerant mind and a faculty of careful observation, has enabled him to learn more about the everyday life and thoughts and motives of the people than almost any European has done before him. The work is certainly one of the most instructive that has ever been written on China, and every page is interesting. Family life in all its varied relationships is illustrated by pen and pencil, as is also official life, commercial life, professional or literary life, life in hotels, and life in the street, pawnshops, pagodas, agriculture, fortune-telling, religion, amusements; in short, it would be difficult to point out in what respect the book is defective. The illustrations are very interesting, and have mostly, we believe, been drawn by native artists. The work ought, we should think, to become a permanent standard work on China.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

The Arrangement of Museums

The subject brought forward by Lord Wharncliffe a few days ago in the House of Lords forms part of a much larger

question which sooner or later must occupy the attention it deserves, viz., the best means of collecting and arranging museums for the purposes of instruction.

How few of those who visit the British Museum or South Kensington, amongst the less instructed portion of the public at least, carry away any distinct reminiscence of what they have seen. The mind is bewildered by the immense number of objects presented to it and the absence of any sequence in the arrangement by which to assist the memory. The principles which are recognised as applicable to education in general apply equally to the arrangement of museums in so far as their educational functions are concerned, and consist, not in overwhelming the student with an immense accumulation of facts, but in presenting to his mind only such facts as are important or typical, and in the order in which it is intended they should be remembered. The order in which the knowledge of things is best received and retained by the memory is precisely that in which the things themselves were evolved; consequently, the arrangement which in a museum is best adapted to impart instruction is at the same time that which best records the history of the things exhibited. This consideration appears to determine conclusively the arrangement which ought to be adopted wherever the education of the public is the object to be attained, but museums, whether local or national, have other objects besides the instruction of the public. They are intended for the preservation of antiquities and natural history objects, many of which are not yet classified and have no place assigned to them, and which consequently cannot be arranged in any historic sequence such as I have alluded to. They should afford materials, not for the use of the public only, but for *savants*, in promoting original research, which is one of their most important functions, and which ought not to be sacrificed for the benefit of the common herd of visitors, and the question is, whether these distinct objects can be properly combined in one museum and by the same arrangement. If my own experience as a collector may be relied upon, this cannot be the case, unless an educational museum is collected from the first with a view to sequence, and unless this object is kept steadily in view whenever any addition is made to the collection, it will miss its aim. Take, for example, the case of the British Museum, which has accumulated from time to time by the accession of more or less homogeneous collections which have been purchased or presented, and which it is important to keep together. Out of any such collection it is probable that only a very few objects could be regarded as typical of any particular phase of development, say in pottery, sculpture, or glyptic art, and the remainder, although of the utmost value to the antiquary and necessary to be retained, would only serve to confuse any arrangement that might be made either for historic or educational purposes. Or take the case of a local museum in any large country town. Two distinct functions present themselves: on the one hand it is of use in preserving the antiquities or natural history specimens of the locality; on the other hand it should consist of general collections scientifically arranged and classified for the instruction of the people of the neighbourhood. We are brought by this to consider the advisability of having two distinct kinds of museums, which would bear pretty much the same relation to one another that a glossary of scientific terms would bear to a series of elementary treatises on different sciences—the one might be termed a museum of reference, the other an educational museum; the first arranged geographically, and the second having an evolutionary arrangement; the one special and the other general; the one arranged by finds and the other by subjects; the one comprising all the various objects that can be brought together from any particular district or country, and the other consisting only of such objects as may be selected as typical or as forming connecting links of development, the one composed exclusively of originals, and the other consisting in great part of casts, reproductions, and models. This provision, although I have placed it last in the list of distinctive functions, is by no means the least important in a practical point of view, because we see that by this means the two institutions ought never to be allowed to clash. Not only are reproductions and casts as useful as originals for the purposes of instruction, but models, in some cases, are infinitely preferable, because taking less room. Might it not serve to clear our ideas if we could arrive at the principle of utilising our existing institutions so as to serve the two distinct purposes above discussed, retaining the British Museum as a museum of reference, devoting South Kensington exclusively to the purposes of education and evolutionary arrangement, and separating the loan collections as a branch distinct from both. By this means we should be enabled to carry out the objects contemplated by Lord Wharncliffe's motion, not confining ourselves to statuary alone, but organising collections to illustrate the history of various other branches of art and industry, each commencing with the rude production of savage and prehistoric man, and ending with the complex contrivances of our own time. Each year the sums hitherto devoted to annual exhibitions would be applied to perfecting and re-arranging the collection, casting out some and replacing them by others more strictly representative in their character. The final result would be a museum of super-organic evolution worthy of the nation and of any labour that might be bestowed upon it. It might be thought, perhaps, that to carry out such a system representing any considerable number of arts and sciences, unbounded space would be requisite, but when it is remembered that the specimens would be rigidly confined to such as represented a distinct step of development, excluding all abnormal excrescences, it is evident that the number of objects required for any particular series would be limited.

My own collection of savage and prehistoric objects now exhibited at Bethnal Green has been collected upon this system during the last twenty years, and although the effectual carrying out of the plan has been limited by the means at my disposal, enough has been done to show that a considerable number of subjects may be represented without any extravagant demand on space. Weapons, pottery, early ship-building, personal ornament, carving and sculpture, musical instruments, early or savage drawings, clothing, early writing, objects illustrating the origin and use of fire, religious emblems, &c., are exhibited separately in cases occupying the basement on one side of the building, and several cases are devoted to the distribution and development of particular forms of ornament. The long rooms in the Exhibition buildings at South Kensington are admirably adapted for the extension of this system. Probably the best arrangement would be to devote the whole range of the side walls to objects laid out in historic sequence and to place in cases opposite each successive stage of art, objects belonging to existing peoples which correspond most nearly to the historic or prehistoric sequence on the opposite side; by this means both ancient and modern phases would be represented, and survivals where they occur could be traced to their sources.

A. Lane Fox

The Phonograph

In reference to Prof. Mayer's account and Prof. Fleeming Jenkins's letters, I may say that I had an opportunity of an hour's observation of a phonograph constructed by Mr. Stroh, 42A, Hampstead Road, on April 3, at the invitation of Prof. Graham Bell. The difference between words produced from the phonograph and those spoken into it gave me the same feeling as the difference between a worn print and an early proof of an engraving. When the words were uttered loudly and slowly and repeated rather faster, it was easy to catch the sense and meaning, but I doubt whether unknown English words would be recognised, and certainly unknown foreign words would present insuperable difficulties. I should myself find the phonograph as at present constructed quite sufficient for my own purposes of registering pronunciation, especially delicate shades of dialectal utterance. Some words, as *see*, almost disappear. Both *ee* and *oo* are difficult vowels, so that Prof. Bell at first thought that the first sounded like the second, while in reality both are altered to indistinct sounds that I do not remember to have heard in speech. The resemblance is so great, however, that *bite*, *bout* could not be distinguished, though one ends nearly with *ee* and the other nearly with *oo*, and there is no other difference in the words. The vowels *ai*, *oa*, as in *bait*, *boat*, are also poor; *aa*, *au*, as in *baa*, *haul*, are really the only good ones. Hence I feel totally unable to speak positively as to the change of vowel quality by altering the rapidity of rotation and therefore pitch. As far as I could observe the quality did change, as it does in speech. We tried pronouncing words backwards, sometimes with good success, but as might be expected, even when the effects were recognisable, they were not always true. Thus, *aabaa*, *aadaa*, passed muster, but *aajaa* failed. The instrument is, however, not delicate enough to bring out these differences. The mechanical obstacle of the tin-foil, which has to be indented, and offers too much resistance, seems to be the cause of this. Such a word as *Scots*, when sung rapidly, at the beginning of *Scots wha hae*, degenerated almost into the simple vowel, the

initial and final *s* were quite lost, and the action of the mutes, *c*, *t*, was almost *nil*.

The invention is highly interesting, the effects at present produced are sometimes startling (as in cries, coughs, laughter, music), the philosophy of the process (taking a permanent impression of a very complex compound vibration, and using it as a mould to reproduce that vibration) is exceedingly attractive, but at present the instrument—at least that one which I saw, differing in many respects from the one described by Prof. Mayer—has not risen beyond a lecture illustration or a philosophical toy. ALEXANDER J. ELLIS

April 13

Phoneidoscopic Representation of Vowels and Diphthongs

PERHAPS your correspondent, Mr. Sedley Taylor, would kindly test with his phoneidoscope the propriety of calling the English combinations *ea*, *ae*, *oe*, *ie* diphthongs, and the simple vowel *I*, as pronounced in the personal pronoun, a simple sound. Perhaps also the English *ā* (as in "*name*") may be regarded as a diphthong. In Otto's German Grammar, the German combinations *ae*, *oe*, *ue*, are classed as modified vowels. I fancy *oe* is a diphthong, though in rapid speech it becomes more like the simple sound *ue*. J. H. BLAKESLEY

Linden, Hannover, April 11

The Acoustical Properties of Soap Films.

IN connection with the interesting results recently obtained by Mr. Sedley Taylor upon the acoustic properties of soap-films, as exhibited in the simple and beautiful instrument which he has termed the Phoneidoscope, I should like to call attention to the following passage published in 1873 by Prof. E. Mach, of Prague, in his *Optisch-Akustische Versuche*:—

"Bei dieser Gelegenheit kann erwähnt werden, dass die Plateau'schen Flüssigkeitshäutchen sich vorzüglich zum Studium der Membranschwingungen eignen. Eine solche Flüssigkeitshaut vor eine tönende Pfeife gebracht zeigt meist mehrere Bäuche. Ein Lichtpunkt, der sich in der Membran spiegelt, gibt mehrere glanzende geschlossene Curven."

After some remarks on the low tones to which these films vibrate, and on their vibrations to the upper partial tones, the author passes on to another subject with the remark:—"Ich erwähne diese Experimente, weil sie vielleicht, weiter verfolgt, zur Beantwortung mancher Fragen über Membranschwingungen beitragen können."

There is no mention, however, of the rotating pairs of coloured vortices noticed by Mr. Sedley Taylor. Brewster appears to have observed similar phenomena (see *Edin. Trans.*, vol. xxiv, "On Colours of Soap Bubbles," &c.) as the result of directed currents of air upon films. I have found that the vortices are also produced when a small lightly vibrating tuning-fork, having its prongs previously wetted with soap solution, is made to touch a flat soap film produced in the ordinary manner. SILVANUS P. THOMPSON

University College, Bristol, April 5

Cumulative Temperature

THE idea of a clock with an uncompensated pendulum for temperature integration referred to by Mr. Cooke (NATURE, vol. xvii. p. 323 and p. 448) has probably occurred to many persons, and was proposed by me in 1840; I found, however, that it was not new then. Forbes says in his Report on Meteorology (Brit. Assoc. Report, 1832, p. 213):—"A mechanical mode of taking the mean of an infinite number of temperatures has been proposed by M. Grassman, by observing the change of rate caused by the influence of temperature upon the uncompensated pendulum of a clock (*Poggendorff*, 1825). The idea is a good one, but was proposed long ago by Dr. Brewster ('Edinburgh Encyclopædia,' art. Atmospheric Clock)." The chief merit in this matter will belong to the person who puts the idea into a working form which can be proved capable of giving accurate results.

April 9 B.

The Southern Drought

IN response to your question appended to my letter in the last number of NATURE, I am able to give you the time of the last great drought in the Gilbert Islands. In 1870 I visited these and several other islands in the South Pacific (an account of my cruise appeared in Dr. Petermann's *Mittheilungen* for June, 1871), and at that time there was a very general drought. I was among the Gilbert Islands during October, and found that no rain had fallen there for several months. The cocoa-nut and pandanus-trees, upon which the people almost entirely depend for food, were very much dried up, and the fruit upon them were small, both in quantity and in size. This drought continued for two years after my visit, and the famine became so severe that many of the people were starved to death. Had it not been for the fish they procured, it is doubtful whether any of them would have survived, for the cocoa-nut and pandanus-trees ceased to yield fruit, and the poor people were obliged to chew the roots of the trees.

Since that time I have heard of another season during which there was little rain, in consequence of which there was comparative scarcity, but this was not to be compared with the great drought of 1870-1872.

I regret to say no long-continued observations on the rainfall have been made in Samoa. My own time was so fully occupied with other matters, during my residence there, that I neglected this one. As the droughts there have not been great enough to cause anything like distress, the periods of their occurrence have, unfortunately, not been recorded. S. J. WHITMEE

Blackheath, April 5

Research in Libraries

BEFORE this "voice from Australia" can reach London, I hope that some steps will have been taken towards carrying out Dr. Mallet's valuable suggestion (NATURE, vol. xvi. p. 457) so far as regards the British Museum and other leading European libraries.

The benefit of the proposed arrangement would, I am sure, be felt in Australia as much as in America. Anybody living here, in the North of Queensland, who may wish to consult a scientific book must take a sea voyage of 700 or 1,100 miles at a cost of 16*l*. or 20*l*. in money and at least a fortnight or three weeks in time. It may happen that one has a busy friend in the library city who will undertake the search through good-nature, but most people would prefer to employ a competent man who would do so as a matter of business.

Should the trustees or directors of the great libraries hesitate (and they may) to accept the responsibility of recommending searchers, probably it would answer nearly as well if the searchers were to advertise references to well-known scientific or literary men. Perhaps a hint might be taken from the Register House in Edinburgh. Titles to land in Scotland require registration for their completion. A purchaser, to satisfy himself that the seller has not previously alienated or burdened the subjects, has to overhaul the books of the register. This is done for him as a rule by professional searchers, some of whom are official and some unofficial. The system has worked admirably for some centuries I believe. Any Scotch lawyer could explain its details.

The subdivision of labour suggested by Dr. Mallet would enable a much higher use to be made of the system than the mere hunting-up of references. For example, if there were a searcher in Paris—a well-read geologist—to whom I could intrust an order for "any references in French geographical works bearing on the date of the erosion of the terrace between the Queensland coast range and the Pacific," or some such information, what possibilities would open out to the dwellers in distant isles, nay, even to the comparatively privileged inhabitants of London itself? ROBERT L. JACK

Geological Survey Office, Townsville, Queensland, Jan. 14

Mimicry in Birds

WITH reference to the correspondence on this subject which has recently apeared in NATURE, may I add the following instance, which has fallen under my own observation?—

On the coast of Kent is a tract of land protected from the sea by an embankment of shingle, and known as the "Reculver Marsh." It is frequented by skylarks and ring-plovers. Almost all these larks have incorporated the well-known alarm note of the plovers into their song. With such distinctness is this double note brought in, that the first time I heard it I could with difficulty convince myself that it was not uttered by *Ægialitis hiaticula*,

In the surrounding district, where larks are equally numerous, I have never detected the peculiar note.

This power of imitating the songs of birds is well known to bird-fanciers and dealers; hence birds taken from the nest are considered worthless by those who admire the natural song. I myself had a Siskin that sang the goldfinch's song, and a nuthatch that I sent to a bird show came back with a wonderful medley of notes, of which he seemed extremely proud, the call-note of the canary and several notes of the blackbird being amongst those I could clearly recognise. J. YOUNG

Notting Hill

Harrow School Bathing-Place

WILL you kindly allow me to appeal through your columns for suggestions how to cure a nuisance which we suffer from year after year in our bathing-place here, and for which we have as yet found no remedy?

The water which is pumped into the bath from a considerable depth is beautifully clear at the beginning of the season, but as soon as the weather becomes hot and the rays of the sun attain power, countless filaments, consisting of confervæ, &c., spring up from the brick floor of the bath, and push their way rapidly to the surface, the depth of the water varying from about four feet to six feet. As the boys plunge from the side into the water and swim about the bath these long wavy stems are shivered into myriads of fragments, which collect on the surface of the water and form there a disagreeable and ugly scum, which detracts not a little from the pleasure of bathing during a great part of the summer term. We have taken some pains to discover a remedy for this, whether by chemical or other means, but as yet have been quite unsuccessful. The weed reappears in equal exuberance year after year and we are helpless. If any of your readers can contribute to the removal of this annual plague, he would confer a great benefit on the school, and any practical suggestions would be gratefully received either by G. Griffith, Esq., Harrow, or by ARTHUR G. WATSON

Harrow, April 8

London Clay Fossils

I SHOULD be glad if any of the contributors to NATURE would kindly inform me of any fossiliferous sections of the London clay at present open in the immediate neighbourhood of London. Many of those named in Whitaker's "Geology of London," such as Highgate, Hampstead Heath, &c., are closed, while others at Lewisham, &c., yield no fossils except a few fragments of wood. HERMANN H. HOFFERT

South Kensington Science Schools, April 15

Meteor

As the meteor of April 2 was seen at Ashwell, Herts, and with much the same course and splendour as observed at Leicester (but without any accompanying sound), it must have been very much further off than your Leicester correspondent imagines.

So bright a meteor, falling so early in the evening, cannot fail to have been much observed. H. GEORGE FORDHAM

Odsey Grange, Royston, Herts

The Nightingale

IN case you have received no earlier communication to a similar effect, you may possibly think it worth while to record that I heard a nightingale twice on the 14th instant, in a plantation by the side of Hanger Lane, in Ealing. It was but an abortive song, such as the first of the season is very apt to be, as if he were rather shy of the sound of his own voice. But there was enough of it to leave no possible doubt as to the identity of the performer. I may add that I have in previous years heard him in the same spot two or three days earlier than elsewhere in this neighbourhood.

I heard the wryneck ("cuckoo's mate") also several times on the same day in Gunnersbury and Hanger Lanes, having heard him once the previous afternoon (13th) in Kew Gardens.

Gunnersbury, April 16 G. J. PEARSE

FLOATING MAGNETS[1]

FOR one of my little books of the Experimental Science Series I have devised a system of experiments which illustrate the action of atomic forces, and the atomic arrangement in molecules, in so pleasing a manner that I think these experiments should be known to those interested in the study and teaching of physics.

A dozen or more of No. 5 or 6 sewing needles are magnetised with their points of the same polarity, say north. Each needle is run into a small cork, $\frac{1}{4}$ in. long and $\frac{3}{16}$ in. in diameter, which is of such size that it just floats the needle in an upright position. The eye end of the needle just comes through the top of the cork.

Float three of these vertical magnetic needles in a bowl of water, and then slowly bring down over them the N. pole of a rather large cylindrical magnet. The mutually repellent needles at once approach each other and finally arrange themselves at the vertices of an equilateral triangle, thus .·. The needles come nearer together or go further away as the magnet above them approaches them or is removed from them. Vibrations of the magnet up and down cause the needles to vibrate, the triangle formed by them alternately increasing and diminishing in size.

On lifting the magnet vertically to a distance, the needles mutually repel and end by taking up positions at the vertices of a triangle inscribed to the bowl.

Four floating needles take these two forms

Five ,, ,, ,,

Six ,, ,, ,,

Seven ,, ,, ,,

I have obtained the figures up to the combination of twenty floating needles. Some of these forms are stable; others are unstable, and are sent into the stable forms by vibration.

These experiments can be varied without end. It is certainly interesting to see the mutual effect of two or more vibrating systems, each ruled more or less by the motions of its own superposed magnet; to witness the deformations and decompositions of one molecular arrangement by the vibrations of a neighbouring group, to note the changes in form which take place when a larger magnet enters the combination, and to see the deformation of groups produced by the side action of a magnet placed near the bowl.

In the vertical lantern these exhibitions are suggestive of much thought to the student. Of course they are merely suggestions and illustrations of molecular actions and forms, for they exhibit only the results of actions in a plane; so the student should be careful how he draws conclusions from them as to the grouping and mutual actions of molecules in space.

I will here add that I use needles floating vertically and horizontally in water as delicate and mobile indicators of magnetic actions, such as the determination of the position of the poles in magnets, and the displacement of the lines of magnetic force during inductive action on plates of metal, at rest and in motion.

The vibratory motions in the lines of force in the Bell telephone have been studied from the motions of a needle (floating vertically under the pole of the magnet), caused by moving to and fro through determined distances, the

[1] A note on Experiments with Floating Magnets, by Alfred M. Mayer. Reprinted from the *American Journal of Science*.

thin iron plate in front of this magnet. These experiments are worth repeating by those who desire clearer conceptions of the manner of action of that remarkable instrument.

SUN-SPOTS AND TERRESTRIAL MAGNETISM

IN a remarkable article on "La Météorologie Cosmique," which has appeared in the *Annuaire* of the Bureau des Longitudes, for 1878, M. Faye says with reference to the influence of sunspots on the earth's magnetism, that the observations of Cassini "give 1787·25 for the date of the maximum observed then at Paris, whilst the latest observations—those of Mr. Broun, himself at Trevandrum—assign 1870·85 for the epoch of the last maximum. The interval is 83·60 years. On dividing this by 8, the number of periods in this interval, 10·45 years, are found for the duration of the period. That is to say, almost exactly the value already found by Lamont by means of his own observations at Munich. The period of the spots deduced by M. Wolf, 11·1 years, not being equal to that for the magnetic variations, these two phenomena have no relation to each other."

I desire to offer a remark on this conclusion, which seems to me too hasty. On examining the two periodic series, that for the diurnal variation of declination and that for the frequency of the solar spots, we see that there is a perfect coincidence in their phases though the length of successive periods is not constant. We may find a mean length of ten, eleven, or more years, according to the epoch from which the calculation is begun, but we shall always find the same length from both series if we commence at the same date.

It seems to me then that the true way to determine whether there is an intimate connection between the two phenomena is to compare their phases, and see whether the maxima and minima of the one coincide with those of the other. If there is identity in these respects, we must without doubt find the same mean values for the periods.

M. Faye accepts the date 1787·25 as that of a maximum for the oscillations of the declination. If we look then at the curve, Fig. 2, given by him in the article in question, we see that this corresponds exactly with a maximum of sun-spot frequency. In like manner similar coincidences are seen in the epochs deduced from the observations of Arago and others up to the present time when compared one by one with the sun-spot observations of Schwabe, Carrington, Secchi, as well as of those made at Kew. The conclusion seems to me very different from that of M. Faye. We are entitled to apply the rule he has given (p. 634): "If two series of phenomena, however different they may appear at first, follow exactly the same period, they ought to be referred to the same cause."

There is another passage upon which I desire to offer a remark: "Two kinds of meteors exercise a considerable influence on the direction of the magnetic needle, these are the auroræ boreales and the cyclones."

For the first there is a general agreement, but for the cyclones what observations have we which prove any such influence? If cyclones exercise a considerable influence on the direction of the needle, in what phase of the phenomenon does this occur? Is it on their formation, on their passage over some particular meridian, or when their centre is over a place? In the last case each cyclone will be a source of disturbance, which will be manifested as it progresses, and not simultaneously at all places, which, however, is what really occurs in the case of magnetic disturbances. Electricity is, without doubt, a cause, but only in the case of such considerable discharges as the aurora polaris; but not the local electricity which may accompany cyclones. When there is a storm, and the thunder rolls, and the electrometer shows enormous variations of atmospheric electricity, changing sign continually, the magnetic needle continues its usual and regular progress. Of this we can offer hundreds of examples.

JOAS CAPELLO

Lisbon, February 22

P.S.—The mean movements of the magnetic needle in the Lisbon Observatory, from eight A.M. to two P.M. are given in NATURE, vol. xiii. p. 448, for the years 1858 to 1875; the following are the corresponding mean ranges for the next two years:—

1876 ... ... 5·81′   1877 ... ... 5·54′

So that the mean movement was less in 1877 than in 1876.

OUR ASTRONOMICAL COLUMN

NEW COMPANION TO ALDEBARAN.—Mr. S. W. Burnham notifies his discovery with the 18½-inch Alvan Clark refractor of the Dearborn Observatory, of a minute star much nearer to Aldebaran than that which makes the double star H. VI. 66; he compares it with the ruddy bright star, as resembling, in difficulty and appearance, Mars and his outer satellite. The mean of three days' observations gives the angle 109°·0, and the distance 30″·35 for 1877·90, or if the second result which, as printed, differs nearly ten degrees from the other two, the angle will be 111°·9. The secular proper motion of Aldebaran, according to Mädler, is 19″·1 in the direction 157°; some years must elapse before the question of physical or optical duplicity can be decided. Eight days' measures of the close companion of Sirius, by Mr. Burnham, with the same instrument, assign for the angle of position 52°·4, distance 10″·83 at the epoch 1877·97.

THE STAR LALANDE 37813.—Mr. J. E. Gore writes from Ballisodare, Co. Sligo, with reference to this star, which appears in the reduced catalogue as a *second* magnitude, and which, observing in the Punjab in August, 1877, he had found a little less than Lacaille 8308 or 7 m. This is one of the errors in the catalogue which, as in a case recently noticed in this column, can only be cleared up by referring to the *Histoire Céleste*. The observation was made on August 20, 1795, and the star No. 37813 was really estimated 7·8 m. α Aquilæ was observed immediately before it, and entered 2 m.; it is this erroneous magnitude for the bright star of Aquila that has become attached to the star of which Mr. Gore writes. There is a very noticeable proper motion in N.P.D., apparently about + 0″·48 annually, as shown by comparison of the observations of Lacaille, Lalande, Jacob, and Argelander, with the position in the Washington Catalogue for 1860.

THE MINOR PLANETS.—Discoveries in this group still progress. No. 186 was detected by M. Prosper Henry at Paris, on April 6, shining as a star of 11·5 m., and No. 187 by M. Coggia at Marseilles, on April 10; it was estimated 10 m. No. 178 (Palisa, 1877, November 6) has been named *Belisana*, and No. 184 (Palisa, 1878, February 28) it is proposed to call *Deiopeia*. With already *seven* additions to the list, it would not appear that 1878 is likely to fall short of the most prolific of preceding years in these discoveries.

THE TRANSIT OF MERCURY ON MAY 6.—If we calculate strictly from Le Verrier's tables of sun and planet, using therefore the value of the sun's diameter which he deduced from the transits of Mercury in his memoir, printed as an addition to the *Connaissance des Temps* for 1848, we shall have the following formula for determining the time of the first *external* contact of limbs in the approaching transit:—

$$t = 3\text{h. } 13\text{m. } 1\text{s.} - [1\cdot8723]\, r \sin l - [1\cdot9079]\, r \cos l, \cos (L - 56^\circ\ 49'\cdot3)$$

in which t is the Greenwich mean time of contact, r the

radius of the earth at the place for which we are computing, l its geocentric latitude, and L the east longitude from Greenwich; the quantities within square brackets are logarithms.

At the Royal Observatory the first external contact is found to occur at 3h. 11m. 35s; the sun will set at 7h. 31m., about 30m. after least distance of centres, so that more than half the transit may be observed. At Edinburgh the first contact takes place at 2h. 58m. 53s. Edinburgh mean time, and the sun will set at 7h. 36m. The first *internal* contact at Greenwich and Edinburgh occurs 3m. 7s. later. The angle from North point of external contact is 45° towards East for direct image.

At Ogden, Utah, to which position it has been stated that a French Expedition is proceeding for the observation of the phenomenon, Mercury enters upon the sun's disc at 7h. 44m. A.M., and the egress takes place at 3h. 18m. P.M., the duration of the transit being 7h. 34m.

At the next transit at the descending node on May 10, 1891, the last external contact at Greenwich, according to Leverrier's tables, will occur at 4h. 50·4m. A.M., and as the sun will not rise till 4h. 19m., but little of the transit can be witnessed in this country. In the transit at the opposite node on November 10, 1894, the first contact of limbs appears to fall close upon sunset here. On November 7, 1881, as will be seen from the *Nautical Almanac*, the transit will be wholly invisible in England. It thus follows that on the afternoon of May 6 next, we shall have in these islands the only favourable opportunity of viewing the planet Mercury projected upon the sun's disc that is afforded during the present century.

GEOGRAPHICAL NOTES

AFRICA.—M. F. Deloncle, a member of the Geographical Society of Lyons, has recently translated into French a remarkably interesting itinerary of the voyages made by a Spanish friar in the middle of the fourteenth century. The work was originally written in the Catalonian dialect and devoted chiefly to travels in Africa.

The Society of Geography has prepared, for the Paris Exhibition, a map of Africa, measuring 2 m. × 2 metres, and showing the route of every explorer from 1754 to Stanley, in 1878. The number of travellers is 121, of whom not less than 42 are French; but a large number of these explored either Madagascar or the Desert round Algeria. The first name written in this list is Mayeur, a traveller now quite forgotten, who crossed the northern part of Madagascar.

NEW MEXICO.—During the season of 1877 a party of the U.S. Geographical and Geological Survey of the Territories, under the command of Lieut. C. C. Morrison, was detailed to survey the section of New Mexico lying between the 105th and 108th meridians and between the 33rd and 35th parallels, about half of which is mountainous, the rest being mesas and plains. In giving an account of their explorations at the last meeting of the Royal Geographical Society, Mr. T. W. Goad, the meteorologist of the party, mentioned some points in regard to the physical features and characteristics of the country surveyed, which attracted special notice. Between the Sierra Blanca and the Oscura Mountains a lava flow was met with of over seventy-five miles in length, with an average breadth of three miles. This Mal Païs, as the Mexicans call it, resembles a black river, widening and narrowing as the country undulates. This stretch of lava, owing to denudation, is somewhat higher than the surrounding country, and is full of caverns. Several of these were visited by the survey party, but the only one of importance was near Fort Stanton, which, like the others, was in a limestone formation, and proved of considerable length; some persons, indeed, asserted that no one had been to the end, though a distance of five miles was measured. The exploration of this cave was of a most uncomfortable nature, necessitating long crawls through narrow passages, and obliging the explorers to wade up to their waists in ice-cold water for hours. Stalactites and stalagmites of immense size were met with. The lake in the cave was said to contain eyeless fish, but none of the party were able to catch or see any. The cañon, again, of the Rio Grande, below Castilla, is of peculiar interest, because it differs in most respects from other cañons, and instead of being worn away by the action of the water alone, it was probably commenced by volcanic action. The sides are of trap-rock, and although the cañon itself is very narrow, its depth was estimated at 1,000 feet. The river at this point has a great fall and rushes along with a velocity of ten miles an hour. Mr. Goad describes the climate of New Mexico as delightful.

GEOGRAPHICAL ANNUAL.—The new volume of *L'Année Géographique*, for 1876, has at length appeared. The delay has been caused by the resignation of the editorship by M. Vivien de St. Martin, whose time is now so fully occupied with other work. The new editors are MM. Maunoir and Duveyrier, and the new volume is quite up to its predecessors. The volume for 1877 will be published about June.

METEOROLOGICAL NOTES

METEOROLOGY OF STONYHURST.—The results of the meteorological and magnetical observations at Stonyhurst for 1877 are already published. In addition to the very full statement of the results for the year, and which are compared with the averages of previous years brought down to date, there are given observations of crops, flowers, trees, and shrubs; observations of the cirrus clouds made at the observatory in connection with Prof. Hildebrandsson's large inquiry into the upper movements of the atmosphere; and a discussion of the hours of occurrence of the barometric maxima and minima during the eight years ending 1875. This discussion has been evidently conducted with great care and with full knowledge of the subject in hand. The results arrived at are of great importance, the chief points being that there is a tendency of the maxima to occur between 10 and 11 A.M. and P.M., the total number from midnight to noon being, however, considerably in excess of that from noon to midnight; and that the minima occur with nearly the same regularity as the maxima, but at different hours, viz., about 3 and 4 A.M. and P.M. The importance of these results lies chiefly in the circumstance that they accord with the hours of the critical phases of the diurnal fluctuations of the barometer, and peculiarly so as regards the annual results. We are much pleased to see from the report that Father Perry is engaged with the discussion of the meteorological observations made at Kerguelen during the Transit of Venus Expedition, to the results of which meteorologists will eagerly look forward.

WEEKLY STATISTICS OF THE WEATHER.—The Meteorological Office has begun to issue weekly statistics of the weather of the British Islands for agricultural and sanitary purposes. For this object the country is divided into two divisions, the one being suited for the production of wheat, and the other for the rearing of stock. For each of the ten regions into which these two divisions are sub-divided there are published the highest, the lowest, and the mean temperature of the week, and the degree to which the last is above or below the average of the week, together with the number of days of rainfall, its amount, and the difference between the latter and the average rainfall of the week. To these follow general remarks on the weather as regards frost, winds, storms, and any irregularity that may have occurred in the rainfall at the selected stations. This step is in the right direction, and the scheme will no doubt soon receive greater extension and further development in order that

it may the fuller meet the requirements of the classes for which it is intended. It is desirable, for instance, if not indeed essential, that the mean temperature be given to tenths of a degree and not merely to whole degrees, particularly when it is kept in view that no inconsiderable portion of Great Britain is but little removed from the limits of the successful cultivation of the wheat, and the rainfall to hundredths of an inch, so as to mark off clearly the practically rainless districts during each week. The number of stations situated on the coast preponderates too largely. Additional stations from several of the great agricultural centres are needed, and a partition of the country into more districts than ten, it being evident that a division of Scotland merely into east and west, and of Ireland into north and south, is inadequate. Scotland, for instance, should be divided at least into north-east, north-west, south-east, and south-west divisions, these differing essentially from each other in their climatic and agricultural peculiarities.

MISSOURI WEATHER REPORTS, NOS. 1, 2, AND 3.—The system of weather service for the State of Missouri is being satisfactorily and energetically developed by Prof. Francis E. Nipher, Washington University, St. Louis. The second report, being for January last, is accompanied with a table showing the rainfall at thirty-eight stations in Missouri and a map on which the amounts are entered and isohyetal lines drawn showing where the fall was nothing, one inch, two inches, and three inches respectively. From this map the distribution of the rainfall, a correct knowledge of which is so important to farmers and others, is seen at a glance. The distribution of the heavy snowfall of the 30th and 31st is particularly detailed, and we are pleased to see the frankness with which Prof. Nipher informs his observers that it has been impossible to give a proper account of the remarkable storm of the 26th, which entered the northern part of the state at 8 A.M. and soon thereafter developed into a severe thunderstorm in central and southern Missouri, owing to the times of the beginning and ending of the storm not being given carefully for a sufficient number of places. We feel assured that the observers will gladly see to the rectification of this and supply the information desiderated in future. From the first report we see that the mean temperature of December was 12°·4 above the average of the month; and with this high temperature, the mean of the month being 45°·6, vegetable and animal life was prematurely urged forward at an undesirably rapid rate. A valuable table accompanies this number, which has been prepared by Dr. Engelmann, giving the mean monthly temperatures and extremes and the mean rainfall at St. Louis for forty-two years, from which it appears that the mean of the coldest month, January, is 31°·7; the warmest month, July, 79°·2, and of the year, 55°·4. The highest temperature noted during these forty-two years was 104°·0 in July, 1860, and the lowest—23°·0 (below zero) in January, 1873. The mean annual rainfall is 42·46 inches, the largest monthly fall being 5·39 inches in June, and the least, 2·13 inches in January.

EXTRAORDINARY RAIN-STORM IN CANADA.—A continuous storm of rain, extending over two or three days, and covering a considerable portion of North America, occurred in the end of February, the weather for some time before having been unusually mild. Near the coast rain prevailed, in the Quebec district much snow fell, about Ottawa, sleet, hail, rain, and snow fell in succession, and on advancing westwards through Canada, and into the United States, the precipitation appears to have been heavier. In Central Canada the floods seem to have been most destructive, and immense damage has been done to the towns built on the rivers, by the loosening of the ice by the floods, which, floating down the swollen rivers, carried bridges and other structures before it. Much damage was also done by the ice running aground at various points, and thereby damming up the rivers, by which extensive stretches of low-lying grounds were submerged.

COMPARATIVE ATMOSPHERIC PRESSURE OF NEW ZEALAND AND GREAT BRITAIN.—Mr. C. Rous Marten, whose name has been so long and so favourably associated with the meteorology of New Zealand, has published a short paper on this subject in the *Transactions* of the Wellington Philosophical Society. The mean pressure of the atmosphere of Great Britain calculated from fourteen stations distributed from the Channel to the Moray Frith, is 29·848 inches; and of New Zealand, as similarly determined from fourteen stations from Southland to Mongonui, 29·918 inches. The interest of the comparison lies in this, that pressure diminishes in both countries at a somewhat rapid rate on proceeding into higher latitudes, and that though the New Zealand stations lie on the average in about 12° lower latitudes than British stations, yet the pressure does not greatly differ in the two countries. The strong resemblances between the climatologies of the two countries result from the peculiar distribution of pressure common to both and the lie of their mountain ranges, by which the prevailing winds are westerly, and being laden with the vapour of the ocean they have traversed, are productive of rainy climates in the west, and dry climates in the east.

NOTES

INVITATIONS have recently been issued by the Rector of the University of Pavia to the various scientific societies of Europe, to participate in the ceremonies connected with the unveiling of the statue of Volta on April 28.

THE Electro-metallurgical Company of Brussels has lately completed a colossal statue of Jan van Eyck, in bronze, by the system of electric deposition. The galvanic process occupied several months, although a thickness of but six to eight millimetres was attained. It is probably the largest object which has been produced by this method, being over twelve feet in height, and is regarded as a much more perfect imitation of the model than could be obtained by casting.

THE meeting of the delegates of the French Sociétés Savantes will take place as usual at the Sorbonne, in the first week after Easter. M. Bardoux will preside over the meeting for the distribution of prizes, and deliver an address summarising all the measures contemplated by the Government for promoting popular instruction.

M. BARDOUX has given the decoration of the Legion of Honour to the oldest schoolmaster of France, who has been teaching since 1818 in the very parish where he was born. The ceremony took place at Clermont-Ferrand at a dinner given by the Prefect in honour of the Minister. The whole scene is said to have been very impressive.

M. ASSELINE, a journalist and a member of the Municipal Council of Paris, died suddenly a week ago. He was one of the Society of Mutual Autopsy recently established in Paris for investigating by *post mortem* examination all the circumstances of death, and his case was the first instance of the application of the rules of the Society. The autopsy was made by Dr. Broca, the president of the Society, and the results published in the papers.

THE Municipal Council of Paris has appointed a Commission of ten members in order to take part in the proceedings of the French Association for the Advancement of Science, which will take place at Paris, as we have already reported. They will sit in their official capacity.

A SOCIÉTÉ DE MINERALOGIE has been formed in Paris, with M. Des Cloiseaux as president. It meets on the second Tuesday of each month in the mineralogical laboratory of the Sorbonne.

A *Cours Annexe* has been created at the Sorbonne for physical astronomy. M. Wolff will lecture on the observational methods of physical astronomy, and the constitution of celestial bodies.

M. Ossian Bonnet, Director of Studies to the Polytechnic School, has been appointed successor to M. Leverrier in his capacity of Lecturer on Mathematical Astronomy.

The Italian Cryptogamic Society, founded in 1858 by De Notaris, has just been reconstituted under the presidency of Prof. F. Ardissone, of Milan. It consists of two classes of members: ordinary (*effetivi*) and foreign. The former consist entirely of Italian, the latter of foreign cryptogamic botanists, the foreign members being elected by the vote of the ordinary members. The Society will publish annual volumes of its "atti," and, in addition, one or more fasciculi every year, each containing fifty new or interesting species of cryptogams, at a cost of 10 lire (Italian) the fasciculus. The British corresponding members at present are the Rev. M. J. Berkeley, Dr. R. Braithwaite, Dr. M. C. Cooke, Mr. Jas. Stirton, and Mr. John Smith. Cryptogamists who are not members of the Society are invited to contribute descriptions or specimens of new species, for which they will receive in exchange the volume or fasciculus containing their contributions.

The Birmingham Natural History and Microscopical Society, one of the most active of our provincial societies, have resolved to spend about 100*l*. in improving and adding to their apparatus.

The Faculty of Medicine at Lyons has taken the initiative in a subscription for the erection of a monument to the late Claude Bernard on the Quai de la Vitriolerie.

Dr Puluj, of Vienna, exhibited at a recent session of the Imperial Academy, an ingenious arrangement for signalling by means of the telephone. The vibrating membranes in two connected telephones are replaced by a pair of tuning-forks giving the same number of vibrations per second. A bell is placed close to each fork and a brass ball is suspended from a thread between the two, but in contact with the fork. If one of the forks be put in vibration by means of a hammer the movement is communicated to the other, which causes a loud ringing on the bell by means of the ball. A response can be sent back in the same manner, and after replacing the vibrating membranes, the usual method of communication begins.

A French inventor, M. Brégnet, has recently completed a so-called mercury telephone, which is quite a variation on the systems already in use. It is composed of two instruments for transmission and reception, connected by means of wires. Each of these consists of a glass vessel, containing acidulated water and mercury, into which is inserted a capillary tube filled with mercury. One wire connects the mercury in the tubes, and the other that in the vessels. When a person speaks before the transmitter, the vibrations of the air are communicated to the mercury, and cause variations in the electromotive force, which are transmitted to the receiver, and there give rise to vibrations of the air appreciable by the ear. A later simplification of the apparatus consists in using a tube with alternate drops of mercury and acidulated water, forming thus a series of electro-capillary elements.

We are glad to know that one of the signs of our times is a more appreciative and intelligent interest in the things lying around us, including the beauties of nature as well as those affairs of a more human interest. Messrs. Marcus Ward and Co., in their new monthly publication, entitled, *Our Native Land*, a copy of which we have just received, certainly deserve well of those who think that the habit of observation can be fostered and developed by calling attention to the many things of beauty and interest in our own country. The work is to consist of reproductions of water-colour sketches, with descriptive notes, and the publication breaks ground by giving coloured plates and text illustrating "Derwentwater," "Ambleside," and "Rydal Falls." The publication is as excellent in execution as it is admirable in idea, and the reproduction of the water-colour of Ambleside is admirable; it is one of the finest specimens of chromolithography that we have ever seen. Its truth to the colour of nature and the softness of the atmospheric effects, leaves little, if anything, to be desired.

Macmillan and Co. are preparing for publication a "Journal of a Tour in Morocco in 1871, including a Visit to the Great Atlas," by Sir J. D. Hooker, P.R.S., &c., and John Ball, F.R.S., with a Sketch of the Geology of Morocco, by George Maw, F.G.S. The work will be illustrated by Mr. Whymper.

Gen. de Nansouty, the director of the Pic du Midi Observatory, has been appointed Officer of Public Instruction as a reward for his efforts and successes. He had already been made, eighteen months ago, Officer of the Academy. The General Council of Vaucluse framed, at its last session, a resolution for establishing a meteorological observatory on the top of Mount Ventoux, a mountain about 2,000 metres high, situated in the most admirable position for an extensive view of an immense region.

The intellectual abilities of the Japanese race have been evidenced in a striking manner by a quartette of students from that country now studying in Berlin. One of these, Dr. Dirokitao, has lately invented an ingenious optical instrument termed the leucoscope, which measures the variations in the perception of light and colour by the human eye, in accordance with the strictest mathematical laws. Another, who has attained the rank of lieutenant in the Prussian army, has introduced a remarkable simplification into the mechanism of the Mauser rifle, which has succeeded the historic needle-gun. Two more who are prosecuting their chemical studies under Prof. Hofmann, have published for two years past several interesting synthetical researches on the aromatic series.

The canvas for the great Paris captive balloon is quite ready; it forms 46 rolls, weighing 60 kilogs. each, having a length of 80 metres, and a breadth of 113 centimetres. It was submitted to a traction of 1,000 kilogs., under which it has extended 25 millimetres per metre. After some time the increase in length was reduced to 12½ millimetres. The net is almost finished. It is composed of 256 ropes 11 millimetres each in diameter, and bearing a strain of 1 ton.

In a note in the *Bulletin* of the French Scientific Association, Col. Gazan gives some interesting observations on the fracture of iron. During his sojourn in the arm manufactories of St. Etienne and Tulle, at the central depôt of artillery, and at the manufactory of Châtellerault, he was able to make important researches on iron. The fracture of iron may be nervous, in grains more or less fine, or in facets sometimes having a surface of several square millimetres; often it presents a mixture of these three features. Thus it is impossible to judge of the quality of an iron before breaking it; and it is on this account that in arm manufactories they break a certain number of bars with which they make a certain number of pieces for which they are intended, and which are afterward broken to ascertain their resistance, that is, the goodness of the iron, which, moreover, is still rendered brittle in presence of phosphorus, arsenic, or sulphur. The best irons are the nervous, then those of fine grain and with facets. On railways it has been proved that rails placed in the direction of the magnetic meridian are affected quite differently from rails placed at right angles to this direc-

tion; the former oxidise and do not become brittle, the latter do not oxidise, but do become brittle. In intermediate directions the rails participate more or less in the qualities of those which are placed in the two extreme directions. What becomes of the iron which is now so plentifully used in the construction of building—girders among others? Is not this a subject for serious research?

The French Minister for Public Works has accepted the plans of an underground railway in Paris, which was worked out by order of the Prefect of the Seine. According to these it is intended to build the central station seven metres underneath the gardens of the Palais Royal. Three different lines will radiate from that spot, viz.: (1) to the Exchange, the Opera, the railway station of St. Lazare, then to Batignolles, communicating with the Great Western Railway and the Chemin de Fer de Ceinture; (2) to Les Halles, the rue Turbigo, the Boulevard Sebastopol, the Boulevard de Strasbourg, the Great Eastern and Great Northern Railways; from the Boulevard de Strasbourg a branch line would lead to the Vincennes and Lyons Railway Stations, passing underneath the Seine to the left bank of the river; (3) to the rue de Rennes, the Montparnasse Railway Station, the station for Sceaux, and to Gentilly. The cost of the lines is estimated at 6,000,000*l.*, and is to be borne jointly by the State, the Departement de la Seine, and the City of Paris.

Writing in *La Nature*, M. Hélène calls attention to the excellent example set by Switzerland in regard to popular meteorology. There is hardly a town but has in one of its squares, perhaps at the side of a lake, an elegant column with instruments required for observation of the usual phenomena. Thus in Fribourg, is a black marble column (about 2·65 m. high) on a granite platform. On the north face is an alcohol thermometer, with double graduation cut in the marble; on the west a mercury barometer; on the east a hair hygrometer. The south face has an inscription giving the longitude, latitude, altitude, barometric and thermometric means, and annual rainfall. On a globe crowning the column are lines giving the direction of the four cardinal points. An inscription near the base tells that the column was erected by the Fribourg Society of Natural Sciences. The monument cost not more than 1,500 to 2,000 francs. Such columns often give various other kinds of information, *e.g.* the hour in different cities of the globe when it is midday at Berne, the heights of neighbouring mountains, measures, variations of lake level, records of severe winters, &c., in short the chief points which a natural curiosity would seek knowledge of. They are generally erected by cantonal societies.

We have received the first two parts of the tenth edition of Cooley's "Cyclopædia of Practical Receipts," revised and partly rewritten by Prof. R. V. Tuson, F.C.S. To what extent the work has been brought up to date may be learned by looking at the articles on Spectrum Analysis and Anemometers; in the latter case the anemometers now in use are dismissed in a foot-note.

Interesting antiquities, coins, vases, &c., have recently been found at Strassburg in some excavations which are being made in connection with water-works. In some parts a number of skeletons of animals have been discovered, amongst others a well-preserved jaw with tusks of a prehistoric boar, and some deer horns, &c.

At Cologne a meeting of the International Society against the pollution of rivers, the soil, and air, took place a few weeks ago. Its reports are published by Herr Hugo Voigt, at Leipzig.

The International Congress for the investigation of the history of America before Columbus, will meet at Brussels during 1879. Originally it was intended to hold the meeting at some American city.

The United States of North America possessed only forty-nine public libraries in the year 1800. The number has now risen to no less than 3,682, and the number of volumes contained in them exceeds thirteen millions.

Two new institutions are about to be established in Germany; one at Bielefeld, for textile industries, and another at Iserlohn, for metal industry.

On March 11 the Ural Mountains were first crossed by a railway train upon the occasion of the opening of the new line from Perm to Jekaterinburg.

On the 14th inst. the Institution for the Deaf and Dumb at Leipzig celebrated the 100th anniversary of its foundation. It is the oldest institution of the kind in Germany.

A new eruption is reported to have occurred in Iceland on March 24, in the vicinity of Mount Hecla.

During the past three years Admiral Duperré has met with considerable success in his efforts to develop the resources of the French colony in Cochin China. He first started an experimental farm just outside Saigon, where sugar-cane, cotton, indigo, coffee shrubs, &c., were planted under the superintendence of a botanist from Paris. From this farm thousands of coffee plants, &c., are distributed every year all over the colony among the French and native planters. Tobacco has also been successfully cultivated, and attempts are about to be made to prepare the leaf for the European market, and an official from the tobacco manufactory at Paris has been appointed to superintend this work. The sugar-cane is found to flourish well in Cochin China, and experiments are being made with a view to discovering the best means of turning it to profitable account.

At p. 16, vol. xvi. of Nature, is an account of a new stimulant known as pitury, which it was shown had been proved by Baron von Mueller to be derived from *Duboisia hopwoodi*, a plant described by himself in 1861. It was mentioned in the paper above alluded to that the better known species of *Duboisia*, namely, *D. myoporoides* of Robert Brown might possibly prove to be of some medicinal value. This prophecy has since been borne out, for in a paper read by Dr. J. Bancroft on Duboisia and Pituri before the Queensland Philosophical Society at Brisbane, a good deal of information is given on both these new medicinal products. With regard to *D. myoporoides*, which is a small tree or shrub, we are told that it is found in various localities from the neighbourhood of Sydney to that of Cape York, and that it has also been found in New Caledonia and New Guinea. It grows plentifully on the borders of the vine scrubs about Brisbane and springs up abundantly after the clearance of forest land. The valuable part of the plant seems to be the leaves, from which an extract was, in the first place, made, and its effects tried upon some cats and dogs, which, during the time they were under its influence, were as helpless as if they were totally blind, falling down when the slightest obstacle came in their way. A trial of its effect was afterwards made on the human eye in several cases, and its action in dilating the pupil was found to be very powerful and rapid. The active principle seems to be almost identical with atropine, both as regards its action and its strength, and it is used in Sydney and Brisbane in place of that alkaloid. A good deal of attention has been given in this country to the new agent by Dr. Ringer and Mr. Tweedy. The former says that it has the power of drying the mouth or preventing the flow of saliva, and that it also produces headache and drowsiness, while the latter considers it quicker and more energetic in its action than atropine, and considerably more so than the strongest extract of belladonna. In every case in which it had been used by him he found its action entirely satisfactory. This subject, which is one of importance, inasmuch as it promises to open up a new

source of supply of a substance fully as efficacious as, or perhaps more so than, atropine or belladonna, has for some time past attracted much attention in the colony where the plant grows, and has quite recently been brought to the notice of the Pharmaceutical Society. It is perhaps worth noting, that one of the colonial names of *Duboisia myoporoides* is the cork wood tree, so named from its light brown corky bark. The wood is of a light yellow colour, even grained, but soft, and used in the colony for carving. Specimens of the wood are contained in the Kew Museum.

THE additions to the Zoological Society's Gardens during the past week include an Arabian Baboon (*Cynocephalus hamadryas*) from Arabia, presented by Dr. A. P. Woodforde; two Chacma Baboons (*Cynocephalus porcarius*) from South Africa, presented by Capt. W. L. Coke; a Green Monkey (*Cercopithecus callitrichus*) from West Africa, presented by Mr. Milward; a Great Kangaroo (*Macropus giganteus*), a Laughing Kingfisher (*Dacelo gigantea*) from Australia, presented by Lieut. Crawford Caffin, R.N.; a Short-eared Owl (*Otus brachyotus*), European, presented by Mr. W. K. Stanley; a Golden-winged Parrakeet (*Brotogerys chrysoptera*) from the Amazons, received in exchange; a South American Rat Snake (*Spilotes variabilis*) from South America, deposited; a Yellow-footed Rock Kangaroo (*Petrogale xanthopus*), born in the Gardens.

THE DETERIORATION OF OIL PAINTINGS[1]

OIL paintings are subject to various kinds of changes, which may be considered as diseases, requiring different treatment according to their different nature. A science needs to be formed, a pathology and therapeutics of oil paintings. The pathology would have to describe and explain those diseases and their progress, and to develop the methods by which a correct diagnosis could be arrived at in each individual case. The therapeutics would teach the remedies which might be applied either to cure or to alleviate the disease, or at least to stop its progress. A hygiene would follow, which would have to teach how to avoid pernicious influences, and which, besides, while giving precepts for the technical process of painting, would have to forestall those constitutional diseases which, even in cases where no noxious influences can be traced, are the causes of decay, after a comparatively short period of existence. As medical science is above all things based on anatomy and physiology, so the exact knowledge of the structure of a picture would have to be acquired previously to any study of its disease. Unfortunately, direct investigation alone can procure no such exact knowledge; on the contrary, we are obliged to enter upon a minute historical investigation of the material as well as of the technical methods adopted by artists of different schools and different periods.

The excellent works of Cennino Cennini, Mérimée, Sir Charles Eastlake, Mrs. Merrifield, and others, have already furnished most valuable material; but still the field for investigation remains unlimited; for, in order to enable us to secure the conservation of each valuable painting, we ought to know exactly how it was made. The artists of the present time would spare infinite trouble to the investigators of future times, if, along with their works, they would leave the account of their practice in the case of each picture. A treatment without exact knowledge of the normal condition, as well as of the nature of the disease, is, as we shall see, as dangerous for the picture as it would be in the case of living beings.

Professional restorers of pictures admit this danger in a general way; each of them, however, is convinced that he himself, by his personal knowledge, skill, and care, knows how to avoid it. The public pays too little attention to the subject, and therefore it occurred to me that it might be useful to give a short account of what we know about this question, of the changes to which oil paintings are exposed, as well as of the means either to avoid or to cure them.

We have to consider, first, the material on which the artist has painted, that is, as far as oil painting is concerned, principally wood and canvas.

[1] Paper read at the Royal Institution, Friday, March 1, by R. Liebreich M.D., M.R.C.S., M.R.I.

Secondly, the priming, that is, the substance with which the surface was prepared in order to be made fit for painting.

Thirdly, the painting itself, that is, the pigments and vehicles used for it, and the liquids that were added during the painting, the mediums, meguilp, siccatire, varnish, essential oils, &c.

Fourthly, the coat or coats of varnish spread over the picture.

The wood on which a picture has been painted may either warp, or get chinks in it, or become worm-eaten, or even altogether rotten. Against warping, the remedy usually applied is moisture. If the panel is very thick, it is first made somewhat thinner; then the back is moistened, and the picture is left to lie on its back for twelve to twenty-four hours, after which time it will be found to have bent straight. Of course this must not be continued longer than necessary, otherwise the convex surface, instead of becoming plane, would become concave. When straight, the picture is kept so by beads which have to be adapted in a particular way, a certain degree of shifting being allowed for the expansion and contraction of the wood.

Cracks in the wood are drawn together by inserting pieces of wood of a special shape.

Sublimate solutions are employed to destroy worms.

Trifling losses of substance are replaced by cement. Small portions of rotten wood, not extending too near the painting, are cut out and replaced by wedge-shaped pieces. If, however, the greater part, or the whole substance of the panel, is rotten, the picture must be separated from it and transferred to new wood, or rather to canvas.

This was first tried by Hacquin in Paris, and was performed successfully upon many pictures, and, among others, upon one of Raphael's Madonnas, in the Gallery du Louvre, and upon Sebastian del Piombo's "Resurrection of Lazarus," now in the National Gallery. The process no longer appears so very marvellous; it is generally executed in the following way:—

First of all, the surface of the picture is pasted over with gauze and paper. After that the wood is made straight by moistening, or, if necessary, by making incisions with the saw, into which cuneiform pieces of wood are driven. By means of a tenon-saw the panel is to be sawn into little squares, which must be removed by a chisel, and in this way the thickness of the wood is reduced to half an inch; it is then planed until it becomes no thicker than paper, and the rest is removed by means of a knife and with the fingers. The painting being thus severed from its basis, it can be fixed on canvas, if the priming is sufficiently preserved. In the opposite case, a mixture made of chalk and glue, or something of the kind, must be put on first, and very evenly smoothed, after being dry. This done, the new canvas has to be fixed upon it by means of a mixture of glue, varnish, and turpentine, and the substance of the picture pressed tightly and evenly against it by means of warm irons.

In order to avoid deterioration, the most minute precepts have been given for preparing the panel. It has to be taken from the best oak, or nut-trees, or cedars. The wood is to be cut into boards during winter-time, and kept till autumn before being dried; it can then be prepared only in the following spring, &c. It would certainly be preferable to give up wood panels altogether for large pictures, and only to think of means to make the canvas stronger. For small pictures, panels offer certain advantages, and can be more easily preserved from decay.

In the canvas we meet with the results of injuries or spontaneous decay. A rent may be mended by rags of linen stuck at the back of the picture. Even a hole may be filled up by pieces taken from other decayed paintings. If the picture is considerably damaged, it will be best to line it. But if the whole canvas is rotten and tattered, it will be preferable to sacrifice it by pulling off the threads one by one, after having secured the painting itself by pasting paper on the front of it. This done, the painting is transferred to another canvas in the same way as those removed from wood.

There are different modes of priming, which may be brought under two principal heads: the distemper and the oil priming.

1. The canvas is distempered by a mixture of chalk or plaster and paste, or glue, which may be laid on raw, unbleached canvas, or this latter may be beforehand prepared with glue or paste. Several coats of this mixture must be put on in succession, one being perfectly dry before the next can be applied. Many of the older oil paintings are painted on such ground. It has the advantage of being quicker prepared, of absorbing the excess of oil, of permitting the colour to enter into the

priming, and to dry quicker, and moreover, of containing a white absolutely innocuous to the others.

The inconveniences, on the other hand, are: that it more easily breaks, and under the influence of humidity separates from the canvas.

2. The oil priming consists of several coats of oil colours. As each of these must be perfectly dry before the next is laid on, and as, moreover, time must be given to the whole to dry completely before painting upon, in order to avoid the sinking in of the colours, the whole preparation is much slower than the distemper. Nevertheless it is now generally adopted.

Rey, in France, has pointed out a process which is a compromise between the two methods; he begins by distempering, and after several coats of distemper, having dried one after the other, he puts a coat of oil which, as it were, changes the distempered ground into an oil-colour ground.

With oil priming it is of importance that the principal colour be white-lead, to which are added comparatively small quantities of yellow, black, or other colours. For a whole century a school, that of Bologna, predominated in Italy, which abandoned this principle. During the second half of the seventeenth and the first half of the eighteenth century, most of the Italian masters of other schools followed its example. Probably for the purpose of obtaining more easily the desired effect of the chiaroscuro, they painted on a brownish-red priming, which consisted of bolus mixed with umber. Not one of those pictures has kept its original colouring. Not only has the priming caused all the dark parts to grow much darker, but it has destroyed, or nearly so, all the glazing, so that only those colours can be recognised which either contain white, or are glazed on white. I can show you numerous instances of this, for, on account of the extreme fertility of this school, there is little difficulty in procuring pictures of masters of that time or of their pupils.

Wood priming does not require the same elasticity as that of the canvas, which ought to be capable of being rolled. Therefore the priming of the wood shows less variations. It is generally composed of chalk or plaster, tempered with starch, paste, size, or glue, and more or less thickly laid on. In some pictures of different centuries we find, either between the wood and the priming, or between the priming and the painting, canvas, and, exceptionally, even paper.

The diseases of the priming are not of a very complicated nature. They manifest themselves principally in three different ways:—1. By cracks in the priming itself. 2. By the severance of the priming from the painting. 3. By the severance of the priming from the wood or the canvas. The third disease is by far the most frequent, especially among pictures on canvas distempered with paste. If small pieces only are scaling off or blistering, they are fixed again to the ground by letting a solution of size pass between the detached part and the canvas, and pressing both gently together. If the deterioration extends over a considerable surface, the picture has to be lined. While this is being done, and while the gluing substance penetrates into the picture, the detached parts are pressed on again with slightly heated irons. If the whole priming threatens to come off, it will be better to take the picture entirely from the panel or canvas, and to transfer it to a new canvas.

I shall show you examples illustrating the before-mentioned points, and among them two pictures; one in oil, taken off from canvas, the other in tempera, taken off from wood. Both of them, strange to say, have escaped destruction without having been transferred to a new canvas, and without being covered with paper, as is usually done, before taking them off. They show you the painting by itself from both sides. I have, of course, used every precaution in bringing them safely over from Florence, where I happened to discover them carelessly stowed away among heaps of old pictures.

We come now to the most important part of the picture, the painting itself. We meet very often with the idea that the old masters had been in possession of colours, that is pigments, the knowledge of which has been lost, and that this accounts principally for the difference between the oil painting of the fifteenth and sixteenth centuries, on the one hand, and that of the eighteenth and nineteenth on the other. But this is a great mistake. We know perfectly well the pigments used by the old masters; we possess the same, and a considerable number of new ones, good as well as bad, in addition. In using the expression of good and bad I am principally thinking of their durability. From this point of view the pigments can be placed under three headings:—

1. Those which are durable in themselves, and also agree well with the other pigments with which they have to be mixed.

2. Such as when sufficiently isolated remain unaltered; but when in contact with certain other pigments change colour, or alter the others, or produce a reciprocal modification.

3. Those which are so little durable that, even when isolated from other pigments, the mere contact of the vehicle, the air, or the light, makes them in time fade, darken, or disappear altogether.

The old masters used, without reserve, only those belonging to the first of these categories. For those belonging to the second they imposed on themselves certain limits and precautions. Those belonging to the third they did not use at all.

That some of the modern masters have not followed these principles is not owing to a lost secret, but to the fact that they disregarded those well-known principles, and even consciously acted against them. In Sir Joshua Reynolds's diary, for instance, we read that in order to produce certain tints of flesh, he mixed orpiment, carmine-lake, and blue-black altogether. Now orpiment is one of the colours of the second category, carmine-lake one of the third. That is to say: orpiment, as long as it remains isolated, keeps its brilliant yellow or reddish-orange colour; but when mixed with white-lead it decomposes, because it consists of sulphur and arsenic, and it, moreover, blackens the white-lead, because the sulphur combines with it. Carmine-lake, even if left isolated, does not stand as an oil colour, and therefore has been superseded by madder-lake.

Unfortunately some of the most brilliant colours are perishable to such a degree that they ought never to be used; yet, it seems to me, that just in one branch of art in which of late remarkable progress has been made, I mean landscape painting, the artists, in order to obtain certain effects of colour not easily to be realised, do not always resist the temptation to make use of a number of pigments, the non-durability of which is proved beyond doubt. However that may be, I think it pretty certain that the pigments in themselves play only a subordinate part in the deterioration of oil paintings, and that the principal part belongs to the vehicle with which the colours are ground, and to the liquids which are added during the painting. I hope, therefore, you will excuse my making some elementary explanations about these liquids.

Oil and fat are bodies consisting of carbon, hydrogen, and oxygen. They may be considered as salts in which glycerine, as a basis, is combined with different acids, stearic acid, palmic acid, oleic acid. If oil is exposed to the air it changes; certain kinds of oil remain liquid; others become thicker and darker, and are gradually transformed into hard and opaque bodies. The drying of oils is based upon a chemical process, during which the oil oxidises by absorbing oxygen from the air, and combining a part of it with carbon to form carbonic acid, and another part with hydrogen to form water. The different oils dry with different rapidity, but this rapidity may be modified by the presence of certain substances, or by certain treatment. Linseed oil, for instance, according to the way in which it has been pressed out of the seed, contains more or less mucilaginous substances. These latter impede the drying of the oil, and have therefore to be removed by a refining process. If linseed oil in a shallow vessel is exposed to the air and light, and especially to a green light, it soon begins to dry, and is transformed first into a kind of varnish and gradually into a solid opaque substance. The drying may be quickened by boiling, and more particularly by the addition of lead, zinc, or manganese. In this way a quick-drying oil varnish may be prepared and used as a siccative. It follows that there are certain substances which impede the drying of oils, and others which facilitate it. Amongst the pigments are some which belong to this category of bodies; white-lead, zinc-white, minium, vermilion, for instance, facilitate the drying; others, such as ivory-black, bitumen, madder-lake, will impede it. Supposing, now, we should add to each of the different pigments the same quantity of oil, the drying of it would progress at different rates. But in reality this difference is very greatly increased by the fact that the different pigments require very different quantities of oil, in order to be ground to the consistency requisite for painting.

Pettenkofer quotes the following figures, given to him by one of the colour manufacturers:—

100 parts (weight)	White-lead	require	12	parts of oil.
,,	Zinc-white	,,	14	,,

100 parts	(weight)	Green chrome ...	require	15	parts of oil.
,,	,,	Chrome-yellow ...	,,	19	,,
,,	,,	Vermilion	,,	25	,,
,,	,,	Light red	,,	31	,,
,,	,,	Madder-lake	,,	62	,,
,,	,,	Yellow ochre... ...	,,	66	,,
,,	,,	Light ochre	,,	75	,,
,,	,,	Cassel's-brown ...	,,	75	,,
,,	,,	Brown manganese	,,	87	,,
,,	,,	Terre verte	,,	100	,,
,,	,,	Parisian-blue	,,	106	,,
,,	,,	Burnt terre verte ...	,,	112	,,
,,	,,	Berlin-blue	,,	112	,,
,,	,,	Ivory-black	,,	112	;,
,,	,,	Cobalt	,,	125	,,
,,	,,	Florentine-brown ...	,,	150	,,
,,	,,	Burnt terra sienna	,,	181	,,
,,	,,	Raw terra sienna ...	,,	240	,,

According to this table a hundred parts of the quick-drying white-lead are ground with twelve parts of oil, and on the other hand, the slow-drying ivory-black requires one hundred and twelve parts of oil.

It is very important that artists should have an exact knowledge of these matters. But it seems to me that they are insufficiently known to most of them. All, of course, know perfectly how different the drying quality of different colours is. But that these different colours introduce into the picture so different a quantity of oil, and how large this quantity is in the colours they buy, and further, that the oil as well as the mediums or siccatives they add to dry the colours, are gradually transformed into a caoutchouc-like opaque substance, which envelops and darkens the pigments; and moreover, that the oil undergoes—not in the beginning, but much later on when it is already completely dry—changes of volume, and so impairs the continuity of the picture—all this is not sufficiently known. Otherwise, the custom of painting with the ordinary oil colours to be bought at any colourman's, would not have been going on for nearly a hundred years in spite of all the clearly shown evil results; results due, chiefly, TO THE PRINCIPAL ENEMY OF OIL PAINTING, THAT IS TO SAY, THE OIL.

That the masters of the fifteenth and sixteenth centuries did not use colours prepared in this way you may consider as absolutely certain; and if we hear the lost secret spoken of, and if we read that the pupils of the old masters had to pledge themselves to keep the secret, we may be sure that it is neither the method of painting nor the pigment used for it which is concerned in that secret, but exclusively the way of preparing the colours. The preparation was a very complicated one, varying with the different pigments; and we know that the pupils passed six years, that is half of the apprenticeship, in grinding the colours for the master.

And therefore it is to this very point that everyone who wishes to study the method of the old masters must first of all direct his attention. I, too, was led by the study of this question to analyse and restore old pictures. The possibility of making such analysis we owe to the relation between the old masters and their pupils. Of course we could not dissect or chemically analyse works of Titian or Raphael. But fortunately the pupils painted with the same material and by the same method as the masters, and thousands of pictures by the pupils, *well preserved* or in different stages of decay, may be easily procured.

I have myself, from among a very great number of such pictures, selected about one hundred specimens, part of which I have brought before you. As their artistic value is not, as you perceive, of the highest description, we need not feel any scruple in experimenting upon or even destroying them, if we can thereby gain any valuable information.

(*To be continued.*)

GAS-LIGHTING BY ELECTRICITY

FOR some time past the street lamps in Pall Mall, Waterloo Place, and part of Regent Street, have been connected by wires, which may have led the uninitiated to think that a new method of fixing telegraphic wires was about to be adopted. This is not the case, however, for although the wires were connected with a battery, they were not intended to convey telegraphic messages, but to experiment on a new method of lighting street lamps by means of electricity. The inventor of this method is Mr. St. George Lane Fox, who recently described his invention to the Society of Arts. Should Mr. Fox's method be adopted, the wires, instead of running from lamp to lamp above ground, will be carried along under ground, and the only thing visible would be a small piece of boxed-in mechanism just under the burner of each lamp. The experiment which was made on Saturday afternoon was not, we believe, completely successful. The magneto-electric machine and the battery which supply the current were placed in a small temporary instrument-house at the bottom of Waterloo Place. At the first trial the whole of the lamps in the circuit were lighted by the current, though in a second trial some of the lamps failed to respond to the current; but that this was owing to some local cause is probable from the fact that the first and last lamps in the circuit always responded to the discharge. We shall endeavour to explain the method adopted by Mr. Fox.

Fig. 1.

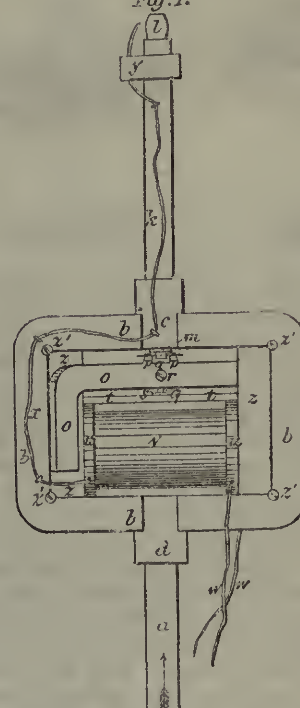
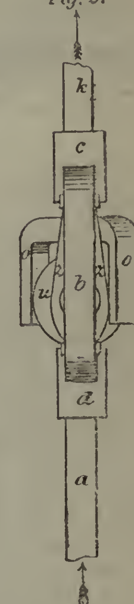


Fig. 2.



In the first place he supplies every lamp with an apparatus similar to Fig. 1; next the lamps must be connected with an insulated conductor, so that, starting from a central station, a wire would travel through each of these machines and back again to the station. Mr. Fox proposes that several of these circuits, each connecting and controlling 200 or 300 lamps, should proceed or radiate from a central station, so that from one point several thousand lamps could be operated upon almost instantaneously.

The method by which he has succeeded in producing the ignition of the gas at a considerable distance, and at numerous points, is by supplying each lamp with a small induction coil, so that the primary wires of each one of these induction coils forms part of the circuit, so in fact as to preserve without a break the metallic continuity of the line. After several experiments it occurred to him that in reality the amount of work to be done in producing a number of small electric sparks was extremely minute, although at the same time requiring to be produced almost instantaneously. Now the amount of work which an electric battery will produce is dependent on the time during which action continues, and in a single instant, or say the thousandth part of a second, the actual amount of power available is naturally extremely small, and he thought that if he could by any means accumulate this power for a short time and then bring it suddenly to bear upon the circuit, the desired result would be obtained. By means of an apparatus he succeeded in accumulating the electric current and storing it up into the condenser or

electric reservoir, which is composed of glass plates and tin-foil laid side by side alternately.

The condenser, however, is not charged direct by the battery, but the current is made to work this Ruhmkorff induction coil, from which there is derived a current having an enormously increased electromotive force, and it is this electricity that is stored up in the condenser.

Having charged the condenser in this fashion, the whole of the electricity is at once sent through the line, and produces most extraordinary results. So much, then, for the lighting of the gas. The process of turning on and off the gas, although involving many important details, is very simple. Mr. Fox makes use of the soft iron core which runs through the centre of the coil to produce a reciprocating horizontal motion of a permanent horse-shoe magnet, suspended on needle-points just above the coil. The soft iron core with the primary coil is, in

Fig. 3.

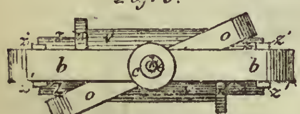
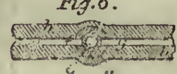


Fig. 5.

Fig. 4.

Fig. 6.



fact, an electro-magnet, which can be magnetised so as to render its poles reversible at pleasure; the magnets are carried in a small metal frame, having a passage through it for the gas to pass to the burner at the top, and being provided with a stop-cock, or valve, which is actuated by the reciprocating magnet. The whole of this apparatus is inclosed in an air-tight metallic case, which measures about $2\frac{1}{2}$ inches high, by $2\frac{1}{2}$ wide, and is screwed on to the supply-pipe in the lamp, the insulated conductor or line-wire being carried down the interior of the lamp-post and laid under ground, except, of course, where an overhead line is admissible. The turning of the gas on and off is accomplished by opening and closing what may be termed an electric needle-tap. The plug of this needle-tap is cylindrical, and about a quarter of an inch in diameter, and is carried in a socket, which it fits rather loosely. It is made to turn in this socket by the action of the reciprocating magnet, a couple of studs, which are brought into contact with a small pin or lever connected with the plug, and forming, in fact, the handle of the stop-cock. The annular space between the plug and the socket (which is about one-thousandth part of an inch) is filled with some liquid, which is retained by capillary attraction between the two surfaces, the joint being thus rendered perfectly gas-tight. The oil of bitter almonds or glycerine are both well adapted for this purpose, on account of their non-oxidisable character, and from the power they possess of resisting the action

Fig. 7.

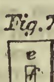


Fig. 8.

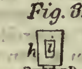


Fig. 9.

of very low temperatures. A special feature in the apparatus is the introduction of a fixed core, which can be magnetised, so as to render its poles reversible at pleasure, and in conjunction with it a movable magnet, the polarity of which is permanent. An electric current sent either forwards or backwards for a few seconds will turn the gas on or off in every lamp in the circuit according to the direction of the current.

To put the system into practical operation, there would be for any district of, say two or three thousand lamps, a central station, from which the wires would proceed in every direction, so as to command a number of distinct circuits; all that is necessary to have at the central station would be a battery of some sort. Mr. Fox would much prefer a magneto-inductor. By means of a switch and a commutator the electric current from this machine can be directed so as to operate separately on each one of the circuits, and by this means turn the gas on or off. When the gas is turned on it is lighted by sending a discharge from the condenser. It is constructed of alternate metallic plates, with an insulator or dielectric between them; the conducting surfaces in this case are of tinfoil, and the dielectric of crown glass. The coil used for charging the condenser need not give more than about three-quarters of an inch spark in the air. The discharge, like the current, will of course have to be sent through each circuit separately, and this is also done by means of the switch arrangement.

The accompanying figures will enable the reader more clearly to understand the description we have given above.

Fig. 1 is a front elevation, Fig. 2 a side elevation, and Fig. 3 a plan of the apparatus; Fig. 4 is a plan of the permanent magnet; Figs. 5 to 9 represent details to be referred to.

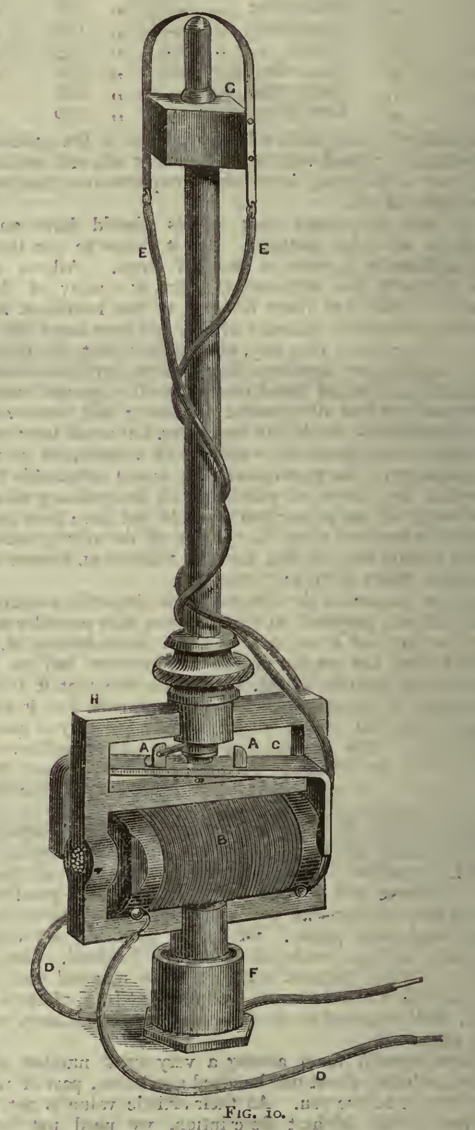


Fig. 10.

a is the gas-pipe leading from any ordinary source of supply; *b b* is a rectangular frame of white metal or brass, cast or made with a hollow core, and having two cylindrical portions, *c d*. The part *d* is screwed upon the gas-pipe *a*, which supports the apparatus, and the part *c* receives the stop-cock; this cock, which is shown in sectional elevation in Fig. 5, and in sectional plan in Fig. 6, is composed of a brass tube *e* (shown separately in Fig. 7), which fits into the cylindrical part *c*, and has two openings, *f f*, corresponding with the passages *g g*, in the opposite sides of the frame *b b*; *h* is the plug of the cock (shown separately in Fig. 8); it is made with a very slight downward taper, and has two apertures or ways *i i*, corresponding with the openings *f f* in the tube *e*, and it is hollowed out in the middle. When the gas is turned on, the apertures *i i* come opposite the

openings *ff*, the gas having then a free passage from the pipe *a* through the two sides of the frame *b b*, and into and through the plug *h*. It will be seen on reference to Fig. 6, that a small turn of the plug is sufficient to open or close the cock. *k* is a pipe screwed into the tube *e*, and leading to the burner *l*. *m* is a projection at the lower end of the plug, and *n* is a pin passed through the same. The plug is supported on the point of the pivot on which a magnet turns, so that very little power is required to turn the plug. *o* is a permanent magnet, which may be either cast in steel, with the two projecting pieces *p p*, or made out of a steel bar bent into the proper shape, and in this case the projections *p p* are produced by screwing in two pieces of metal. *q* is the pivot on which this magnet turns; it is passed through a vertical hole in the magnet, and fixed by a screw *r*. The lower end of the pivot rests in a steel step *s*, which is supported by a small wooden beam *t*, secured to the ends of the wooden bobbin *u*. *v* is the induction-coil; it is composed of a core of soft iron wires, two layers of primary wires wound with covered copper wire of about No. 20 BWG, and upon these about ten to fifteen layers of secondary wire of about No. 40 BWG. The primary wires *w w* form part of the circuit by which the lamps to be lighted or extinguished simultaneously are connected. One end of the secondary coil is connected to an insulated wire *x*, leading to the burner *l*, where it terminates in a platinum point, and the other end is connected to the frame *b*, or to any other metallic part of the apparatus, so as to be in metallic connection with the burner. The insulated wire *x* passes through an earthenware support *y* (seen in plan in Fig. 9), fixed to the pipe *k*. The soft iron core projects about three-eighths of an inch from each end of the wooden bobbin *u*. The bobbin is fastened to wooden supports *z z*, which are fixed to the frame *b* by screws $z^1 z^1$.

Fig. 10 (for the use of which we are indebted to the Society of Arts) is a view of the complete apparatus as attached to a gas lamp.

AMERICAN SCIENCE

THE March number of the *American Journal of Science* opens with a valuable paper, in which Prof. Norton collates the various observations made on Coggia's comet. The theory of cometary phenomena he arrives at is (briefly) that the direct action of the sun on the side of the nucleus exposed to the solar rays is to form an envelope of gaseous carbonic oxide. This envelope of diamagnetic gas is traversed by the ideal lines of magnetic force proceeding from the nucleus, which are also lines of conduction through the gas. The electricity set free by the ascending currents of gas, by reason of the diminished gaseous pressure, is propagated along these lines, and the impulsive force of the electric currents detaches streams of the successive molecules of the gas in the direction of the lines of conduction. Both the nucleus and the sun exert repulsive forces on the escaping molecules; but their effective actions may be either repulsive or attractive, according as their attraction prevails over the attraction of gravitation, or the reverse. The author elucidates this theory at some length.

In a reply to Mr. Mallet's review (in the *Philosophical Magazine*) of General Abbott's paper on the velocity of transmission of earth-waves, in which the value and accuracy of the Hallet's Point observations were doubted, the General describes some new observations on the subject, which seem to establish these points: 1. A high magnifying power of telescope is essential in seismometric observations. 2. The more violent the initial shock the higher is the velocity of transmission. 3. This velocity diminishes as the general wave advances. 4. The movements of the earth's crust are complex, consisting of many short waves first increasing and then decreasing in amplitude, and, with a detonating explosive, the interval between the first wave and the maximum wave, at any station, is shorter than with a slow-burning explosive.

A new method for decomposition of chromic iron, proposed by Mr. Smith, consists in exposing it (in an exceedingly fine state) with bromine to a temperature of 180° C. from two to three days. Prof. Marsh furnishes an account of some new Dinosaurian reptiles.—Prof. Kimball describes some experiments on journal friction at low speeds.—There are also notes on some reactions of silver chloride and bromide, brightness of the satellites of Uranus, &c.

The new number of *Appalachia*, the journal of the Appalachian Mountain Club, contains a valuable address by the president, Dr. S. H. Scudder, in which he reviews the principal scientific expeditions in the United States during the past year. Dr. Scudder himself is attached to the Hayden Survey, and made the discovery of the beds of fossil insects at Florissant, near Manitou, Colorado. During the past year 20,000 fossil insects have been exhumed from this quarry.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—The fifth and final report of the Syndicate appointed in May, 1875, to consider the requirements of the University in different departments of study, has been issued. The Syndicate have considered the question of the residence to be required of professors. They are of opinion that it is desirable—(1) that the time for which the University may require the residence of professors shall be left to be determined by the University in the case of each professorship, without any general statutable restriction; (2) that no professor shall be considered to satisfy the condition of residence who is not for the time required making his home within a mile and a half of Great St. Mary's Church, unless special permission, available for not more than one year at a time, but renewable, be granted by the Vice-Chancellor and Sex Viri, and that such permission shal not be granted unless the Vice-Chancellor and Sex Viri are satisfied that the professor has made such arrangements as will secure his being reasonably accessible in Cambridge during term time. The Syndicate have also had under their consideration the importance of individual personal intercourse between students and teachers, and it has also been suggested that the inspection and revision of students' note-books by the teacher may in many cases be of considerable use. The precise manner in which such personal intercourse may be most effectually secured will probably vary very much in different subjects and for different teachers, but it seems important that the arrangements should be such that the professor himself may in all cases see a portion of the work of his class, so as to make himself accurately acquainted with their wants. The Syndicate have referred to the Board of Medical Studies the question whether it is desirable to found a complete medical school in Cambridge so as to make it possible for a student to complete his whole medical course here, or whether it is better for all concerned, while making the teaching at Cambridge as perfect as possible in the scientific subjects which are the basis of medicine, to leave students to carry on elsewhere the greater part of their clinical studies and most of what relates directly to the practice of medicine. The reply of the Board of Medical Studies states that they consider it inexpedient that students should complete their whole professional education at any single medical school, and that it is therefore desirable that students should pursue their studies away from Cambridge for a year or more before commencing practice, either before or after their final M.B. examination. They believe, however, that it would be in most cases advantageous to students to carry their medical studies in Cambridge further than is usually done at present, and in some cases as far as the final M.B. examination, and they are therefore of opinion that the University should provide systematic instruction in all the subjects necessary for a medical degree, as is done at other Universities. In order that this may be carried out satisfactorily the Board of Medical Studies think that the University should provide:—1. A Professor of Pathology. 2. A Professor of Surgery. 3. Systematic teaching in (1) midwifery and the diseases peculiar to women (2) medical jurisprudence; (3) sanitary science; (4) mental diseases. 4. Systematic clinical teaching.

R.G.S. PUBLIC SCHOOLS' PRIZE MEDALS.—The following is the award of the Public Schools' Prize Medals annually given by the Royal Geographical Society:—Physical Geography—Gold Medallist, William John Newton, of Liverpool College; Silver Medallist, Christopher Mounsey Wilson, of Clifton College; Honourably Mentioned—E. G. Harmer, University College School; M. H. Clifford and M. A. Soppitt, of Dulwich College; and J. S. G. Pemberton, of Eton College. Political Geography—Gold Medallist, William Wallis Ord, of Dulwich College; Silver Medallist, George Arnold Tomkinson, of Haileybury College; Honourably Mentioned—A. R. Ropes, of the City of London School; A. Kay, of Rossall School; and D. Bowie, of Dulwich College.

GREIFSWALD.—The University has received a grant of 381,000 marks for a new library building, and 200,000 marks for the construction of a physical laboratory.

SCIENTIFIC SERIALS

Bulletin of the Nuttall Ornithological Club. A Quarterly Journal of Ornithology. Vol. III. January, No. 1.—This journal, on entering upon its third volume, has increased its quarterly numbers from a thin part of twenty-four pages to a part containing forty-eight pages and a coloured plate. It will continue, as before, under the editorial management of Mr. J. A. Allen, assisted by Prof. Baird and Dr. E. Coues, and it is intended that the volume for the current year should contain an exhaustive *résumé* of the current literature relating to North American Ornithology. The present number contains—Dr. E. Coues: On *Passerculus bairdi* (with plate), and *P. princeps.*—H. W. Henshaw on the species of Passerella.—W. A. Cooper: On the breeding of *Carpodacus purpureus*, var. *Californicus.*—W. Brewster: On the first plumage of North American birds.—J. A. Allen: On Wallace's theory of birds' nests.—N. S. Goss: Breeding of the duck hawk in trees.—Notes of recent literature and general notes.

Reale Istituto Lombardo di Scienze e Lettere, Rendiconti, vol. xi. fasc. iii.—On the action of so-called catalytic force viewed according to the thermodynamic theory, by M. Tommasi.—Study on the dominant diseases of vines, by M. Garovalio and Cattaneo.—On the chronology of Tyrrhenian volcanoes, and on the hydrography of the Val di Chiana previously to the miocene epoch, by M. Verri.—On the permanent magnetism of steel at different temperatures, by M. Poloni.—On the plasmogonic production of leptothrix and leptomitus, by M. Cattaneo.—On the refrigeration of pulverulent metallic solids (continued), by M. Cantoni.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, February 28.—"On the Reversal of the Lines of Metallic Vapours," by G. D. Liveing, M.A., Professor of Chemistry, and J. Dewar, M.A., F.R.S., Jacksonian Professor, University of Cambridge. No. I.

In order to examine the reversal of the spectra of metallic vapours, the authors observe the absorptive effect produced on the continuous spectrum emitted by the sides and end of the tube in which the volatilisation takes place. For this purpose they use iron tubes about half an inch in internal diameter, and about twenty-seven inches long, closed at one end, thoroughly cleaned inside, and coated on the outside with borax, or with a mixture of plumbago and fireclay. These tubes are inserted in a nearly vertical position in a furnace fed with Welsh coal, which will heat about ten inches of the tube to about a welding heat, and they observe through the upper open end of the tube, either with or without, a cover of glass or mica. To exclude oxygen, and avoid as much as possible variations of temperature, they introduce hydrogen in a gentle stream through a narrow tube into the upper part only of the iron tube, so that the hydrogen floats on the surface of the metallic vapour without producing convection currents in it. By varying the length of the small tube conveying the hydrogen, they are able to determine the height in the tube to which the metallic vapour reaches, and to prevent further displacement of the vapour, and thus to maintain different lengths of the iron tube full of metallic vapour at a comparatively constant temperature for considerable periods of time.

By this means the following observations have been made up to the present time:—

The first metal experimented on was thallium, one of the most volatile of metals. After arranging the current of hydrogen so as to keep the tube free from air, but without any rapid movement of the gas, they saw the characteristic line reversed, and maintained it so for a considerable time.

The metal indium, closely allied in its behaviour and volatility to thallium, was next examined, and they observed the bright blue line reversed. This was most plainly visible when that portion of the vapour which was nearest to the sides of the tube was looked through.

They had great difficulty in preventing the oxidation of magnesium in the tube, and in using tubes wider than half an inch, did not succeed in getting any reversal, but with half-inch tubes the *b* lines were clearly and sharply reversed, also some dark lines, not measured, seen in the blue. The sharpness of these lines depended on the regulation of the hydrogen current, by which the upper stratum of vapour was cooled.

A piece of metallic lithium was introduced, and gave no results. Sodium was next added in the same tube, and this did not bring out the reversal of the lithium lines. Similarly, chloride of lithium and metallic sodium, introduced together, gave no better results. To a tube containing potassium vapour, some lithium chloride was added, but no lithium line appeared. On adding metallic sodium to this atmosphere, and more lithium chloride, the bright-red lithium line appeared sharply reversed, and remained well defined for a long time. It is worthy of observation that the lithium line was only reversed in a mixture of the vapours of potassium and sodium, and it seems highly probable that a very slightly volatile vapour may be diffused in an atmosphere of a more volatile metal, so as to secure a sufficient depth of vapour to produce a sensible absorption. This would be analogous to well-known actions which take place in the attempt to separate organic bodies of very different boiling points by distillation, where a substance of high boiling-point is always carried over, in considerable quantity, with the vapour of a body boiling at a much lower temperature. It is a matter for future investigation how far chemical interactions taking place in a mixture of metallic vapours affect the volatility of a third body, and what relation, if any, this may have to such phenomena as the increased fusibility of mixtures of salts of potassium and sodium, and the well-known fluidity of the alloy of those metals.

As the authors have had occasion to use sodium and potassium in their tubes, they have had opportunities of observing the absorption spectra of these metals, and they find that there is a great deal yet to be observed in regard to these spectra. Up to the present time they have not observed any of the appearances noted by Lockyer, "On a New Class of Absorption Phenomena," in the *Proceedings* of the Royal Society, vol. xxii., but they have repeatedly noted the channelled-space spectrum of sodium described by Roscoe and Schuster, in the same volume of the *Proceedings*. They observed in their tubes no channelled space absorption by potassium, but continuous absorption in the red and one narrow absorption band, with a wave-length of 5,730, not corresponding with any bright line of that metal.

With reference to the absorption spectrum of sodium vapour they remark that it is by no means so simple as has been generally represented. The fact that the vapour of sodium in a flame shows only the reversal of the D lines, while the vapour, volatilised in tubes, shows a channelled space absorption, corresponding to no known emission spectrum, appears to be part of a gradational variation of the absorption spectrum, which may be induced with perfect regularity. Experiments with sodium, carried out in the way described, exhibit the following succession of appearances, as the amount of vapour is gradually diminished, commencing from the appearance when the tube is full of the vapour of sodium, part of it condensing in the cooler portion of the tube, and some being carried out by the slow current of hydrogen. During this stage, although the lower part of the tube is at a white heat, we have always noticed, as long as the cool current of hydrogen displaced metallic vapour, that, on looking down the tube, it appeared perfectly dark. The first appearance of luminosity is of a purple tint, and, with the spectroscope, appears as a faint blue band, commencing with a wave-length of about 4,500, and fading away into the violet. Next appears a narrow band in the green, with a maximum of light, with a wave-length of about 5,420, diminishing in brightness so rapidly on either side as to appear like a bright line. This green band gradually widens, and is then seen to be divided by a dark band, with a wave-length of about 5,510. Red light next appears, and between the red and green light is an enormous extension of the D absorption lines, while a still broader dark space intervenes between the green and the blue light. The dark line in the green (wave-length about 5,510) now becomes more sharply defined. This line appears to have been observed by Roscoe and Schuster, and regarded by them as coinciding with the double sodium line next in strength to the D lines, but it is considerably more refrangible than that double line. In the next stage, the channelled space spectrum comes out in the dark space between the green and blue, and, finally, in the red. Gradually the light extends, the channels disappear, the D lines absorption narrows, but still the dark line in the green is plainly

discernible. Lastly, there is only D lines absorption. The blue and the streak of green light at first observed seem to the authors due to luminosity of the vapour itself, where it is somewhat cooled, the later stages being mixed phenomena of absorption and emission.

As the absorption line, with wave-length about 5,510, has not been distinctly recorded by other observers, they have endeavoured to trace it under somewhat different conditions from that of the vapour volatilised in white hot-iron tubes. This absorption-line is easily seen when a gas-flame is observed through a horizontal glass tube, about three inches long, containing sodium volatilised in the middle of the tube by the heat of a Bunsen's burner, and equally well whether the tube contains hydrogen or nitrogen, besides sodium. They have also observed the same absorption-line when a piece of commercial magnesium ribbon (which always contains sodium) is ignited in a horizontal position, so that the metal melts and produces an elongated flame. It is of some interest to note that absorption-lines of about this wave-length, in the solar spectrum, are given by Kirchhoff and Angström not corresponding with emission lines of known elementary bodies.

When potassium vapour is observed, whether in the iron tube or in a glass tube, an absorption-line is seen, with a wave-length of about 5,730, which is more refrangible than the yellow double emission line of potassium, and does not correspond to any known bright line of that metal.

They reserve, for a future communication, the discussion of the identity or non-identity of these absorption-lines with lines in the solar spectrum and the inferences which may be drawn from such determination.

The method of observation described may be used to observe emission-spectra as well as absorption-spectra, for if the closed end of the tube be placed against the bars of the furnace so as to be relatively cooler than the middle of the tube, the light emitted by the vapours in the hottest part is more intense than that emitted by the bottom of the tube. This succeeds admirably with sodium, but they have not specially observed it with other vapours.

Chemical Society, March 22.—Dr. Gladstone, president, in the chair.—The following papers were read:—On aromatic nitrosamines, by Dr. O. N. Witt. The author gives an account of his study of some complicated reactions of diphenylnitrosamine. He has found that ordinary ethylic nitrite contains nitric acic, and has therefore used mixtures of pure amylic nitrite and nitric acid for acting on diphenylamine, and has obtained mononitrodiphenylnitrosamine in light yellow plates melting at 133·5° C., and two bodies which, on the removal of their nitroso groups, yielded dinitrodiphenyamine and an isomeric substance. The final product of the action of strong nitric acid is hexanitrodiphenylamine.—The next paper was on a new process for the volumetric estimation of cyanides, by J. B. Hannay. The cyanide is dissolved in water, and the solution rendered alkaline by ammonia. A standard solution of mercuric chloride is run in with constant stirring until the liquid is distinctly opalescent. The end reaction is sharply marked and very delicate. The presence of silver does not interfere, so that the process can be used for estimating the cyanides present in a plating bath.—The last paper was on certain bismuth compounds, Part 7, by M. M. P. Muir. The author has compared the behaviour of bismuthous and phosphorous chlorides in certain reactions; the latter substance acts as a reducing agent in some cases in which the former does not exert any such action. The author has also studied two oxalates of bismuth, the production of the so-called bismuthates, and some experiments with bismuthous iodide.—Mr. Williams exhibited a fine sample 24 oz. of natural salicylic acid, also about one gallon of pure methylic alcohol.

Linnean Society, March 21.—W. Carruthers, F.R.S., vice-president, in the chair.—Mr. G. T. Saul exhibited an example of the enormous development of adventitious roots from a species of *Berberis*.—On behalf of Mr. J. Willis Clark of Cambridge, there was exhibited mounted specimens of the male, female, and young of the fur-bearing seal of the North Pacific. Mention was made of the "rookeries" of these creatures, containing over 3,000,000 seals in a compact area. Like old Turks, a male dominates over a harem of a dozen or fifteen females, which he guards with jealous care, for two months or more, never stirring from the spot, and meantime fights terrific battles for its maintenance. A neutral zone exists to the rear of the breeding-grounds, where the enforced bachelors and adolescent young of both sexes repair. These come and go continuously, passing to and fro through free lanes of passage. Others of these animals delight in dashing among the breakers on the surf, or in droves frolic and play on the sand and grassy dunes adjoining the more rocky ground of the "rookery." The method of shaving the fleshy side of the skin, thus cutting loose the roots of the long coarse hairs, and retaining the superficial fine fur of commerce was explained, as also other interesting points in the economy and natural history of the Otaries.—The Secretary read the gist of a paper on the venation of the leaf of hemlock (*Conium maculatum*), by Mr. J. Gorham. The latter's observations show that in a piece ½ inch long, by ½ inch wide, by registration of the veinlets in a tabular form, and constructing these in figure, an exact counterpart of the venation of the entire leaf results. Comparisons of leaves of different umbelliferous genera prove that each can be detected and recognised from the merest fragment.—A communication was made by Mr. B. Clarke on a new arrangement of the classes of zoology, founded on the position of the oviducts, or when these are absent on the position of the ovaries, including a new mode of arranging the mammalia.—A notice in abstract was given on some genera of the Olacaceæ, by Mr. J. Miers. He describes a new genus, Rhaptarrhena, from Brazil, allied to Aptandra; also three other genera, Myoschilos, Arjona, and Quinchamalium, which possess a distinct though small calyx and separate calycle.—The Rev. M. J. Berkeley and Mr. C. E. Broome gave a list of fungi from Brisbane, Queensland. Among these Agarics, Clavarei, and fleshy fungi are scarce; interesting forms of Polyporei obtain while leaf-parasites are poorly represented. Some species are identical with Ceylon and South American kinds, and several are common to Europe.—The following gentlemen were elected Fellows of the Society:—John Evans, F.R.S., C. P. Ogilvie, Arthur Veitch, and Sydney H. Vines, B.A.

Zoological Society, March 19.—Mr. Arthur Grote, vice-president, in the chair.—The Secretary exhibited the type specimen of *Dicrurus marginatus* of Blyth, and pointed out its identity with *Muscipipra vetula* (fam. Tyrannidæ).—Mr. J. W. Clark, F.Z.S., exhibited and made remarks on some stuffed specimens of the Sea Lion (*Otaria ursina*) of the Prybylov Islands, which had been presented to the Museum of the University of Cambridge by the Alaska Commercial Company.—A communication was read from the Marquis of Tweeddale, F.R.S., containing the sixth of his contributions to the ornithology of the Philippines. The present memoir gave an account of the collections made by Mr. A. H. Everett in the Island of Leyte.—Mr. P. L. Sclater, F.R.S., read a report on the collection of birds made during the voyage of H.M.S. *Challenger*, in the Sandwich Islands, and pointed out the characters of a new species of duck, of which it contained specimens, and which he proposed to call *Anas wyvilliana*.—A communication was read from Mr. W. A. Forbes, F.Z.S., containing notes on a small collection of birds from the Samoan Islands and the Island of Rotumah, Central Pacific.—A communication was read from Mr. F. Nicholson, F.Z.S., containing a list of the birds collected by Mr. E. C. Buxton, at Darra Salam, on the Coast of Zanzibar.—Messrs. F. Du Cane Godman and Osbert Salvin gave descriptions of new species of Central American butterflies of the family Erycinidæ.—Prof. A. H. Garrod, F.R.S., read some notes on the visceral anatomy of *Lycaon pictus* and *Nyctereutes procyonides*.—A communication was read from Mr. Andrew Anderson, F.Z.S., containing the description of a new Indian *Prinia*, obtained in the Bagesur Valley, North-Western Himalayahs, which he proposed to name *Prinia poliocephala*.

Meteorological Society, March 20.—Mr. C. Greaves, president, in the chair.—Mr. B. L. Smith was elected a Fellow.—The discussion on Dr. Tripe's paper on the winter climate of some English sea-side health resorts was resumed and concluded, after which the following papers were read:—Notes on a water-spout, by Capt. W. Watson, F.M.S.—Notes on the occurrence of globular lightning and of waterspouts in Co. Donegal, Ireland, by M. Fitzgerald.—Observations of rainfall at sea, by W. T. Black.—The discussion on the subject of waterspouts and globular lightning was adjourned till the next meeting, on April 17.

Anthropological Institute, March 26.—Mr. John Evans, D.C.L., F.R.S., president, in the chair.—The following new members were announced:—Dr. Sebastian Evans and Dr. Allen Thomson, F.R.S.—A paper was read by Mr. Francis A. Allen on the original range of the Papuan race. This paper was a brief *résumé* of the opinions held by many anthropologists

with regard to the origin, characteristics, and distribution of these races, and an attempt to prove that they once extended on the west as far as Africa, and on the east as far as America. The writer especially dwelt upon the statements of Herodotus with regard to the eastern and western Ethiopians, and the black Colchians, and referred to the legend of the Asiatic Memnon, and the existence of black races in Central America, within the historic period. The director then read a paper by Dr. Julius von Haast, F.R.S., on some ancient rock paintings in New Zealand. The author considered that when these rock paintings were carefully studied by archæologists and linguists, they would prove that at one time there had been an introduction of a far higher civilisation than the Maories ever reached.

Institution of Civil Engineers, April 2.—Mr. Bateman, president, in the chair. The paper read was on the Huelva Pier of the Rio Tinto Railway, by Mr. T. Gibson, Assoc. Inst. C.E.

Edinburgh

University Chemical Society, February 20.—John Gibson, Ph.D., F.R.S.E., presiding.—Mr. W. L. Goodwin read a paper on a method of removal of iron from cupric sulphate for analytical purposes, in which he stated that this could be performed by the replacement of the iron by cupric hydrate.—Mr. Alexander Macfarlane, M.A., B.Sc., read a paper on the disruptive discharge of electricity, in which he gave the difference of potential necessary to produce sparks at different distances up to ten millimetres, and also with different pressures and gases as dielectrics.

February 27.—W. Inglis Clarke, B.Sc., in the chair.—A paper on electrolysis was read by R. M. Morrison, D.Sc., Chemical Demonstrator of the University, in which he traced the history of electrolysis down to the present time, showing that as recently as 1840 the art was practically in its infancy, and that at the present day it was in numberless ways made use of. The chief points of theoretical and practical interest were dwelt upon, both with regard to the various metals which could practically be used, and to the solvents from which the best results were obtainable.

Vienna

Imperial Academy of Sciences, January 10.—On the behaviour of propylic glycol in a high temperature, by M. Linnemann.—On the direct transformation of isobutylic iodide into trimethylcarbinolamin, by M. Brauner.—On artificial malic acid from evernic acid, by M. Loydl.—On the Maxwell-Simpson synthesis of aerolein from diiodacetone, by M. Voelker.—On the behaviour of β bibrompropionic acid towards iodide of potassium, by M. Zotta.—On the so-called rag-illness of workers in paper manufactories, by M. Frisch.

January 17.—The undulating nutation of internodes, by M. Wiesner.

January 31.—Determination of the path of the second comet of 1874, by M. Wenzel.—Contributions to a fuller knowledge of the Tunicata, by M. Heller.—On Ampère's fundamental electrodynamic experiments, by M. Ettinghausen.—On the behaviour of phoroglucin and some related substances towards woody cell membranes, by M. Wiesner.—On the degeneration of leaf-growth of some Amygdaleæ, produced by species of *Exoascus*.—On the theory of surface potential, by M. Wassmuth.—Contribution to study of electricity, magnetism, terrestrial currents, magnetic variation, declination, inclination, and intensity, by M. Daubrawa.—On a simple method of drawing a tangent to the ellipse and parabola, by M. Zimels.

Paris

Academy of Sciences, April 8.—M. Fizeau in the chair.—The following among other papers were read:—Extract from a work by M. Chevreul, on the vision of colours. M. Chevreul describes some effects obtained by rotation, with diminishing speed, of a disc having one half red the other white, as compared with a similar disc viewed when at rest.—On the transparence of coloured flames, by M. Gouy. For measuring very weak radiations the objective of the collimator of a spectroscope is half covered with a plane mirror which reflects the rays from a second collimator parallel to the axis of the first. Thus in the focal plane of the telescope are got two superposed spectra, received on a slit parallel to the lines, which serves as eye-pieces. The two flames compared send their rays through the two collimators respectively; the eye sees through the prisms half of each of the objectives as a circle with its two halves of the same colour, but differing in brightness. The same brightness is given them by means of two Nicols on the second collimator. The angle of the principal sections is then read, and indicates the result of experiment. Coloured flames, got from a mixture of common gas and air with a finely-powdered salt in it, were carefully regulated and inclosed in another flame at the same temperature, but without metallic vapour. M. Gouy demonstrates the transparency of flame for the rays it does not emit, and for its own radiations.—On the variation of indices of refraction in mixtures of isomorphous salts, by M. Dufet. He finds that the differences between the indices of a mixture of two isomorphous salts and those of the component salts are in inverse ratio of the number of equivalents of the salts present in the mixture.—Direct fixation of oxygen and sulphur in benzine and toluene, by MM. Friedel and Crafts. Such fixation is accomplished by the intervention of chloride of aluminium; the authors cite it as supporting their hypothesis about this class of reactions.—Researches on nitrification by organic ferments, by MM. Schlœsing and Muntz. The vegetable organisms, mould and mycoderms, which are strongly productive of combustion of organic matter, do not produce nitrification; on the contrary, they transform nitric acid, placed at their disposal, first into organic matter then, partly, at least, into free nitrogen, the last phenomenon being often attended by production of ammonia. Hence they effect a loss of the combined nitrogen on the surface of the globe. The function of nitrifying combined nitrogen seems to be the special attribute of a group of particular beings, and not common to all the organisms which are intermediaries of combustion.—Absorption by the living organism of carbonic oxide introduced in small quantities into the atmosphere, by M. Gréhant. Man or an inferior animal caused to respire for half an hour in an atmosphere containing only $\frac{1}{779}$ of carbonic oxide, absorbs this gas sufficiently for about half of the red corpuscles combined with the gas to become incapable of absorbing oxygen, while in an atmosphere containing $\frac{1}{1449}$ of carbonic oxide, about a fourth of the red corpuscles are combined with this gas.—On the organ called chorda dorsalis in *Amphioxus lanceolatus*, by MM. Renaut and Duchamp. Amphioxus deprived of red blood containing hæmoglobin in its special elements, has no longer a chorda dorsalis comparable in its structure to that of all vertebrates.

CONTENTS

THURSDAY, APRIL 25, 1878

THE COMING TOTAL SOLAR ECLIPSE[1]

II.

IN my former article I referred to the possible employment of slitless spectroscopes during the coming eclipse, the prism being replaced by a grating in some cases. It will be convenient here to give the results arrived at by the Siam Expedition with an instrument of this description, which, for shortness, was called a prismatic camera.

The plates secured present at first sight a very puzzling appearance; they are unlike anything ever obtained before, and a good deal of thought had to be spent upon them before all the knowledge they were afterwards found capable of furnishing to us was properly appreciated. One of the plates was exposed for one minute at the commencement of totality, the other for two minutes at the end. The differences between them are those due to the phases of the eclipse. In the first, two strong protuberances close together are photographed; these are partially covered up in the second, while another series is revealed on the following limb in consequence of the motion of the moon over the sun.

Now in both the photographs—that exposed for one minute and that exposed for two—the strongest of the prominences are repeated three times, that is to say, three spectral images of them are visible, each of these images being produced by light of different wave-lengths which the prominences emitted.

The question is what are these particular wave-lengths thus rendered visible? Unfortunately no photograph was taken of the cusps either before or after totality; a scale therefore was out of the question; and when the task of assigning wave-lengths to these spectral images fell upon Dr. Schuster and myself, while we were preparing the Report which was sent in to the Royal Society last year, the difficulties we encountered were very considerable.

Everybody I think will consider that we were justified in expecting the lines of hydrogen to be represented in such a photograph. Now the photographic hydrogen lines are those at F, near G and at *h*, and the silver salts usually employed are such that the action is most intense near G, less intense near *h*, and least at F; the running down from G to F being rapid, and that from G to *h* much more gradual, so that while at one end F may be said to be the limit of photographic activity, at the other it is continued long past *h*. We were therefore justified in assuming as the preliminary hypothesis, that the image of least refrangibility was produced by the F light of hydrogen, the more so as the continuous spectrum also photographed—which continuous spectrum, as we had independent means of determining, came from the base of the corona—gave us also an idea of the part of the spectrum in which each image was located.

Taking then F as a starting point and assuming the next line to be the one near G, we had a quite satisfactory method of checking the assumption, by comparing the real distance between the images with the calculated one.

[1] Continued from p. 483.

A goniometer was therefore brought into requisition, and the angular distance between F and the line near G carefully measured in order to determine the dispersion of the prism actually employed. This dispersion was one which should bring the images about as far apart as they were actually found to be; this therefore was so far in favour of our assumption, that is to say, it did look as if we had got hold, on the photographs, of images of the prominences built up by the F and G light of hydrogen.

It was next the turn of the third line, the one at *h*. On the assumption already made, it was easy to determine the distance from the G image, at which the one representing *h* should lie. In this place, however, we found no image whatever of any of the prominences.

Now this was a very extraordinary result, and there was only one way, so far as we could then see, of accounting for it. Dr. Frankland and myself, nearly ten years ago now, produced evidence which seemed to indicate that this line of hydrogen was only produced by a very high temperature. This being so, then, we should have to conclude that the prominences were of a relatively low temperature; this, however, I am far from saying, and here there is undoubted work of the greatest value to be done at the next eclipse, and I for one feel certain that our American cousins will do it.

I have not, however, yet referred to the strongest image of all shown in the photographs. This lies a little further from the central one than does the first on the other side of it. On the assumption before stated its wave-length lies somewhere near 3957. This number, of course, is only an approximate one, but the region occupied by the line was obviously so near the boundary of the visible spectrum, that a long series of experiments, in which we called in the aid of photography and fluorescence, was made in order to determine whether an unrecorded hydrogen line existed in that region. All I can say is that the point may be said to be yet undetermined. It is quite true that in several vacuum tubes which Dr. Schuster and myself employed, a strong line more refrangible than H was seen, but then these same tubes unfortunately showed us lines in the visible spectrum, which beyond all doubt did not belong to hydrogen. The elimination of impurities is such a delicate matter, and one requiring such a large expenditure of time, that our report was sent in leaving this point *sub judice*. We tried hydrogen at atmospheric pressure in order to get such a predominance of the hydrogen vibrations as to mask the impurities, but this did not serve us, for the continuous spectrum was so bright in the violet and ultra-violet as to render observations of lines next to impossible. Owing to many reasons, Dr. Schuster's absence from London being one of them, we have not been able to renew the search.

The near coincidence of this spectral image with the H-line leads us to ask the question whether Young's beautiful work in his mountain observatory might not help us on this point. Young found the calcium lines always reversed in the penumbra and near every large spot. This important statement shows us that calcium is one of the metallic vapours which is most frequently ejected from below into the prominences; it is possible, therefore, that the prominences, the spectral images of which were photographed, may have been due

to an eruption of calcium. This, of course, is only a suggestion, but the fact that it is a suggestion merely shows how important it is that this point should engage attention next July. If the prominences are then constituted as they were in '75, this violet line will doubtless turn up again, and that is why I have been most anxious to point out not only the conclusions to which we have been led, but the extreme difficulty of arriving at any conclusion whatever, unless by one method or another we have an absolute comparison of the spectrum of the prominences with that of the sun itself.

I have before referred to the fact of the registration on the plates of a continuous spectrum. If we were to suppose the whole light of the corona to be due to 1474 light, for instance, we should expect to get just as definite an image of the corona in the prismatic camera as in an ordinary one. And if everything outside the moon gave us nothing but a line spectrum, the moon's limb would have a perfectly defined edge. Now as a matter of fact, only one such edge is seen in the photographs. We have only one complete ring with a thoroughly defined hard outline, such as that to which reference has been made. This hard ring corresponds to the second spectral image of the prominences, and is a continuation of it. Supposing we were right about the prominences, the ring would be due to the high temperature *h* line of hydrogen (supposing us wrong it might be a companion line to 1474); as the observations of Respighi, Janssen, and others, in the Indian eclipse of '71 endorsed the American observations of '69 that the hydrogen lines are the strongest in the photographic parts of the corona, we may very possibly be really dealing with hydrogen.

Now the edge of the corona, or the upper part of it considering it as the sun's atmosphere, as seen on our photographs, is precisely such as would be given by homogeneous light; that is, there is a distinct image, and there is one image and not three or any other number. Have we any means of determining the wave-length of the light by which this image has been produced? Let me give an idea of one method which we employed:—A circle of the same size as the image of the moon on a photographic enlargement of the original negative was cut in paper and placed over the enlargement until the corona was symmetrical round it, as we know it to have been symmetrical round the moon's body, or nearly so, at that phase of the eclipse.

We found as a considerable endorsement of the assumption which we made regarding the hydrogenic origin of the chromospheric images, that the paper circle in this position had its circumference coincident with the hard ring to which I have referred as being a continuation of the middle spectral image of the prominences. Next, one of the ordinary photographs of the corona was enlarged to the same size as that of the one produced in the prismatic camera. When these were superposed so that the outlines of both coincided as much as possible, it was again found that the edge of the moon lay along the ring.

Now then for the continuous spectrum. The general woolliness of the photographs which at first sight gives rise to the idea that they were out of focus, and that there is nothing to be got out of them, is of course only in one direction, that at right angles to the edge of the prism employed. There is a well-defined structure running parallel to this direction, which of course is the line of dispersion; this structure is doubtless due to irregularities in the corona, drawn out by the prism into bands; it is easy to determine the limits of this continuous spectrum.

Examining the centre of the photographs we find that on one side the structure stops short at F, on the other it extends to a considerable distance beyond the prominence image in the ultra-violet, spaces of light being visible beyond 3530.

From these data we concluded that the continuous spectrum-giving region extends at least to a distance of 3′ of arc from the sun's limb. This continuous spectrum is well shown on photographs taken at the beginning and end of the eclipse. One of the plates of the prismatic camera was exposed, until the signal for the end of totality was given. Dr. Schuster states that all the observers agreed that the signal was given rather too late, and the fog on the plate indicates an intense illumination; nevertheless, the edge of the sun is not drawn out into a continuous band but rather into three distinct bands. It is probable, therefore, that when the plate was exposed, only the lower part of the chromosphere had appeared, and that it gave out light of such intensity that everybody imagined that the sun itself had come out of eclipse. I observed this myself in 1871, and a very striking fact it is.

So much then for the results obtained by the prismatic camera in '75. When the report is issued—and its issue cannot be much longer delayed—it will be seen that the hasty sketch I have now given can be followed in greater detail.

One of the most remarkable points about the expedition to Siam was the failure to obtain even spectra of the sun with the ordinary telespectroscopic cameras employed. No doubt the unforeseen delays which left very little time for the adjustment of instruments, have a great deal to answer for. I have little doubt that if the attempt is made next July, when any quantity of skilled help will be at hand, and any amount of rehearsal will be possible, that a full measure of success will be obtained, at all events for the most photographic part of the spectrum. An ordinary photograph of the corona was obtained by Dr. Schuster in two seconds; and my experience with photographic spectra enables me to say that this photograph was taken by means of an almost monochromatic light—that near G. Now as the coming eclipse will enable an exposure of almost 100 times longer than this to be employed, I do not think that the undoubted feebleness of the object need be feared. Besides, this method would enable us to pick up the light of those lower reaches of the chromosphere which, as has been already stated, are of such extreme brilliancy as to have been mistaken, on many occasions, for the sun itself.

Up to the present time no attempt has been made to obtain a photographic record of the polarisation of the corona. The difference of colours indicating radial polarisation observed by me when I used the biquartz in 1871, certainly have left the impression on my mind that it would be quite easy to obtain a permanent record of them. This would be a very valuable result, and one

which would set at rest a question which, though I consider it settled in my own mind, is yet, I believe, held to be still doubtful by many interested in these matters.

In what I have written I have touched only upon obvious work suggested by the previous observations. I have little doubt that the preparations of the skilled astronomers of the United States include many surprises and daring attempts among the solid work which we are quite certain of.

All here wish them the extremest measure of success, which I am sure their efforts will do more than command.

J. NORMAN LOCKYER

ATLANTIC SHELLS

Testacea Atlantica; or, the Land and Freshwater Shells of the Azores, Madeiras, Salvages, Canaries, Cape Verdes, and Saint Helena. By T. Vernon Wollaston, M.A., F.L.S. Royal 8vo, pp. 588. (London: L. Reeve and Co., 1878.)

IT is with a saddened feeling we take up our pen to notice this valuable contribution to malacology; for ere its pages had left the hands of the binder, its talented author had passed "into the shadowy land."

The name of Wollaston is connected ancestrally with more than one department of science, and the author of the present work has well maintained the honourable reputation of Dr. Wollaston, the discoverer of palladium and rhodium, and the founder of the Wollaston Medal and Award.

Compelled in 1847 to visit Madeira on account of his health, he commenced to collect the land-shells of the various outlying islands and rocks of the Madeiran Group; and although (as he tells us) insects, rather than mollusca, formed at that time the main object of his researches, he was able to add a considerable number of unmistakably new species to the careful and elaborate catalogue which had previously been compiled by his friend and companion, the late Rev. R. T. Lowe, then chaplain at Funchal, Madeira, and to whom the present work is dedicated.

So interested did he become in the insects and land snails of Madeira, that, although no longer compelled to submit to exile on account of his health, yet he returned again and again to Madeira and spent many weeks under canvas high up among the mountains collecting.

In 1858 he visited the Canaries in the yacht of his friend, Mr. John Gray, and again in 1859. On both these expeditions he was accompanied by Mr. Lowe. He was thus enabled thoroughly to explore the numerous and widely-scattered islands of the Canarian group under the most fortunate circumstances for collecting.

Under the same happy auspices he visited the Cape Verdes in 1866, Mr. Lowe again being his companion. In 1875 Mr. Wollaston sailed for St. Helena with Mr. Gray, where he spent six months in investigating the natural history of that remote little oceanic rock, being on this occasion accompanied by Mrs. Wollaston; the Rev. R. T. Lowe, his friend of many past years, having lost his life in 1874 on his outward voyage to Madeira.

Mr. Wollaston has felt it desirable to place these facts on record, in order to show that the several islands and archipelagos treated of in the volume before us—with the exception of the Azores—had all been visited personally by himself.

Although this book contains descriptions of no fewer than 558 species and varieties of land and freshwater mollusca, the author does not claim for it the position of a monograph, but rather a critical enumeration of all the forms hitherto recorded, with special reference to *habitat* in the several Atlantic archipelagos.

Out of the large number of species and varieties described in this work, there are only twenty-nine which are claimed by the author as *actual novelties;* sixteen of these are from the Canaries, nine from Madeira, two from St. Helena, one from the Salvages, and one from the Cape Verdes. Mr. Wollaston would have conferred a still greater service on his fellow-workers had he given short diagnostic characters of all the species enumerated. This would greatly have facilitated the identification of the various forms and saved the student much time and avoided the necessity of referring in many instances to other works. It is also much to be regretted that references are not given to the excellent published figures of most of the species which are to be found in Reeve's "Conchologia Iconica" and the second edition of Martini and Chemnitz's "Conchylien Cabinet" by Küster. Well-drawn and correctly-coloured figures are almost indispensable for the accurate determination of land-shells where form and colour are dominant characters. It is easy to *see* and identify the form, when carefully delineated, but almost an impossibility to convey it to the mind in words.

Mr. Wollaston has shown throughout the strongest preference for the limitation of species—at times becoming extremely hypercritical—and in some instances he seems to be altogether in doubt as to what constitutes specific rank. For example, under *Helix bicarinata* (*vide* p. 161), he states that he is far from certain that it is more than a phasis of *H. echinulata;* yet a few lines below he observes that he has never found a single example among thousands which could be strictly regarded as intermediate.

Again (p. 209) *Pupa fanalensis*, "this may be only a depauperated state of the var. β. *anconostoma* of the *Pupa umbilicata*, which the latter has gradually assumed through having found its way into the higher regions, nevertheless I believe it to be truly distinct."

It is strange to find a man with Wollaston's admitted talents and vast opportunities for observation struggling hard against the accumulated evidence of more than thirty years, and clinging tenaciously to the last to the doctrine of the immutability of species. Thus in his Summary (p. 561), when commenting on the difficulties which arise in defining what is a "species" and what a "variety," he adds, "these remarks are by no means intended to insinuate that the lines of demarcation between species, when correctly interpreted, are ever, in my opinion, *really* confused or doubtful, the exact opposite having always been my firm belief."

Eliminating what Wollaston calls "the European or more distinctly Mediterranean forms" from the catalogue, so that only "the Atlantic element" remains, "the actual species which range beyond the limits of a single archipelago are marvellously few—about four or five being common to the Madeiras and Azores, about

five or six to the Madeiras and Canaries, and about one to the Canaries and Cape Verdes; whilst between the Azores and Canaries there are only about five, and between the Madeiras and Cape Verdes about one. Moreover there are strong reasons for suspecting that some even of these (perhaps, indeed, most of them) may have been accidentally transported amongst the islands, through indirect human agencies, at a comparatively recent date; so that we are driven to conclude that, so far as the absolute *species* are concerned, of which their aboriginal faunas are respectively made up, the groups are practically almost independent of each other. And yet, in spite of this, I have had occasion to insist more than once upon the many characteristic *types* which, under the aspect of totally different but nevertheless allied species, permeate *to a greater or less extent* the entire 'province,' giving to it an amount of *unity*, through its several component parts, which it is scarcely possible not to recognise." (P. 563.)

The "*Atlantis* hypothesis" was clearly present to Wollaston when he wrote:—

"There may doubtless be many explanations, perhaps equally plausible, of these phenomena, but I must confess that none commends itself so thoroughly to my mind as the possible breaking up of a land which was once more or less continuous, and which had been intercolonised along ridges and tracts (now lost beneath the ocean) which brought into comparatively intimate connection many of its parts, even whilst others, though topographically near at hand, were separated by channels which served practically to keep them very decidedly asunder. It is on some such principle as this that I would account for the Canaries appearing to be not only as widely removed from Madeiras as perhaps even the Cape Verdes are, but (whilst further to the south) *to possess a fauna of which the 'Mediterranean' element is much more traceable.* This latter circumstance, which is shadowed forth likewise by the Coleopterous statistics, is by no means a fanciful one, whole groups which are indicative (more or less) of Mediterranean countries, but which have no single representative elsewhere in these Sub-African archipelagos, being quite at home at the Canaries." (P. 565.)

An evolutionary phase of mind must have prevailed with Wollaston when he penned the following sentence:—

"It is quite clear that the depression of certain tracts, and the upheaval of others, would produce an amount of disturbance in the fauna which could not fail to show itself *in some one way or other* which would afterwards become more or less decipherable; and I cannot conceive much difficulty in picturing the kind of change which might be brought about by the isolation of a cluster of individuals on a small rock, destined henceforth to become the habitat of a race which would, we may feel well nigh certain, rapidly mature for itself *some* slight distinguishing mark." (P. 566.)

But he quickly returns to his former state, and adds:—

"Considering how unmistakable the evidence is for the variability (in this particular sense) of many of the Atlantic types—a 'variability' so decided that a slightly different phasis has been assumed in certain of the Archipelagos, for nearly every separate island and isolated rock, it may sound, perhaps, somewhat paradoxical to speak, nevertheless, of their apparent freedom from further change; and yet if there is one fact more distinctly shadowed forth than another it is, without doubt, their *present stability.*" (P. 566.)

Further on he continues:—

"After the most rigid and conscientious inquiry, I am bound to add that the 'developments,' so called, which might well be supposed to have been slowly elaborated, are (if any) simply *inappreciable.*" (P. 567).

However widely we may disagree with Mr. Wollaston's conclusions on the *quæstio vexata* of species and varieties, his critical remarks are of great importance from the large series of specimens examined by him, and the fact that the types themselves were, in many cases, in his possession.

In an admirable lecture "On Insular Floras," by Sir Joseph Hooker, delivered before the British Association at Nottingham in 1868, he described the Madeiran flora as composed of two elements, the one clearly allied to that of the shores of the Mediterranean, the other totally different, and allied to none other but what is found in the Canaries and Azores, which he designated "the Atlantic Element."

That Sir Joseph Hooker's researches on the flora and Wollaston's observations on the insect and molluscan fauna of these Atlantic islands should bring out precisely similar results, will seem the less surprising when we remember the direct connection and interdependence existing between plants and insects, the latter acting as the great fertilising agents to the former; whilst the dependence of land snails upon plants is equally manifest.

All the difficulties raised by Wollaston as to the rare, peculiar, and isolated forms described by him were met and answered by Sir Joseph Hooker ten years ago. Assuming these minute islands to be relics of an older and once larger land-area which had been gradually reduced by subsidence, he pointed out that such a change, by contracting the area would intensify the struggle for existence. He showed that they were not new forms likely to increase and multiply, but rather old forms dying out. Also that in this exterminating process man was even a more destructive agent than the subsidences of land. For instance Madeira when discovered was so densely wooded that the settlers set fire to the forest and the fire raged for seven years, no doubt exterminating many species and reducing the number of others proportionately.

In Porto Santo rabbits had proved even more destructive than man; whilst in St. Helena, the introduction of goats in 1513, had almost exterminated the forests and the subsequent replanting of the island with exotic plants prevents the remaining indigenous vegetation from resuming its sway.

Whatever be the extent of area which we reclaim from ocean for our ancient "Atlantis," it is evident that formerly intercommunication existed between the Azores, Madeira, the Canaries, the Cape Verdes, and Southern Europe in Miocene times, for Prof. O. Heer considers some of the *Helices* of Porto Santo to agree with those of the Swiss molasse.

The poet's dream may therefore well be realised by the geologist:—

"Which tells, great pictured Continent, of thee
O blest ATLANTIS! can the legend be

Built on wild fancies which thy name surround?
Or doth the story of thy classic ground
With the stern facts of Nature's face agree?
What if no tongue may tell!—thy halo fair
Still lingers round the isles which slumber there."
("Lyra Devoniensis," p. 135).

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Indian Rainfall

As Dr. Hunter has been good enough to mention my name in his letter to NATURE (vol. xvii. p. 59) in connection with a comparison of the rainfall of Northern India and the sun-spot period, I may, I trust, be allowed to express my opinion regarding the validity of some of the conclusions he draws therefrom. In the first place I would remark that Dr. Hunter's idea of the *winter* rainfall of Northern India being due to the *immediate rebound of the summer monsoon* from the Himalayan barrier is at variance with facts in the meteorology of the country. The rebound ought to take place directly the monsoon vapour-current impinges upon the Himalaya, *i.e.*, in the *summer*. In fact, it is by a succession of oblique rebounds from this impassable barrier that the monsoon is gradually reflected towards the N.W.P. and the Punjab.

The *winter* rains, on the contrary, have nothing to do with the monsoon, being, as is well known, due to a branch of the anti-trade, which, descending in the Punjab, is deflected by the Himalayas towards Behar and Bengal, occasionally reaching Calcutta, lat. 22° 35′ N.

Now between the rains of the summer monsoon and those of the anti-trade (or anti-monsoon as it is occasionally called), there is a well-marked interval of bright, clear, settled weather for two or three months throughout Northern India. After this interval the skies again become clouded, and about Christmas, or shortly after, the gentle but soaking rain of the cold weather sets in, and is repeated at intervals up to the end of March. It is evident, therefore, that the two currents, monsoon, and anti-trade, are totally unconnected with each other; and hence arises the desirability, especially in a question like the present, in which its secular variations are being discussed, of completely separating the rain of the former from that of the latter vapour current. I cannot but think that it is his omission to allow for these vapour currents that has led Dr. Hunter to offer such an erroneous explanation of the results obtained. According to him, copious precipitation should take place in the interval (October to December) between the two seasonal falls, during which clear weather is invariably present. It may be added that the period (December to April) which I took to comprise the winter fall, commenced after this interval.

The real explanation of the direct variation of the summer, and the inverse variation of the winter rainfall of North India, with the sun-spot period, is the hypothesis which first led to its verification coincidently and independently, by Mr. Hill and myself.

To enter upon a complete exposition of this hypothesis would occupy too much of your valuable space, but as it has been found to explain most of the anomalies which have hitherto proved such powerful obstacles (especially in extra-tropical regions) to the universal extension of the theory of sun-spot influence (I use the term advisedly) on the different elements of terrestrial meteorology, I will here briefly indicate its general outlines for the benefit of other workers in the same field of investigation.

The hypothesis, to start with, assumes *the solar radiation to vary inversely with the sun-spot frequency*.

It then takes account of the probable effects of such a variation upon the vapour-bearing currents throughout the globe with respect to velocity, direction, season, and latitude. According as trade, anti-trade, monsoon, or anti-monsoon, prevail (1) at different places at the same season, (2) at the same place at different seasons, so will specifically distinct effects arise both from the amount of vapour brought and its conditions of precipitation, to determine which, not only the general conditions introduced by latitude and season, but the local and peculiar meteorological functions of the region must be carefully studied.

Now as the principal effect of a secular change in solar radiated heat must be to cause a similar direct secular change in the normal convection currents of the atmosphere, we may expect the tropical trade-wind and monsoon regions to furnish us with some evidence, whether direct or indirect, in favour of the above hypothesis.

Little direct evidence has at present been adduced besides that given by Mr. Hill from a comparison of wind velocity in the N.W.P. (NATURE, vol. xvii. p. 505). A good deal of indirect evidence, however, is furnished in the monsoon regions by the occurrence of abnormal droughts and floods in contiguous districts (the drought in the N.W.P. and floods in Assam and Burmah last year were good examples of this kind) at the time of minimum sun-spot, when the velocity of the current being increased it travels in a more contracted channel, and, by a more equable distribution of rain at the time of maximum sun-spot, when the velocity of the current being decreased, it is more liable to extend laterally. In the trade-wind regions similar evidence is furnished by the fact of a deficiency of rain and cyclones at the time of minimum sun-spot, with a corresponding excess of both at the time of maximum sun-spot. The augmented velocity of the wind currents at the former epoch, preventing the formation of local areas of condensation and precipitation, and therefore (according to Messrs. Blanford and Eliot's theory of cyclone generation) of cyclones and their accompanying downpours; while the diminished velocity at the latter epoch favours the same.

Finally, the anti-trade which in its seasonal shifts north and south traverses the entire temperate zone, in the winter bringing rain to North India, Palestine, Madeira, California, &c., and in the summer to Northern Europe and Siberia, should give signs of a secular change in intensity and humidity, corresponding according to the hypothesis inversely with the sun-spots. In the summer, when large continental areas like Europe are more immediately under the direct influence of solar heat, local convection currents being set up will tend to disturb and complicate the effect of any general change in the strength of the anti-trade. In the winter, on the other hand, the obliquity of the incidental solar rays leaves the anti-trade in undisputed possession of the field. At this season, therefore, there should be a marked variation in the rainfall of the temperate zone, more particularly in those regions between 25° and 40° N. and S. lat., where the rainfall of *this season* is the chief rainfall of the year, corresponding *inversely* with the sun-spots. Even in those regions where the rain falls at all seasons, if we pick out the winter from the total annual falls, as was done by Mr. Draper, for New York (NATURE, vol. xvii. p. 15) in accordance with Mr. Hill's admirable suggestion (vol. xvi. p. 505), the results favour the hypothesis. But they do this in a far more marked manner where the rain of the entire year falls during the winter months, as in the Mediterranean and at Jerusalem, which have consequently hitherto been considered by Dr. Jelinek and Mr. Meldrum to afford strong evidence against the theory of a direct connection between rainfall and sun-spots. The inverse variation of the winter rainfall of Northern India is only another example of the same law, and shows how extremely important it is to analyse the seasonal variations separately before deciding the question by a mere cursory glance at the *total annual* falls. The apparent anomalies which Dr. Hunter finds presented in the North American rainfalls are, I think, due to his having compared the total annual falls. If he and other investigators will only take the hint dropped by Mr. Hill, and which I cordially endorse, of comparing the seasonal falls separately, they will find, I think, that while the summer rainfalls of the temperate zone show either a non-periodic variation, or symptoms of one coinciding directly with the sun-spots, the *winter* falls will in general show unmistakable signs of a variation coinciding *inversely* with that of sun-spot frequency and area.

E. D. ARCHIBALD

Sun-spots and Rainfall

I HAVE read with much interest Dr. Meldrum's paper on Sun-spots and Rainfall in NATURE (vol. xvii. p. 448), particularly that part of it in which Dr. Hunter's method of discussing the rainfall of Madras is criticised, and a method of inquiry in sun-spot researches is proposed. This method is, so far as I am aware, a new one, and as such, is deserving of careful examina-

tion as to how far it is applicable to the data submitted for discussion.

Dr. Hunter published the data for discussing the rainfall at Madras during the six sun-spot cycles, ending 1876, these being all the available data for Madras. As regards the sun-spots, we certainly have no *positive* data earlier, at least, than these cycles, whatever value may be attached to the approximate earlier figures supplied by Dr. Wolf. As regards, therefore, both the elements under discussion, viz., the sun-spots and the rainfall, the period discussed by Dr. Hunter represents the whole of the cycles for which material is available.

In dealing with this period, Dr. Hunter divides it into six equal cycles of eleven years each, this being substantially the average duration of the sun-spot cycles. I have arranged the relative numbers published in Wolf's last list (*Wolf. Astronomische Mittheilungen*, pp. 35-37), according to the cycles adopted by Dr. Hunter, with the result that all the six minimum years of sun-spots occurred either in the first year of the cycle, or in one of the immediately adjoining ones on either side of it, viz., in the second or in the eleventh years. As regards the years of maximum sun-spot, five out of the six occurred in the fifth or sixth years of the cycle, and the remaining year of maximum sun-spots occurred in the eighth year.

In his paper Dr. Meldrum states that as the sun-spot cycles are not all of the same length, it is evident that, by starting from any one year and going backwards over a long period, always using the same fixed number, a maximum and a minimum year might fall into the same group, and it was to obviate the occurrence of this contingency which the above analysis of Dr. Hunter's method shows did not occur during the period discussed by him, that Dr. Meldrum has proposed his new method as a more accurate mode of discussing the data.

To test the value of this new method of inquiry, I have arranged Wolf's relative numbers of sun-spots in accordance therewith, the maximum year of sun-spots of each cycle being placed in the sixth year, the minimum years being marked with an asterisk, and the "mean cycle" of eleven years being calculated from the thirteen years in the manner described by Dr. Meldrum :—

Year.	1811-23	1824-36	1832-44	1843-55	1855-67	1865-77	Means	Mean cycle.	Year of cycle.
1	1·6	8·1	26·3	*13·1	7·7	31·4	14·7		
2	4·9	16·2	*9·4	19·3	*5·1	14·7	11·6	14·9	1
3	12·6	35·0	13·3	38·3	22·9	*8·8	21·8	25·4	2
4	16·2	51·2	59·0	59·6	56·2	36·8	46·5	48·8	3
5	35·2	62·1	119·3	97·4	90·3	78·6	80·5	77·0	4
6	46·9	67·2	136·9	124·9	94·8	131·8	100·4	91·1	5
7	39·9	67·0	104·1	95·4	77·7	113·8	83·0	83·0	6
8	29·7	50·4	83·4	69·8	61·0	99·7	65·7	65·6	7
9	23·5	26·3	61·8	63·2	45·4	67·7	48·0	49·0	8
10	16·2	*9·4	38·5	52·7	45·2	43·1	34·2	34·6	9
11	6·1	13·3	23·0	38·5	31·4	18·9	21·9	24·6	10
12	3·9	59·0	*13·1	21·0	14·7	11·3	20·5	22·5	11
13	*2·6	119·3	19·3	7·7	*8·8	*7·0	27·5		

It will be seen from this table that with this arrangement the year of minimum sun-spots has occurred on the tenth, twelfth, thirteenth, first, second, and third years. By Dr. Hunter's arrangement the minimum years fell within a compact group of three consecutive years out of a cycle of eleven, whereas by Dr. Meldrum's arrangement they are scattered over seven years out of a cycle of thirteen. Further, I find that in the second cycle what is virtually a maximum year (viz., 1836 with 119·3 of sun-spots) fell within his minimum group, or in the thirteenth year. This is precisely the result which the method was designed to avoid, but as to the occurrence of which there was not an approach under Dr. Hunter's arrangement.

Again, if the same relative numbers of Wolf be arranged as Dr. Meldrum proposes, so that the year of minimum sun-spots of each cycle be placed in the ninth year of the thirteen years, it will be found that the maximum years are scattered over the twelfth, thirteenth, first, second, third, and fifth years of the series. By Dr. Hunter's method of arrangement five out of the six maximum years fell in the fifth and sixth years of the series, while the remaining one fell in the eighth year, thus again presenting a compact group, whereas Dr. Meldrum's method scatters them over more than half of his series of thirteen years.

An objectionable feature of this new method is the necessary repetition of figures which it involves. Thus, in the table given above, embracing six cycles, nine minimum years occur; and in the table in which all the minimum years are so arranged as to stand in the ninth year of the cycle, nine maximum years also occur, so that if the Madras rainfall were discussed by this method, the averages would be computed from tables in which the maximum and minimum years occur eighteen instead of twelve times.

Mr. Meldrum's method might be improved if he entirely struck out the first and thirteenth years of the thirteen years series, and simply "bloxamed" the remaining eleven years for the years of his "Mean Cycle;" that is, made the first of these years the mean of the eleventh, first and second; the second year the mean of the first, second and third. Even, however, with this change the method is inferior to that employed by Dr. Hunter, and the force of this statement will be the more readily recognised if it be kept in mind that we have no *positive* data from which the relative numbers of the sun-spots can be calculated prior to the time when Schwabe began his great work of sun-spot observation.

Edinburgh, April 22 ALEXANDER BUCHAN

Trajectories of Shot

MR. NIVEN was perfectly welcome to make use of my experiments and tables, as he has done, in trying to devise new methods of calculating trajectories of shot. And when he had satisfied himself that his methods possessed some advantages over others, he required no excuse whatever for their publication. But I altogether object to Mr. Niven's rule for finding v_β being connected in any way with the mode of calculation adopted by me. I beg, therefore, to place side by side Mr. Niven's rule, to which I object, and my rule, which I make use of, and so leave the matter. Mr. Niven says respecting v_β :—

"The first steps in our work must be to guess at it. The *practised calculator* can, from his experience, make a very good estimate. Having made his estimate he determines k. He uses the value of k in equation (a), and if he gets the velocity he guessed at, he concludes that he guessed rightly, and that he has got the velocity at the end of the arc. If equation (a) does not agree with him he makes another guess, and so on till he comes right."

The following is the course I pursue to find v_β. Refer to the table of coefficients and take out the value of k_a corresponding to the initial velocity v_a. Substitute in equation (a) and find a *first* approximate value of v_β. Now determine the mean value of k between v_a and v_β just found, substitute in equation (a), and thus find a *second* approximate value of v_β, which will generally be found sufficient. Otherwise adjust by proportional parts.

In this way the value of v_β is found accurately on the supposition that k has remained constantly at its mean value between v_a and v_β. Here the operations are of the simplest kind, and no guessing or practised calculator is required. And with a view to diminish the tedium of making these calculations, tables of $\Sigma\,(k)$, $\Sigma\,(k \div g)$, $(1000 \div v)^3{}_a$, &c., have been calculated and printed, but their publication has been delayed on account of the experiments proposed to be made with low velocities.

Since Mr. Niven described the process of guessing as "*extremely* dangerous," there can be no doubt that the epithet was "extreme." As I supposed, he is not prepared to supply me with a single practical case where his condition of danger is satisfied. And if a case cannot be found then the objection falls to the ground. Whether we consider the range of values of k for spherical or ogival-headed shot, for velocities above 1,200 f.s., we shall find that $\frac{dk}{dv}$ lies between the limits o and $-$ 0·09, or, where k is a mean over an arc, between o and $-$ 0·05 about. And it is the smallness of this tabular value which renders it difficult, if not impossible, to satisfy Mr. Niven's condition of danger. But if this quantity had not been small, then the cubic law could not have been used even approximately. Mr. Niven is at liberty to take shot of any size used in practice, moving at any attainable velocity beyond 1,200 f.s., and the coefficients of resistance for either spherical or ogival-headed projectiles. The objection is Mr. Niven's, and he must take the onus of supporting it if he still thinks it of value.

I regret to have to write anything in opposition to Mr. Niven's paper, because in all other respects it appears to me a valuable contribution to the science of ballistics. F. BASHFORTH

Minting Vicarage, April 17

"Mimicry" in Birds

If Mr. Young will look at the fourth edition of Yarrell's "British Birds," he will find that the fact he mentions (*ante*, p. 486) has already attracted notice, for he will there read (vol. i. p. 616):—

"In places near Thetford, where the ringed plover is common, skylarks often imitate the note of that bird, making it part of their own song." ALFRED NEWTON

Magdalene College, Cambridge, April 19

Our starlings here, which are a numerous and accomplished colony, have acquired the following notes of other birds:—Curlew, red-shank, blackbird, thrush, magpie, swallow, swift, chaffinch, house sparrow, hedge sparrow. The most successful imitations are those of the curlew, red-shank (the note uttered by the latter on taking wing), and the swallow. I have several times this year been certain that I heard a swallow twittering on the house-top, and found that the note proceeded from a starling.

The jays also in this neighbourhood, which are very plentiful, are very able mimics; the note of the carrion crow is about their most successful effort. H. H. S.

Riding Mill-on-Tyne, April 22

The Westinghouse Brake

Under the heading, "Notes," in Nature, vol. xvii. p. 140, there is a paragraph describing the automatic brake of the Westinghouse Brake Company, St. Stephen's Palace Chambers, Westminster, the latter part of which refers to a ball which performs certain functions under different circumstances. In a previous account in the *Times*, three balls are mentioned as being used in the experiment; it also states that several gentlemen were investigating the mathematical principles under which these functions fell. I have not seen any results of their work, neither is there any comment upon it in Nature. I therefore take occasion to mention it, in order that if any account of it has passed me, I may be informed of it, or that, if no results have appeared, this may lead to the subject being investigated by some of the mathematical correspondents of your esteemed paper. G. O. K.

Sound and Density

Since velocity of sound does not vary with *density* (Balfour Stewart, Chap. IV., "Elementary Physics"), would you kindly state the answer that should be given to the question, *Why* does sound travel quicker in *water* and *wood* than in *air*, and what is the relation between *density* and *velocity of sound* in *water*, *wood*, *air?* J. Cameron

The Academy, Montrose, April 18

[The velocity of sound depends on the ratio between the mass and the elasticity, and in air (to which Prof. Stewart refers) it does not vary with the density of the air if its temperature only remain constant. In this case the denser the air the greater the mass, but the greater the elasticity in the same proportion. The ratio between mass and elasticity is thus unaltered, and therefore the velocity remains under these conditions the same.—Ed.]

OUR ASTRONOMICAL COLUMN

The Transit of Venus in 1882.—In addition to independent calculations of the circumstances of this phenomenon, founded upon Le Verrier's tables of the sun and planet, to which reference has already been made in Nature, we have to record the publication of two memoirs upon the same subject, the first by Herr Bruno Peter, who is attached to the Observatory at Leipsic, the second by Dr. Karl Friesach, of Graz, which has been received within the last week. As was to be expected where practised calculators are working upon the same data, the direct results from the tables are in very close accordance with those previously published; indeed the advantage of so many repetitions of such work is not very evident. The differences which the calculated times of the geocentric contacts exhibit are almost wholly due to the employment of different semi-diameters of sun and planet. Le Verrier suggested (*Annales*, vol. vi. p. 40) that for the present the values to be employed should be respectively 958″·424 and 8″·305 for the mean distance. Herr Peter has used 961″·21 and 8″·472, and Dr. Friesach, 960″·0 and 8″·305. Their results for Paris mean times of contacts and least distance of centres are subjoined:—

Transit of Venus, 1882, *December* 6.

	Peter. h. m. s.	Friesach. h. m. s.
First external contact ...	2 4 21·4	... 2 4 52·8
,, internal ,, ...	2 25 3·9	... 2 25 11·6
Last internal ,, ...	8 1 56·5	... 8 1 42·6
,, external ,, ...	8 22 39·0	... 8 22 1·6
Least distance of centres ...	5 13 29·9 641″·7	... 5 13 27·3 641″·5

Encke's Comet in 1878.—Observers in the southern hemisphere may be reminded that this comet is likely to be a pretty conspicuous telescopic object in their evening sky, in the first days of August. According to Dr. von Asten's latest researches on the motion of this comet, the period of revolution at the last perihelion passage was 1200·8 days, which, without taking any account of perturbations (not likely to be very material during the present revolution), would bring it again to perihelion on July 27. Mr. Tebbutt, of Windsor, N.S.W., has once found Encke's comet without assistance beyond his own calculations, but it will probably be Dr. von Asten's intention to furnish southern observers with a reliable ephemeris commencing with August next. Observations will not be practicable before the perihelion passage, the comet being too near to the sun's place.

The "Berliner Astronomisches Jahrbuch" and the Minor Planets.—The volume of this ephemeris for 1880 has just appeared under the joint editorship of Professors Förster and Tietjen. The general contents are similar to those of preceding volumes. The ephemeris of the moon is again transferred, with full acknowledgment from the *Nautical Almanac*, and a great amount of labour of computation is thereby saved, which is made to tell upon the specialty of the work, the preparation of ephemerides of the small planets as far as their orbits are sufficiently determined. The reader who may be in search of the elements of these bodies will find in this new volume of the *Berliner Jahrbuch* the most complete and reliable table yet in the hands of astronomers. It includes orbits of all the minor planets to No. 172, with the exception of No. 155, *Scylla*, for which the necessary materials for calculation are wanting; and while referring to *Scylla*, it may be remarked that the four observations on November 8, 9, 22, and 23, 1875, cannot be represented by an elliptical orbit, which raises a suspicion that those of November 8 and 9 may belong to one planet, and those of November 22 and 23 to another, not, so far, recognised in the list. On examining the table of elements it is seen that No. 153, *Hilda*, has by far the longest period, while No. 149, *Medusa*, is credited with the shortest, according to the calculations of Prof. Tietjen. The observations of *Medusa*, however, extend over a period of eight days only, but they appear very exact, and it has happened that from a similar short interval of accurate observation, very close approximation to the true elements of an elliptical orbit has been attained; we may especially note the case of the short-period comet of De Vico in 1844: from eight days' very precise observations, M. Faye deduced an orbit which, as was pointed out by Prof. Brünnow, was almost identical with the result of his own elaborate investigation of the elements from the whole extent of observation. It is unfortunate that *Hilda* has escaped observation at the last opposition, since of all the small planets it is most desirable to keep this one in view, from the fact of its orbit allowing of a very much closer approach to the planet Jupiter than is possible in the case of any other.

The best orbit is by Kuhnert, but it is probable that the want of observations in 1877-78 is not wholly attributable to errors of elements, but in a certain degree to the position of the planet at a considerable distance from the ecliptical belt of the sky for which charts of small stars are as yet published, and, in addition, to its faintness. *Medusa*, which appears to have a very small inclination, may possibly be recovered in the ensuing summer.

GEOGRAPHICAL NOTES

TASMANIA.—The prospects of Tasmania are reported to be improving, owing to the development of its mineral resources. Very large quantities of tin, as is well known, have been discovered at Mount Bischoff, and quite recently the vast tract of country to the north-west, which has always been looked upon as valueless, has been explored with more care, and though it is probably of little use for agricultural purposes it has been found to contain enormous quantities of iron and other minerals. Traces of gold have been met with in the beds of some of the rivers, and copper has also been found. In the dense forests which are common in this region specimens of the *Eucalyptus* have been seen which are said to be more than 300 feet in height. Further exploration is still being carried on with a view to the accurate determination of the capabilities of this part of Tasmania.

AFRICAN EXPLORATION.—Journalistic enterprise is again contributing to the exploration of Africa, and this time Paris is vying with London and New York. M. P. Soleillet, who has been equipped under the auspices of the *Moniteur Universel*, departs soon for a tour of exploration in Equatorial Africa, to follow in the footsteps of his fellow-journalist Stanley. The development of openings for French commerce is to form a prominent feature in the undertaking.

PARIS.—The Paris *Société de Géographie* has elected Baron de la Roncière Le Nourry its president for the ensuing year.

FRENCH GUAYANA.—Dr. Crevaux, who was sent out by the French government to explore the interior of French Guayana, has returned to Paris after completing one of the most arduous journeys in the annals of South American discovery. After having fulfilled his instructions to penetrate to the Tumuc-Humac range, he determined to make the passage of these mountains, and descend into the valley of the Amazon, an attempt which has several times been tried in vain during the past three centuries. Although deserted by all his attendants, with the exception of a negro, he succeeded, after overcoming numerous obstacles, and battling with famine during a march of sixteen days across an uninhabited tract, in reaching the head waters of the Yary, from whence a canoe-voyage brought him to the Amazon. Of the 500 leagues traversed in this journey, 225 were hitherto completely unknown.

SURVEY OF NEW YORK.—The Second Annual Report of the State Geographical and Topographical Survey of New York, in charge of Mr. James T. Gardner, gives an account of the labours of the commission during the year. The principal work of the year has been the precise determination by primary triangulation of points in eleven counties, embracing an area of about 3,000 square miles; 167 points were located in an area of 1,700 miles in seven counties alone; the average has been one to every ten square miles. Fifty-one monuments have been placed in defining the boundaries of counties, this being a very important part of the work of the survey. The report is accompanied by several maps showing the progress of the work, the position of the stations, &c.

BIOLOGICAL NOTES

A NEW FRUIT.—Mr. Hollister has introduced from Japan to San Francisco a fruit, which is said in its native country to have as many varieties as are grown in this country of our apple, and the sweetness of the fruit is more or less retained by all of them. It is known as the Japanese Persimmon and, according to Mr. Hollister, is the most beautiful of all the fruits he had ever seen and the most delicious to the taste—four of the fruits which ripened with him weighed three quarters of a pound each, they were of a rich yellow colour, and looked like balls of wax; these were pronounced equal to a good pear or peach. The tree is a highly ornamental one, a prolific bearer, and as hardy as a pear. Its fruit season is from October to March. It seems quite adapted to the soil and climate of California. The grafted trees bear in four years. The seedlings require double that time, and are not reliable (*Proceedings*, Acad. of Science, California, in *American Naturalist* for March, 1878). This is the well-known and beautiful fruit of *Diospyros kaki*, Linn., fil., a near ally of the Persimmon of the Southern United States of America. Mr. Hiern tells us in his Monograph of the Ebenaceæ that the Chinese preserve this fruit with sugar, and that it has for a long time been in cultivation with them and the Japanese. The fruit has a thin skin, with a sweet orange-scarlet coloured flesh, with six or eight dark smooth seeds. It was beautifully figured in the *Gardeners' Chronicle* for 1872.

FOSSIL INSECTS.—Messrs. S. H. Scudder, of Cambridge, and F. C. Bowditch, of Boston, attached to Hayden's United States Geological and Geographical Survey, spent two months in Colorado, Wyoming, and Utah, in explorations for fossil insects and in collecting recent coleoptera and orthoptera, especially in the higher regions. They made large collections of recent insects at different points along the railways from Pueblo to Cheyenne, and from Cheyenne to Salt Lake, as well as at Lakin (Kansas), Garland and Georgetown (Colorado), and in various parts of the South Park and surrounding region. Ten days were spent at Green River and vicinity in examining the tertiary strata for fossil insects, with poor results. The tertiary beds of the South Park yielded only a single determinable insect; but near Florissant the tertiary basin described by Mr. Peale in one of the annual reports of the Survey was found to be exceedingly rich in insects and plants. In company with Rev. Mr. Lakes, of Golden, Mr. Scudder spent several days in a careful survey of this basin, and estimates the insect-bearing shales to have an extent at least fifty times as great as those of the famous locality at Œningen in Southern Bavaria. From six to seven thousand insects and two or three thousand plants have already been received from Florissant, the specimens from this locality being remarkable for their beauty. There is every reason to believe that the tertiary strata of the Rocky Mountain region are richer in remains of fossil insects than any other country in the world, and that within a few months the material at hand for the elaboration of the work on the fossil insects of the American tertiaries which Mr. Scudder has in preparation, will be much larger than was ever before subject to the investigation of a single naturalist. Mr. Scudder has in all now more than 12,000 specimens of fossil insects.

THE CLIMBING OF THE VIRGINIA CREEPER.—Mr. B. D. Halsted has studied the mechanism of climbing in the Japanese Ampelopsis, and finds that the clinging discs terminate tendrils which are homologous with main stems. While approaching a support, these discs flatten themselves on the inner side. The surface of the disc is papillose, and excretes a sticky substance; and the irregular contraction of the tendril draws the vine to its support (*Proc.* Boston Soc. Nat. Hist., January, 1878).

THE EARLIEST CHANGES IN ANIMAL EGGS.—The patient researches of van Beneden, Grieff, and Oscar Hertwig have discovered many interesting facts in the structure of simple ova when laid, the mode of fertilisation, and the first segmentation. Oscar Hertwig's last observations are on the starfish Asteracanthion (*Morphologisches Jahrbuch*, vol. iv. Part I.), and he describes the changes as follows:—The germinal spot of the unfertilised ovum first shows a separation into two portions, while part of the germinal vesicle contributes material out of which first one and then a second "directive corpuscle" is formed. By this time the germinal vesicle is undistinguishable, having left a small portion as the ovinucleus (*eikern*). When fertilisation takes place, the spermatozoon gives rise to a small body, the sperm-nucleus (*spermakern*); this body approaches the ovinucleus, and they fuse to form the segmental nucleus (*furchungskern*); this precedes the division of the whole egg into two cells. If such observations are extended to many species and confirmed by other observers, we shall have an important gain in our knowledge of the results of fertilisation.

GLACIAL AND POST-GLACIAL FISHES OF NORWAY.—We learn from the Danish *Naturen* the appearance in the third part of the *Nyt Magazin for Naturvidenskaberne*, of a paper, by M. Robert Collett, on the glacial and post-glacial fishes of Norway. These fishes, which are most perfectly preserved in chalk-lumps, the outer shapes of which more or less perfectly exhibit the outer shapes of the included fishes, are found in clay deposits some 360 feet above the sea; the fishes belong all to the existing fauna, displaying at the same time their Arctic and North Atlantic origin. Out of twelve species, described by the authors, the most common is the *Mallotus villosus*, which is found everywhere; one species, the *Clupea sprattus*, is worthy of notice, because it is now a native of more southern waters.

POACHING BIRDS.—Mr. N. B. Moore has made observations at the Bahamas on the *Certhiola flaveola*, which obtains nectar from the flower of *Verea crenata* by thrusting its bill at once through the petals into the nectary. It is only after the bird has made an opening that small black ants and other small insects are found in the nectary. But these birds also poach on the woodpecker's preserves. One day Mr. Moore observed a *Picus varius* extracting sap from a logwood sapling, and as the woodpecker flew away, two Certhiolæ appeared, perched near the sap-pits from which the juice was oozing, and by cunningly thrusting in their penicillate or bristle-tipped tongues, commenced to lap or suck the fluid into their mouths. This practice was constantly observed afterwards. Mr. Moore fixed the bowl of a teaspoon in a fork of the same tree, and placed some strained honey in it. In three days the Certhiolæ found this, and commenced to feed on it. They were followed by another bristle-tongued bird, *Dendrœca tigrina*, and other species, who also attacked the woodpecker's sap-pits. These are interesting instances of apparent intelligence on the part of birds (*Proc.* Boston Soc. Nat. Hist., January, 1878).

GEOLOGICAL TIME[1]

IF a rigid body be in rotation about an axis of symmetry it will continue to rotate about that axis, but if it be set spinning about an axis inclined to that of symmetry the axis about which it spins will be continuously displaced relatively to the body; in other words, it will wabble.[2] This wabbling is well illustrated by the motion of a top whilst it is "going to sleep."

As the rotating body approaches more and more nearly the spherical shape, so does the wabbling become slower and slower. If the earth, which is nearly spherical, were to wabble in its diurnal rotation it would do so in about 305 or 306 days.

Dr. Haughton has lately published[1] an ingenious speculation, founded on the possibility of the wabbling of the earth, in which he seeks to determine limits to the duration of geological time from the observed absence of any motion of this kind.

The object of the short paper, of which I am here giving an account, was to combat the applicability to the case of the earth of Dr. Haughton's results.

The method pursued by him may be shortly described as follows:—If a continent were to be suddenly upheaved the earth's axis of figure (or strictly speaking, the principal axis of greatest moment of inertia) would be displaced from its previous position; immediately after the earthquake, the axis of rotation being where it was just before the earthquake, is no longer coincident with the axis of figure, and therefore a wabble is set up in the earth's motion. If it were not for frictional resistances that wabble would continue for ever after. But it is easy to see that, as the ocean is not rigidly connected with the earth, a tide of 306 days period would be set up. This tide would then rub on the sea-bottom, and would gradually reduce the wabble and bring the earth "to sleep" again like a top.

By reference to the estimate of Adams and Delaunay of the effects of tidal friction in retarding the earth's rotation, Dr. Haughton endeavours to find a numerical value for the frictional effect of such a 306-day tide as above explained. He then finds how long it would take to reduce a wabble of given amount to one of any smaller amount.

In a previous paper he had already shown that the elevation of the continents of Europe and Asia must have shifted the earth's axis of figure by 69 miles at the earth's surface. If, therefore, such an elevation took place suddenly, it must have started a wabble, in which the axis of rotation described a circle of 69 miles radius round the axis of figure.

But Dr. Haughton is of opinion that astronomical instruments are now so perfect, that a wabble of 5 feet in radius would be detected, and that it is not, therefore, permissible to suppose that the present actual wabble has a radius of even 5 feet. His numerical calculations, then, show that it would take 641,000 years to reduce the radius from 69 miles to 5 feet by means of the tidal friction, and he, therefore, concludes that, if Europe-Asia were manufactured *per saltum*, that event cannot have taken place less than 641,000 years ago, and that it may have been at a much more remote epoch.

The improbability of this supposition induces him to consider the case of elevation by 69 geological convulsions, each of which displaced the axis through one mile, and where the radius of the wabble is reduced to five feet between two successive convulsions. He here finds that the elevation of Europe-Asia must have occupied $27\frac{1}{2}$ million years, and that no geological change altering the position of the earth's axis through one mile can have taken place within the past 400,000 years.

He lastly supposes that the wabble has a radius of 5 feet, and that the geological changes take place at such a rate that the increase of the radius is exactly destroyed by friction during each wabble, so that the radius of 5 feet remains constant. On this supposition he finds that the time required was 4,170 million years.

Now it appears to me, from this method of treatment, that Dr. Haughton is of opinion that a second earthquake of elevation following a first would necessarily increase the radius of the wabble. For if not, why does he postulate a lapse of time between successive earthquakes, and in the last case make the supposition of the increase of radius be exactly destroyed? It is on this point

[1] Abstract of a paper read before the Royal Society on March 14.
[2] I follow Dr. Haughton in the use of this very expressive word.

[1] Notes on Physical Geology, No. III., *Proc. Roy. Soc.*, vol. xxvi. p. 534.

that I venture to differ from him, for it seems to me, from dynamical considerations, that a second equal impulse following a first, at some time within 306 days, might either double the wabble, alter its amount, or annihilate it, according to how it was timed to take place.

If I am correct in this view, I cannot but think that the estimate of geological time falls to the ground. For even if the elevation of continents took place impulsively, we can have no possible data for judging of how the earthquakes were timed with reference to the position of the axis of rotation, and unless they were properly timed the radius of the wabble could not increase; and the increase of the radius is, I imagine, essential to Dr. Haughton's method.

But if we set aside the impulsive theory of elevation, the work contained in my paper, "On the Influence of Geological Changes on the Earth's Axis of Rotation,"[1] will be applicable; for I there considered the effects of a slow continuous elevation of continents. In that paper I show that such a mode of elevation would set up a wabble of 306 days' period in the earth's motion. But this wabble is of quite a different character from that contemplated by Dr. Haughton, for it is unsymmetrical, so that the axis of rotation coincides with the axis of figure every 306th day.

By a very simple application of a formula given in that paper, it will be found that, supposing the continuous elevation to take place at such a rate that the axis of rotation is 5 feet distant from that of figure when at its greatest distance, then the axis of figure must be travelling with reference to the solid earth at the rate of $\frac{3}{16}$ of a second of arc per annum. Thus, in 19,200 years it will have travelled over 1° or 69 miles. That is to say, Europe-Asia might have been elevated in 19,200 years without the axis of rotation ever having described a circle of more than 5 feet in *diameter*. If the elevation were then to stop suddenly a symmetrical wabble would be set up (such as that considered by Dr. Haughton), and the *radius* of this wabble could not be greater than 5 feet, and might be zero, according to the exact time of the stoppage.

This investigation makes no reference whatever to the effects of tidal friction, and there are certain considerations which lead me to believe that even the above estimate of time might be largely reduced.

The conclusion at which I arrive therefore is that the elevation of Europe and Asia might have taken place in very much less than 20,000 years without leaving behind any wabbling in the earth's motion traceable by astronomical observations. Dr. Haughton's views, if generally accepted, are of the very greatest interest to geologists, and they therefore merit the strictest examination; as I have devoted a good deal of time to this subject I thought it might perhaps be useful to write this note. Should my present criticism be incorrect, there is little doubt but that it will meet its just fate of refutation.[2]

G. H. DARWIN

[1] *Phil. Trans.*, vol. 167, Pt. I.

[2] Since this has been in type Dr. Haughton has read another paper before the Royal Society, in which he concludes, from purely geological evidence, that "the hypothesis of a shifting pole (even if permitted by mechanical considerations) is inadmissible to account for changes in geological climates." Therefore whether he agrees or not in the justice of my mechanical criticism, he seems to be of opinion that the wabbling of the earth will not give geologists much light as to the duration of geological time.

EARLY ELECTRIC TELEPHONY

IN 1861 the first successful attempt at the construction of an electric telephone was made by Philip Reis, a teacher in a school at Friedrichsdorf, near Homburg. On October 26, 1861, Reis showed his instrument, which he termed a "telephone," to the Physical Society of Frankfort-on-the-Main; and on that occasion he succeeded in electrically transmitting various melodies, which were distinctly heard throughout the room. In the paper he read before this Physical Society, published in the annual report of the Society for 1861, Reis states:—"Melodies were sung, not loudly, into the transmitting apparatus placed in a hospital some 300 feet away from the audience, care being taken that no sound could be heard, by direct transmission, or by conduction along the wires. The sounds of various musical instruments were clearly reproduced, as the clarionet, horn, organ-pipe, and even harmonium and pianoforte when the transmitter was placed on their sound-boards, provided the tones were within the compass of *f* to F″. Articulation was not reproduced equally well. Consonants, however, were in general pretty clearly heard, but not the vowels." In this report, which is entitled "Telephony by Means of Electric Currents," Reis shows how he was led to the construction of his instrument by a study of the mechanism of the organ of hearing, and of the manner whereby sounds are perceived by the human ear, and he gives a series of diagrams representing the resultant curves that would be produced by the combination of various concords and discords. Thus, he was led to perceive that "if it were possible to create, in any manner, a mode of vibration whose curve resembles that of any tone or chord, then a sensation would be produced similar to that given by the tone or chord itself." This principle, he affirms, guided him onwards.

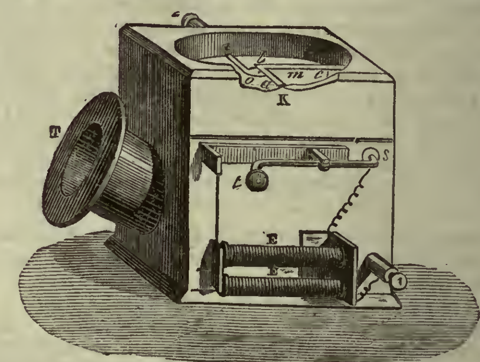


FIG. 1.—This and Fig. 2 show the usual but imperfect form of Reis's telephone. Fig. 1 is the transmitting apparatus. T represents the mouthpiece, *m* the membrane closing the upper portion of the box K, a portion of which is cut away to show the movable lever, *a b c*, resting by a projecting point *b*, on the platinum disc *o*, fixed to the centre of the membrane and connected with the binding screw 2. The arm *a b c* is metallicly connected with the binding screw 2. The key *t* closes the circuit when the instrument is in use, and the electro-magnet E E is for the purpose of receiving communications.

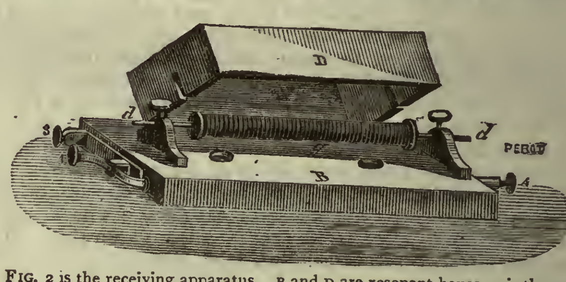


FIG. 2 is the receiving apparatus. B and D are resonant boxes, *g* is the coil through which the current passes magnetising the iron wire *d*, 3 and 4 are binding screws to which the line and return wire are attached, the circuit being closed by the key 1.

The first instrument he made was constructed of very homely materials. The bung of a beer-barrel was pierced through with a conical hole, the smaller end was then covered by a membrane, the skin of a German sausage being used for this purpose; to this was fixed, with a drop of sealing-wax, a little strip of platinum joined up to one

end of a small battery; a wire was adjusted near to, but not touching, the platinum strip; this wire led to the receiving instrument, and thence back to the other pole of the battery. On speaking into the conical orifice in the bung the membrane was thrown into vibration, and the attached metal strip coming into contact with the adjoining wire, momentarily completed the electric circuit. The vibrations of the membrane thus sent a corresponding series of intermittent currents into the receiver, which, in the first instance consisted simply of

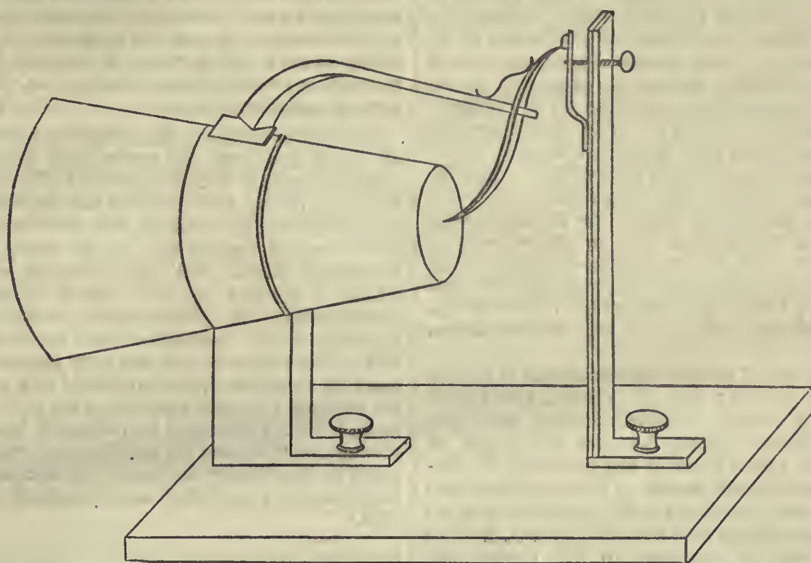


FIG. 3.—Sketch of improved form of transmitter made by Reis in his telephonic experiment (1862).

a knitting needle surrounded by a coil of wire, and placed on a violin to serve as a sound-board. Though Reis afterwards considerably improved upon his earlier instruments, the improvements do not seem generally known, and the arrangement just described is substantially that usually constructed and figured as Reis's telephone (see (Figs. 1 and 2.)

In Dingler's *Polytechnisches Journal*, vol. 169 (1863), p. 29, is a report on Reis's improved telephone by Legat, inspector of telegraphs in Cassel, &c. This report was originally printed in the *Journal* of the East German Telegraph Company for 1862. Considerable modifications are here shown in both transmitter and receiver. The membrane is formed of a collodion film and is not loaded with any metal

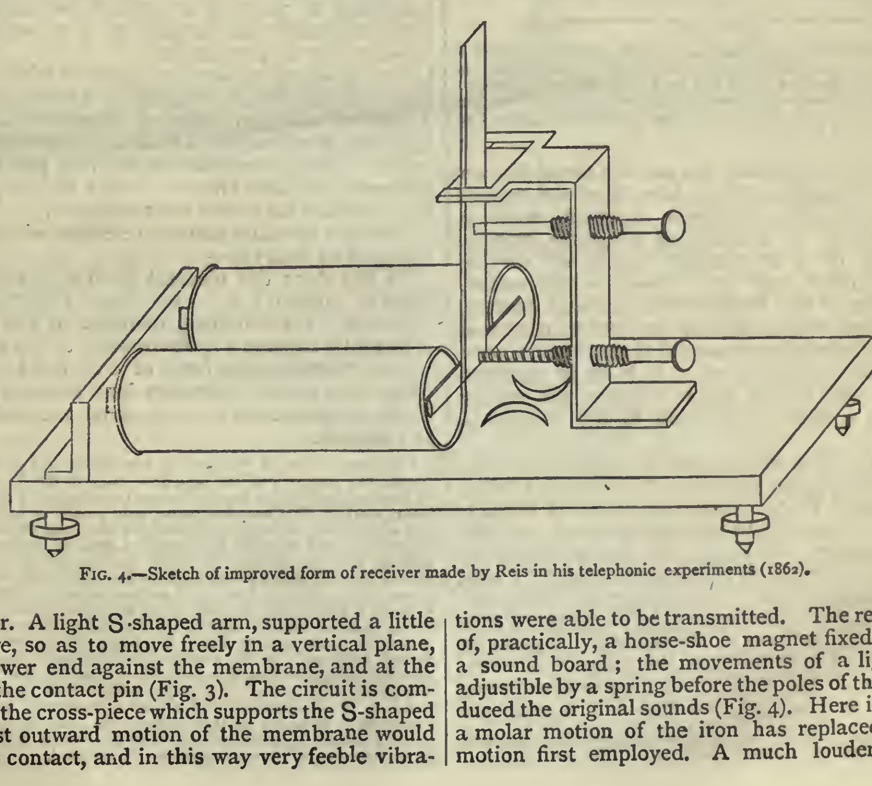


FIG. 4.—Sketch of improved form of receiver made by Reis in his telephonic experiments (1862).

contact-breaker. A light S-shaped arm, supported a little above its centre, so as to move freely in a vertical plane, abuts at the lower end against the membrane, and at the upper against the contact pin (Fig. 3). The circuit is completed through the cross-piece which supports the S-shaped lever; the least outward motion of the membrane would thus break the contact, and in this way very feeble vibrations were able to be transmitted. The receiver consisted of, practically, a horse-shoe magnet fixed horizontally on a sound board; the movements of a light iron keeper, adjustible by a spring before the poles of the magnet, reproduced the original sounds (Fig. 4). Here it will be noticed a molar motion of the iron has replaced the molecular motion first employed. A much louder sound is thus

obtained, and by bringing the iron keeper near to, or even into gentle contact with the magnet, every grade and rate of simple vibration could be reproduced, as the present writer is able to testify.

With this instrument Reis obtained better results and even transmitted imperfect articulation. Legat speaks of single words in reading and speaking being indistinctly heard; but any sudden modulation of the voice as in surprise, interrogation, &c., was clearly reproduced. Still more definite is the following statement, occurring in an article on Reis's improved telephone in No. 15 of Böttger's *Polytechnisches Notizblatt* (1863):—"The experimenters could even communicate to each other words, only such, however, as they had already heard frequently." In confirmation of this the present writer has received a letter from Dr. Messel, a name well known to chemists, who was a former pupil of Philip Reis and an eye-witness of his early experiments. Dr. Messel states—"There is not the shadow of a doubt about Reis having achieved imperfect articulation; I personally recollect this very distinctly and could find you many others who were witnesses of the same fact." [1]

As an interesting sequel to this historical note it should be mentioned that in 1865 Mr. S. Yeates, the skilful instrument maker of Dublin, introduced some modifications in one of Reis's instruments he had purchased, of the usual early form, which enabled him to obtain the distinct articulation of several words. The modifications were twofold: (1) the knitting needle receiver was replaced by an electro-magnet and movable keeper, as Reis had already done, though unknown to Mr. Yeates (see Fig. 5); and (2) a drop of very slightly acidulated water

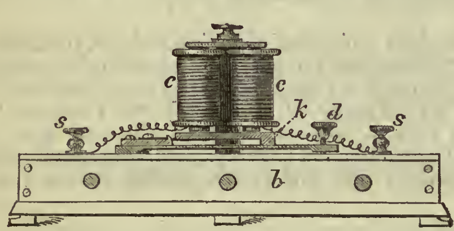


FIG. 5.—Yeates's receiver for Reis's telephone. Upon the sounding box *b* an electro-magnet *cc* is supported by the brass pillar seen behind. A light iron keeper *k* is fastened at one end by a steel spring to a wooden bridge, which can be raised or lowered by the screw *d*, so that the keeper can be brought almost into contact with the electro-magnet. The circuit is completed by the binding screws *s s*.

was placed between the contact pin and the metal disc on the membrane. The intermittent character of the current was thus abolished, and a very near approach made to the true principle of an articulating telephone, namely, the employment of a *continuous current* of varying strength. This instrument was shown in November, 1865, at a meeting of the Dublin Philosophical Society, and some members of that society who were then present have testified to their remembrance of the fact that several words were transmitted fairly well. It is to be regretted that at the time Mr. Yeates did not pursue the matter further, nor give a wider publication to the success he obtained.

But between the best of the results obtained by Reis and others in the direction of articulation, and the splendid achievements of Prof. Graham Bell, there is unquestionably a very wide step. In the sensitive and beautiful instrument discovered by Prof. Bell, the voice of the speaker generates thrills of magneto-electricity, which, being strictly proportional to the sonorous vibrations, reproduces the voice and its expression in the receiver in a fairy-like far-away whisper. Nevertheless it must be borne in mind that it is unlikely the telephone of the future will employ the voice to generate the driving power, but only to modulate the flow of a current obtained by coarser means. It is in this direction that Reis worked, and though his method was faulty in the employment of an intermittent current, the same cannot be said of the arrangements adopted by Mr. Edison, of New Jersey. And inasmuch as Mr. Edison has already discovered and brought to a practical issue such remarkable additions to our knowledge as quadruplex telegraphy, the electro-motograph, and the phonograph, we have, in these achievements, the earnest of success to those excellent telephonic investigations wherein Mr. Edison has already won an enduring fame.

W. F. BARRETT

[1] My best thanks are due to Dr. Messel for much information concerning Reis and for a reference to his papers in the journals alluded to.

P.S.—Since writing the foregoing article, the publication of which has been for some time delayed owing to the crowded state of the columns of NATURE, my attention has been drawn to a claim made by Mr. John Cammack, to be the first inventor of the electric telephone. From this it would appear that in the early part of 1860 Mr. Cammack made and exhibited an electric telephone, whilst a student in the Royal School of Medicine, Manchester. A photographic copy of the original drawing of the instrument has reached me, and so far as this goes it embraces not only the intermittent current used by Reis, but the principle of the continuous current of varying strength employed by Bell and Edison. In fact, if Mr. Cammack can furnish historical proof, the arrangement shown in his drawing, with its explanatory note, is identically the same as the method, long after independently invented and patented by Prof. Graham Bell.[1]

W. F. B.

ACTION OF LIGHT ON A SELENIUM (GALVANIC) ELEMENT

In the course of a series of experiments on the electrical behaviour of selenium, undertaken with a view to remove, if possible, the difficulties in the way of constructing constant resistances of this material, I have had occasion recently to investigate the effects of surface tension due to light.

I find that the action of light on crystalline selenium (annealed at 200° C.) is much more striking when the selenium forms one element of a galvanic couple than when it acts as a resistance.

The most convenient arrangement which I have found for observing this, is to make up a couple consisting of (1) a plate of selenium hanging suspended by means of a platinum wire, and (2) a strip of platinum foil, in distilled water. The potentials of the two poles are not very different, and any change in the electro-positiveness of the selenium is at once very apparent.

The first selenium-platinum element which I constructed behaved as follows:—

In the dark the element gave a steady electromotive force of about 0·1 volt, the selenium being *positive* to the platinum. On admitting daylight to the selenium plate it instantly became *electro-negative* to the platinum, showing an electromotive force of 0·05 volt in that direction. That is to say the selenium had become 0·15 volt more electro-negative by the action of the light than it was in the dark.

[1] Perhaps the word "claim" is too strong, as I observe Mr. Cammack speaks very modestly of the idea he so early sketched out. Such ideas are of course valueless in a practical sense, unless brought to the test of experiment, and this Mr. Cammack seems only partly to have done; this too is just where Prof. Bell succeeded; by his persistent experiments overcoming all obstacles and affording by the way a striking illustration that facts may after all upset the strongest *à priori* conclusions. In connection with this remark the following passage from the last edition of a well-known work on Mental Physiology (p. 632), is not without interest:—"Everyone who accepts as facts, merely on the evidence of his senses, or on the testimony of others who partake of his own beliefs, what Common Sense [with capitals] tells him to be much more probably the fiction of his own imagination—even though confirmed by the testimony of hundreds affected with the same epidemic delusion—must be regarded as the subject of a 'diluted insanity.'" Yet Baron Münchausen's trumpet has been outdone by the phonograph: the "fiction of imagination" by a fact "confirmed by the testimony of hundreds." However as these latter have "merely the evidence of their senses to offer," we presume they are all the victims of "a diluted insanity," if the reasoning of the eminent author be accepted.

After the first impulse this extreme electro-negativeness of the selenium, due partly to polarisation, gave way and it gradually passed again to the electro-positive side, where, after a few minutes, it settled to a constant value, but still electro-negative to its condition in the dark.

I found that the slightest shadow or other variation in the intensity of the light caused a considerable variation in the electromotive force of the couple and a consequent indication.

On excluding the light the selenium instantly increased in electro-positiveness, and soon settled to its original position.

A couple in which two plates of selenium were opposed to each other, light being excluded from one and admitted to the other, gave identical results, only the resistance of the element was much greater.

The effect of light, therefore, in modifying the surface tension of selenium is evidently to render it more electro-negative and presumably not more metallic, as has been suggested in explanation of its increased conductivity.

I am endeavouring to construct a combination of selenium elements which, with a mirror galvanometer and photographic arrangement may be used to give a trustworthy record of the intensity of daylight. The practical difficulties in the way at present I have hopes will not be insurmountable. ROBERT SABINE

NOTES

WE regret to notice the death, on the 18th inst., of Dr. Thomas Thomson, F.R.S., for some years Superintendent of the Botanic Gardens at Calcutta, and joint author of Hooker and Thomson's "Flora Indica." He was also a contributor to Sir J. D. Hooker's "Flora of British India," now in progress.

THE following are the names of those whom the Council of the Royal Society have recommended for election on June 6 next as appointed:—J. G. Baker, F. M. Balfour, Rev. T. G. Bonney, Prof. J. H. Cotterill, Sir W. Elliot, Canon W. Greenwell, T. Hawksley, C.E., J. Hopkinson, D.Sc., J. Hughlings Jackson, M.D., Lord Lindsay, P.R.A.S., S. Roberts, E. A. Schäfer, H. Sprengel, G. J. Symons, and C. S. Tomes.

THE scientific world has sustained a loss by the decease of the Rev. James Booth, LL.D., F.R.S., Vicar of Stone, near Aylesbury, which occurred on the 15th inst., at the age of seventy-one. He was educated at Trinity College, Dublin, where he obtained several prizes and graduated in honours. He was elected a Fellow of the Royal Society in 1846, to a very great extent in recognition of his earliest publication, "A New Method of Tangential Co-ordinates," and also as the inventor of a new system of parabolic trigonometry. In 1852 and 1853 he contributed to the *Philosophical Transactions* two memoirs on "The Geometrical Properties of Elliptic Integrals." He was also known as the contributor of several papers on mathematical subjects to the *Philosophical Magazine*, and not a few of these, we believe, have found their way into other languages. In 1859 he was presented to the living of Stone by the Royal Astronomical Society, to whom the advowson belongs.

DR. FREDERICK KAMPF, who has been the astronomer of Lieut. Wheeler's U.S. exploring expedition, died in Washington, on March 30, at the age of thirty-six. Dr. Kampf was educated at Bonn, and emigrated to the United States in 1870, securing a position in connection with the United States Coast Survey until 1873, when he joined the party of Lieut. Wheeler as already mentioned. He promised to attain to much distinction as an astronomer and observer, and his untimely death is much to be lamented.

THE collection of shells of the late Dr. P. P. Carpenter, of Montreal, is for sale. The opportunities of Dr. Carpenter for making this collection of desirable specimens were very great, especially from the great Reigan collection of Mazatlan shells, which he purchased many years ago, and after investigation deposited duplicate series in several museums in Europe and America. The collection embraces about 4,000 species and varieties, for the most part original types. The collection is deposited for the present in the museum of M'Gill College, Montreal.

DR. RUD. FALB, of Vienna, who is engaged in studying the earthquake region in South and Central America, has left Chile and announces his arrival at Arequipa. He intends to ascend the volcano of Misti, near Arequipa, which is some 17,600 feet in height. He also reports that at the southern part of the Peruvian coast the shocks of earthquake continue with unabated violence, and that a violent eruption occurred recently from the Cotopaxi Volcano in the Andes of Quito, without, however, doing much damage.

AN Ethnographical Congress will assemble in Paris on June 24, and continue for three days. The head-quarters of the Commission are rue Monsieur, 19.

The Chair of Surgery at the Collége de France, occupied by the late Claude Bernard, has been offered to Prof. Charrot. It has been decided to erect the statue of the distinguished physiologist immediately before the Collége de France.

DR. E. BAUMANN, one of the most promising of the young physiological chemists of Germany, has received a Professorship in the Berlin University.

WE notice the death of Prof. H. Girard at Halle, on April 12. He was, until a recent date, director of the Mineralogical Museum of the University of Halle, and his name is associated with several mineralogical researches, more especially in connection with the Stassfurt deposits.

AMATEURS of spectrum analysis will thank Mr. Browning for a little pocket case he is now selling which permits a study of absorption phenomena in a very satisfactory manner. Various substances, which give very characteristic band absorption, have been mixed with gelatine so as to form a thin transparent coloured film. In that we have received, roseine, eosine, cochineal, indigo, aniline blue, Hofmann's violet, and other colouring matters have been treated in this way. There are twelve differently coloured films in all, and the variations in the spectra are very striking. On holding the films horizontally close to the slit so that one film falls on the upper and the next on the lower part of it, a capital idea of the use of comparison spectra can be gained.

M. DABRY DE THIERSANT, a French *Chargé d'Affaires*, who has been instrumental in introducing a number of Chinese plants and animals into his native country, is now making arrangements for importation in quantities of the *setz*, one of the most valued fish found in Chinese waters. The fish belongs to the carp family, and when fed on sea-plants in ponds, attains with great rapidity a weight of about forty pounds. During the past three years experiments made on the fish in the Jardin d'Acclimatation have shown it to be well adapted to a European climate, and as it increases rapidly, it is hoped that within a few years it can be introduced extensively throughout Europe.

A LITTLE village in the neighbourhood of Draguignan, France, has lately been the scene of a remarkable subsidence which has attracted the curious from all directions. An elliptical tract of ground, containing over 10,000 square feet, sank gradually one day, accompanied by loud noises, until it left an orifice of over 100 feet in depth, with water at the bottom. Numerous trees and vines disappeared completely in the depths of the new lake. A similar depression on a smaller scale occurred in the same vicinity a century ago, and both the phenomena are attributed to the action of subterranean streams.

The Central Society of Agriculture of France took possession, on March 13, of a splendid hotel which has been built for its use and presented to it by M. Behague, one of its most active members. This society is one of the oldest in Europe, having been established more than a century ago, under the reign of Louis XV. It has never been suppressed or interrupted in spite of the several changes and revolutions the French Government has undergone during that eventful period.

The International Congress of Medical Sciences, which was to be held at Amsterdam in September, 1878, has been postponed to 1879, in order to avoid a coincidence with the Paris International Exhibition.

M. Bardoux, the French Minister of Public Instruction, having established an observatory for astronomical and meteorological purposes at Besançon, the general council of the Doubs department have voted a sum of 5,000 francs for meteorological observations to be conducted in the building.

The private view of Winkler's Lunar Landscape, which we recently described, will be on Saturday and Monday next, at the new premises of the Belgian Gallery, 112, New Bond Street. The picture will be lighted by artificial light, this having been found to be most suitable to the nature of the subject.

Kew Gardens were opened to the Public on Easter Monday at 10 A.M., and will be opened at the same hour on future bank holidays. Of the necessity of this step some idea may be formed from the following statement of the number of visitors on Monday:—From 10 to 1, 3,352; from 1 to 7, 42,833; total, 46,185.

Macmillan and Co. are about to publish a little manual of "Practical Chemistry, for the Use of Medical Students," &c., by Mr. M. M. Pattison Muir, of Caius College, Cambridge.

A terrible hurricane is reported to have occurred at Tahiti, on February 7, in which 120 persons were killed, and much property destroyed.

Hitherto we have had no occasion to say anything concerning the disturbances in the East, but during the past week there was a commotion at that now historical place St. Stefano, which we cannot let pass without reference. Happily the commotion was only seismic and did no damage. It occurred on Friday evening last and was strongly felt at Pera, while at Ismid and Broussa damage is said to have been done. The sea in the neighbourhood of the British fleet was so agitated that the commander of a gunboat sent a request to Admiral Hornby to give him previous notice on the next occasion of torpedo practice.

In connection with our recent note on chemical dictionaries we notice the completion of the second volume of the German "Handwörterbuch der Chemie," ending with the article on Electricity from the pen of Prof. Wiedemann, of Leipzig.

At a recent meeting of the French Academy, M. Gaiffe presented an apparatus with which one may determine immediately, and by a simple reading, the electromotive force of any electro-generator. It is based on Fechner's method of measuring such forces, and the measures are read in volts. M. Gaiffe employs a very resistant multiplier, and a small rheostat, by means of which introducing resistances, greater or less, into the circuit, the instrument may be adapted for very different measures of electromotive force, the same divided scale, however, being always employed. On introducing such and such a resistance you divide or multiply, in a proportion marked on the contacts of the rheostat, the electromotive force indicated by the galvanometric needle. Forces may thus be measured by the instrument from $\frac{1}{10}$ volt up to 150 volts.

J. E. Peijsmann has just published, in Batavia, an account of a scientific tour through the island of Amboina, one of the most important spice islands in Malaysia. In addition to a variety of valuable and novel observations, M. Peijsmann collected over 1,000 varieties of plants and 300 specimens of the fauna, which are to be transmitted to the museums of the University of Leyden.

A new bridge across the Rhine is now in course of construction at Basel; it will rest on five pillars.

The Dutch Government intends to construct a canal from Amsterdam through the so-called Geldern Valley to the Upper Waal (the southern branch of the Rhine), and also additional canals in the provinces of Drenthe and Overijssel. Both projects are of great commercial importance with regard to navigation on the Rhine and the communication by water between Germany and Holland.

Dr. Schomburgk, the director of the Botanic Garden, Adelaide, South Australia, has issued a very brief report relative to the economical value of the various species of South Australian "Eucalyptus." He shows that out of the large number of species of Eucalyptus spread over Australia and Tasmania, only thirty appear in the extra-tropical part of South Australia. The South Australian species do not reach so great a height as those of the east, north, and south, and only about ten species yield timber that is much valued and used, though Dr. Schomburgk thinks many more might be utilised. The most valuable timber in the colony is stated to be the red gum (*Eucalyptus rostrata*, Schlecht). It is the most durable of all the South Australian woods, and is mostly used for underground work, bridges, jetties, railway-sleepers, and for shipbuilding; moreover, it has the reputation of being proof against the attacks of white ants. This tree grows to a height of from 100 feet to 130 feet. The next most important species is the white gum (*Eucalyptus stuartiana*, F. Muell.), the blue gum (*E. vincinalis*, Las.), and the stringy bark (*E. obliqua*, L'Herit.). Dr. Schomburgk points out that the woods are not the only useful products of the Eucalypti. From *E. obliqua*, *E. leucoxylon*, and *E. rostrata*, acetic acid is obtained; wood-spirit is also procured from the first two, essential oils are produced from the leaves of *E. vincinalis*, *E. stuartiana*, and *E. citriodora*; tar from woods of *E. rostrata*, *E. leucoxylon*, and *E. obliqua*; and from the barks of several other species paper has been made.

It is announced by M. Toselli, that through successive improvements of his refrigerating apparatus, he can now produce one kilogramme of ice in the space of two minutes.

The announcement of the intended publication, in a short time, by the Smithsonian Institution, of a complete catalogue of the plants of North America, will be hailed with great satisfaction by botanists. The region covered extends from Greenland and the Arctic Ocean, on the north, to the borders of Mexico, and from the Atlantic to the Pacific. The species are enumerated in their systematic sequence, with their synonyma. The work will be published in two parts, the first, on the polyptalæ, constituting a volume of about 480 pages. It covers the ground of volume i. of Torrey and Gray's "Flora of North America." The title of the work will be "Bibliographical Index of North American Botany," by Sereno Watson.

The increase of volume of liquids through absorption of gases has lately been investigated by Messrs. Mackenzie and Nichols, in the Physical Laboratory of Berlin University. Experimenting in the first instance with carbonic acid and water only, and employing two different methods, they reach the same result, viz., that the expansion is directly proportional to the quantity of gas absorbed. They further examined the expansion of water saturated with carbonic acid by heat, and got a curve having

about the same course as that for pure water, except that the maximum of density was reached, not at 4°, as in the case of pure water, but under 3°, as is the case with salt substances.

RECENT observations by M. Ebermayer demonstrate (1) that the air in a large forest is in summer nearly twice as rich in carbonic acid as free open air; (2) that forest ground in summer contains much less CO_2 than unwooded ground (the CO_2 formed by slow decomposition of humus in the close forest seems mostly to pass into the air, and is probably utilised by the leaves for assimilation); (3) that, with rise of temperature, the increase of CO_2 in arable ground is very much greater than in forest ground; and (4) that the spread and motion CO_2 in the ground seems to take place very slowly, for in two places quite near together the amount of CO_2 may be very different. Among other bearings of these facts, the ground covering of a forest can have no important influence on the amount of CO_2 and lime in spring water, and unwooded ground may have a greater action in this respect. Again, animals living underground, *e.g.* foxes, naturally prefer the ground air of the forest, with its little CO_2, to to the ground air of the open field, which has much more.

THE influence of concentration of liquids on their electromotive force has lately been investigated by M. Moser (*Monatsb. der Berliner Acad. der. Wiss.*), who connected two glasses of differently concentrated solutions of the same salt by a siphon, and completed the circuit by wires with electrodes, which were always of the same metal. In all such cases a current arises, passing in the liquid from the dilute to the more concentrated solution. M. Moser used zinc sulphate, nitrate, chloride, and acetate, copper sulphate and nitrate, iron chloride, silver acetate and nitrate, and other salts. The highest electromotive force was $\frac{1}{6}$ Daniell, and was got with very dilute and concentrated zinc chloride solution. The various effects are arranged in tension-series. By the currents referred to, metal is dissolved in the dilute solution, separated out in the concentrated one. The equivalent of the work done by the current, M. Moser considers, is the work of attraction force between the salt and the water. The current is to be regarded as a reaction current against passage of the ions, as the polarisation current is the reaction current against the decomposition current.

THE subject of acoustic repulsion continues to be studied by M. Dvorak (*Wied. Ann.*, No. 3). Among other things he constructs an acoustic reaction wheel and an acoustic torsion balance. The former consists of four light paper or glass resonators placed tangentially at the four ends of two thin cross-bars of wood, pivoted at their intersecting point by means of a glass cap. The mouths of the resonators are all in the same relative position. The wheel is placed before the open end of a tuning-fork resonator, and enters into rotation when the fork is sounded. In another case the sound from the large resonator is transmitted through a conical tube beyond whose thin end is a wheel with square pieces at the end of the cross-arms. In the acoustic torsion balance a wooden bar furnished with a resonator is hung by a wire (as in Coulomb's balance) within a case, which has on the resonator side an opening for admission of sound. By repulsion of the resonator the strength of tones of the same number of vibrations may be compared.

LECTURING at the Sorbonne lately on atmospheric electricity, M. Mascart sought to reproduce the phenomena of thunderstorms. The dull explosions of thunder and the fulgurations in the heart of clouds preceding fulminant discharges, as also the latter, were imitated by means of a powerful Holtz machine, charging batteries, and condensers suitably arranged. The singular movements of thunder-clouds, which, obeying electric attractions and repulsions, are often observed to move in the atmospheric ocean in counter-currents, were illustrated with the aid of a balloon of hydrogen gas, to which was suspended a piece of metallic wire. The weight of the wire was such that the small aerostat, rendered slightly heavier than the displaced air, would descend; but when it was electrified, it rose again, as if freed from its burden. M. Mascart did not attempt an explanation of this curious phenomenon, which has not been repeated since the time of van Marum.

IN a recently-published report by M. Kellner to the Naturforscher Versammlung at Munich, he describes experiments made along with some others on an eleven-year old Wallachian horse of 434 kilo. weight, with regard to the relation of work done and decomposition of albumen. In five successive periods of thirteen to fourteen days the animal was fed with 5 k. meadow-hay, 5 k. oats, and 1·5 k. chopped wheat straw, and did work to the extent of 500,000, 1,000,000, 1,500,000, 1,000,000, and 500,000 kilogrammetres in the five periods respectively. In periods I. and V. the work done was the same, in II. and IV. doubled, and in III. tripled; in II. and III. the course was doubled and tripled, and in IV. the weight doubled. Of the dry substance of the fodder were digested in period I. 56·53 per cent., II. 56·45 per cent., III. 56·49 per cent., IV. 54·01 per cent., V. 53·07 per cent. The horse's weight varied as follows:—I. 534·1, II. 529·1, III. 522·3, IV. 508·8, V. 518 kilo. The excretion of nitrogen was on an average of the last six to nine days of each experimental series, I. 98·81 k., II. 109·16, III. 119·82, IV. 107·53, V. 101·88. These numbers show strikingly, in opposition to Voit's and Pettenkofer's results, that with increase of work done, is associated a not inconsiderable increase of decomposition of albumen.

THE additions to the Zoological Society's Gardens during the past week include an Indian Leopard (*Felis pardus*) from India, presented by Major Tubbs; a Red Deer (*Cervus elaphus*), a Common Fox (*Canis vulpes*), European, presented by Mr. Carroll W. Ansdell; two Spotted Ichneumons (*Herpestes auropunctatus*) from Nepal, presented by Mr. J. McIntosh; a Suricate (*Suricata zenik*) from South Africa, presented by Mr. Percy Howard; an Azara's Fox (*Canis azaræ*) from Brazil, presented by Dr. A. Stradling; a Stanley Crane (*Tetrapteryx paradisea*) from South Africa, presented by Capt. A. F. Lendy; a Leadbeater's Cockatoo (*Cacatua leadbeateri*) from Australia, presented by Mr. W. Ruston; a Collared Fruit Bat (*Cynonycteris collaris*), four Common Foxes (*Canis vulpes*), born in the Gardens.

THE DETERIORATION OF OIL PAINTINGS[1]

II.

IF we compare the pictures of the Italian and Dutch schools of the fifteenth, sixteenth, and seventeenth centuries, with those of the French and English schools of the last hundred years, we are struck by the great difference in the nature of their diseases. We may divide those diseases into *constitutional* ones—that is to say, such as are based on the method and the material used for painting, and into those produced by external influences.

The Dutch pictures of the fifteenth, sixteenth, and seventeenth centuries, and the Italian pictures of the fifteenth and sixteenth centuries, seem to me perfectly free from constitutional diseases. It is only in the seventeenth century that the Italian pictures show a special constitutional alteration, caused by the practice of the Bologna school.

The pictures of the last hundred years of the French school, of a part of the English school, and some painters of other schools, have been attacked by a constitutional disease perfectly defined and characteristic of this period.

Among external influences injurious to oil painting, we have to consider dampness, heat, bad air, dust, smoke, mechanical injuries, and last, not least, the destructive or "altering" hand of the picture-restorer.

Pettenkofer's scientific researches first clearly defined the influence of humidity on oil paintings, showing that it produced a discontinuity of the molecules of the vehicle and the resinous substances. As glass, when pulverised and thereby mixed with air, loses its transparency, and water, when mixed with oil,

[1] Paper read at the Royal Institution, Friday, March 1, by R. Liebreich, M.D., M.R.C.S., M.R.I. Continued from p. 495.

becomes of a milky aspect, so the oily and resinous substances contained in paintings will become dim as soon as air penetrates between their particles. The picture thus assumes a greyish, dim appearance, and the pigments seem to have been fading. That this is not really the case has been proved by the influence of a process invented by Pettenkofer, which he calls regeneration. In a flat box the picture is exposed to air impregnated with alcohol. Of this latter the resinous elements of the picture absorb a certain quantity, swell and fill up the interstices between the separated particles so as to reunite them into an optically homogeneous transparent substance.

The alcohol does not affect in the same way the hardened oil. If the interstices between its particles are not filled up by the swelling resin, it becomes necessary to introduce a new substance into the picture, and this is called nourishing a picture.

Pettenkofer has the great merit of having clearly proved that the nourishing of a picture with oils, as the custom was formerly, and still is to some degree, is a very objectionable proceeding, as it has the effect of darkening the colours for ever. He recommends, instead of oil, balsam of copaiva, which has become since an invaluable means for preserving and restoring oil paintings, and will be more and more extensively used.

I have frequently applied Pettenkofer's method, and with very beneficial effect; but whenever I mentioned it to professional picture-restorers, here as well as on the Continent, I always found them to reject it, either *à priori*, or after experiments incorrectly made.

In Munich, it seems, the pictures of all periods and of all schools have had to suffer under local influences and through the changes in the humidity of the air. This accounts for Pettenkofer having principally described this, so to say, endemical disease. In other galleries this affection does not appear so frequently, and Pettenkofer's method, therefore, will not find everywhere the same extensive application as at Munich. I think, however, that with some modifications it may be employed against some other alterations. I have, for instance, found it efficacious with paintings which had been injured by exposure to great heat. I shall show you a small picture which had been hanging for a long time so near a gas flame that it was almost completely scaling off, and so entirely faded that it scarcely looked like an oil painting at all. In that state it was exposed to alcoholised air, then nourished with balsam, and its back slightly varnished; and the scales starting from the canvas were refixed by pressure. And now it appears fresh in colour, firm in substance, and perfectly smooth on its surface. The old, cracked varnish, melted together by the alcohol, looks as if fresh laid on.

Humidity sometimes favours the development of fungus. The round, black, small spots which pass through the canvas and the painting of these two pictures are produced by the same little plant which Prof. Tyndall showed you when he spoke on the highly interesting subject of spontaneous generation.

Oil and water, so injurious to oil paintings, enter both into the material used for lining. Anxious to exclude these sources of danger, and to simplify the whole process, I have endeavoured to replace it by a new method which I shall submit to you this evening.

How paintings may be disfigured by restorers you see in this picture, which was renovated with oil colours according to the practice only abandoned about thirty years ago, when it was advantageously replaced by the use of varnish colours.

The amount of external injury oil paintings sometimes endure and stand is perfectly amazing. Pictures in the course of centuries, during the destructive fury of wars and revolutions, may have been torn out of their frames, rescued from below the ruins of burned monasteries, may subsequently have passed from one *bric-à-brac* shop to another, where they have been piled up, to be pulled about at each new inspection, and literally trodden under foot, whereby they have finally been reduced to a state of colourless, greyish, or black rags. Still such pictures may not unfrequently be awakened, as it were, to new life, to their original brilliancy of colour, if, with all necessary care, their injured limbs are put together again, their wounds are healed, and fresh nourishment, air, and thorough cleansing, are administered to their lacerated bodies.

A sound constitution is, of course, a necessary condition for obtaining any such result, without it we can only obtain a partial cure. We see this with reference to the Bolognal school of the seventeenth century. The pictures which you see here are instances of this. From the state of rags to which they were reduced they have passed, by appropriate treatment, into the state of firm, even, well-conditioned, and clean pictures. The constitutional alteration characteristic of their time and school, however, could not be cured. You will, therefore, perceive that the contrast is too great between light and shade, that the half tones are too weak and that the glazings spread on dark ground, which certainly existed formerly, have been destroyed by the growing of bolus and umber of the priming. That this is not the fault of the method of restoration is clearly proved by the state in which you will find all the pictures of this school, even those best preserved in the best galleries of all countries.

The constitutional diseases of pictures belonging to the French and to the English school of the last hundred years are of still more serious nature, and much more difficult to cure. Many of them, though they were never exposed to any injury whatever, nor are likely ever to be so in our present state of civilisation, cannot be guarded from premature decay in spite of all possible care with which they are kept.

The principal symptoms of their bad constitution are :—

1. Darkening of the opaque bright colours.
2. Fading of the transparent brilliant colours.
3. Darkening, and above all, cracking of the transparent dark colours.

The best opportunity to study these several appearances is given us in the Museum of the Louvre, which contains a great number of such pictures in the section occupied by the French school. I have paid particular attention to the cracks in these pictures, as I find that in shape, in size, in position, as well as in relation to the various colours, they differ distinctly from the cracks in older pictures and in those of other schools. This, of course, is of importance, not only for the explanation of the reasons which produced them, but as a symptom which, in a given case, might determine the diagnosis, whether a picture be an original or only a copy. The special characteristics of these cracks are the following :—

They are all but exclusively found in the thickly laid on transparent dark colours, and they are the deeper and the more gaping in proportion to the thickness of the layer of the colour and the extent of the dark surface. The chief cracks run parallel to the outlines of surfaces painted with bright opaque colours, such, for instance, as are used for the flesh tints, and which are more or less thickly laid on. But there is generally a slight distance between the bright colours and the cracks.

Lateral branches of these cracks pass into the white, but they do not gape, provided the white colours had been laid on directly upon the priming, and not upon a layer of dark transparent and not sufficiently dried colour.

This examination of the cracks of pictures has sometimes afforded me a peculiar insight into the practice used for the picture. In the well-known picture, for instance, by Guéricault, of "The Wreck of the *Medusa*," in the Gallery of the Louvre, the cracks follow exactly the outlines of the bright flesh-tints. The arm of one of the dead bodies hanging in the water is so covered by planks and water that nothing of the forearm is to be seen. It is, however, very easy to prove that originally that arm was painted in all its length, for the cracks do not only follow the outline of the visible upper arm, but also the no longer visible forearm, and all the five fingers. This proves that the fore part of the arm and the hand were originally painted in flesh-tints before they were covered over by the planks, and the water painted afterwards. In Ingres' portrait of Cherubini, the face of the latter is beautifully preserved, while that of the Muse, as well as her drapery, is covered with cracks. In the depth of the cracks of the white drapery an intense blue tint is to be seen. Mr. Henry Lehmann, of Paris, the favourite pupil of Ingres, who knows the history of this picture as an eye-witness, and whom I consulted about this very striking appearance, gave me the following information :—Ingres painted the head of Cherubini in Paris, and then took it with him to Rome. There it was pieced into a new canvas and lined. Then the Muse was painted, and before the colours were perfectly dry, another model was chosen, and a new Muse painted over the old one. The colour of the drapery was likewise altered, and this explains the cracks in the white colour, and explains also why the blue appears in the depth of the cracks of the drapery.

Among the English artists of the last hundred years, some have painted with the same material and by the same process as their French contemporaries, and consequently with the same unfortunate results. Others avoided these by using the same

material with more precautions. Others, again, and among them Sir Joshua Reynolds, have in their different works followed various practices, and consequently had varied results. Thus, some of Sir Joshua's pictures have kept perfectly sound. Others are cracked in the characteristic way just mentioned. Others, again, are cracked in an absolutely irregular way. We can easily form an idea of it if we read in his "Diary Notes," for instance, the way in which he painted the portrait of Miss Kirkman, which he began with whiting and gum tragacanth, then covered it successively with wax, then white of eggs, and then varnished it.

The study of the alterations already fully developed in pictures painted within the last hundred years only, and their comparison with the works of the old masters, would suggest the following rules for the process of painting:—

1. The oil should in all colours be reduced to a minimum, and under no form should more of it than absolutely necessary be introduced into a picture.

2. All transparent colours which dry very slowly should be ground, not with oil at all, but with a resinous vehicle.

3. No colour should be put on any part of a picture which is not yet perfectly dry; and, above all, never a quick-drying colour upon a slow-drying one, which is not yet perfectly dry.

4. White and other quick-drying opaque colours may be put on thickly. On the contrary, transparent and slow-drying colours should always be put on in thin layers.

If the effect of a thick layer of these latter is required, it must be produced by laying one thin layer over another, taking care to have one completely dry before the next is laid on. If transparent colours are mixed with sufficient quantity of white-lead, they may be treated like opaque ones.

We come now to the last layer of the picture, to that one which is spread over its surface in order to equalise optical irregularities, and to protect it at the same time from the air. I mean the varnish.

The varnish may crack or get dim; then it should be treated with Pettenkofer's method; but it may become dark yellow, brown and dirty, and so hide the picture that it becomes necessary to take it off and to replace it by a thin layer of new varnish. It is here that picture-restorers, or we may say picture-cleaners, display their beneficial skill, and also their very destructive activity.

If a picture is throughout painted in oil, if its substance has remained sound and even, and varnished with an easily soluble mastich or dammar varnish, then there will be neither difficulty nor danger in removing the varnish. This can, in such a case, be done either by a dry process, that is, by rubbing the surface with the tips of the fingers, and thus reducing the varnish by degrees to a fine dust, or by dissolving the varnish by application of liquids, which, when brought only for a short time into contact with the oil painting, will not endanger it. We have, however, seen that the works of the old masters are not painted with oil colours like those used by modern painters, but, on the contrary, that certain pigments, and especially the transparent colours used for glazing, were ground only with resinous substances. These latter have, in the course of time, been so thoroughly united with the layer of varnish spread over the surface of the picture, that there no longer exists any decided limit between the picture and the varnish. It is in such pictures that a great amount of experience, and knowledge of the process used for the picture, as well as precaution, are required in order to take away from the varnish as much only as is indispensable, and without interfering with the picture itself. Numberless works of art have been irreparably injured by restorers, who, in their eagerness to remove dirt and varnish, attacked the painting itself. They then destroyed just that last finishing touch of the painting, without which it is no longer a masterpiece.

The difficulty and danger are much greater in cleaning those pictures which have not been varnished with the ordinary easily-dissolved mastich or dammar varnish, but have been painted over with oil, oil-varnish, or oleo-resinous varnish. It seems incredible that these substances should ever be used for such purposes; it is, however, a fact that there are still people who fancy that it will contribute to the good preservation of their pictures to brush from time to time a little of those liquids over their surface. They recognise too late that the varnish becomes more and more dark, of a brownish colour, and opaque. If such varnish has afterwards to be removed, then we meet with the great difficulty, that this can be done only with substances which would just as easily dissolve the whole picture as the hardened layers spread over it.

This shows what can be the value of those universal remedies which from time to time appear, and are praised for the innocuous way in which pictures by their means may be cleaned.

There is at this moment a great discussion going on in Italy about Luporini's method. Luporini is a painter and picture-restorer in Pisa, who believes himself to have invented a new means of cleaning pictures without any danger. Some months ago, in Florence, I examined a large number of pictures cleaned by him. Those of the Gallery of St. Donato, belonging to Prince Demidoff, mostly Flemish and Dutch landscapes, are cleaned very well and without any injury to the painting. On the contrary, the St. John, by Andrea del Sarto, one of the finest pictures of the Palazzo Pitti, I found very much altered by the restoration of Luporini. I had studied that picture very closely the year before, and should now sooner believe it to be a modern copy than the cleaned original. It has lost all softness of outline and the characteristic expression of the face. The change in the flesh tints can scarcely be explained otherwise but by an entire removal of the glazing.

I think it is taking a heavy responsibility to allow a new experiment to be tried upon such an invaluable work of art. Even private persons, who are fortunate enough to be in possession of such treasures, ought to feel responsible for the good preservation of masterpieces, which are, it is true, their material property, but which intellectually belong to the whole civilised world of the present and of the future.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—Messrs. Mackren, Robbs, and Hichens, have been appointed to Scholarships in Natural Science at Gonville and Caius College.

EDINBURGH.—At the Graduation Ceremonial on Tuesday the degree of Doctor of Science in the Department of Mental Science, was conferred on Jacob Gould Schurman, B.A.; in the Department of Mathematics on Alexander Macfarlane, M.A., B.Sc.; in the Department of Chemistry on William Inglis Clark, B.Sc. The degree of Bachelor of Science was conferred on William Thomson in the Department of the Mathematical Sciences; on John Adrian Blaikie and James Johnstone Dobbie in the Department of the Physical Experimental Sciences; on William A. Haswell in the Department of the Natural Sciences; on James Alfred Ewing and John Gray in the Department of Engineering; and on John Brown, M.D., John Berry Haycraft, M.B., C.M., and John Trehame, M.B., C.M., in the Department of Public Health. The Hope Prize Scholarship in Chemistry was awarded to Mr. Lewis Johnstone, and the Falconer Memorial Fellowship for the encouragement of the study of Palæontology and Geology, of the annual value of 100*l*., tenable for two years, and conditionally for four years, was awarded to R. A. Lundie, M.A., B.Sc.

BALTIMORE.—We recently referred to the system of fellowships at the Johns Hopkins University, Baltimore. From a statement on the subject which has come to hand, we learn that twenty fellowships, each yielding 500 dols. a year, are annually open in the University. They are awarded by the trustees on the nomination of the Faculty, as nearly on the first of June as may be found practicable. Candidates are invited from any part of the country. The object of this foundation is to give to a few scholars of promise the opportunity to prosecute further studies, under favourable circumstances, and likewise to open a career for those who propose to follow scientific and literary callings. The University expects to be benefited by the presence and influence of the Fellows, and by their occasional services; from among the number it hopes to secure from time to time some of its teachers. Three of the twenty fellowships are allotted this year to each of the five departments, Greek, mathematics, chemistry, physics, and biology; and the remaining five will be allotted either in these departments or in others, at the discretion of the Faculty. Appointments are made by a careful consideration of all the evidence submitted to the Faculty. Every candidate in presenting his name is expected to address a letter to the president indicating the course of his previous reading and study, and his general purposes with reference to future work. It is desirable for him to present in printing or manuscript an essay or thesis which may have been written either

for this occasion or for any other purpose. If he has been engaged in any scientific or literary research he should indicate its character, and generally give evidence as to his previous career and *bonâ fides*. The holders of the fellowships are required to reside in Baltimore during the entire academic session, and they are not permitted to engage in teaching, out of the walls of the University, unless for exceptional reasons in other colleges which may ask for some temporary service. They are expected to devote all their time to study under the guidance of one of the professors, or if there be no professor in the chosen department, under the general approbation of the Faculty. Toward the close of the Academic year a report of his work is expected from each Fellow. As opportunities offer, the Fellows are encouraged to prepare and read lectures or essays on subjects to which they have given special attention. They are also required to render occasional services as examiners or as assistants in the laboratories; but those services are not burdensome, unless they are compensated by additional stipends. Those who become distinguished by their attainments may be assured of the constant encouragement of the Faculty. With all these precautions there seems little chance of the Johns Hopkins University being eaten up by idle Fellows.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, April 11.—"The Acceleration of Oxidation caused by the Least Refrangible End of the Spectrum," by Capt. Abney, R.E., F.R.S.

In a paper contributed to the *Philosophical Magazine* in January last, the author expressed an opinion that Chastaing's idea regarding an acceleration of oxidation being caused by red light might prove true in regard to the oxidation of the photographic image, and elsewhere[1] that Becquerel's coloured spectra might be explained on the same principles, and this he finds to be true as regards oxidation of the photographic image.

A silver bromide film was exposed to diffused light. It was then submitted to the action of the solar spectrum, whilst immersed in a solution of potassium permanganate, hydroxyl, potassium bichromate, or nitric acid, or in ozone. When the strength of these was correct, a reversed image of the least refrangible end of the spectrum was obtained, an increase in oxidation taking place where the red rays acted, the reversal commencing somewhere near D, and extending into the ultra-red.

The accelerating effect of the red rays is most marked when the solutions are weak; but there is a limit to the dilution caused by the fact that in the films employed the silver salt is sensitive as far as the wave length 10,000, and there must be sufficient strength to oxidise the invisible image as it is formed, besides gradually destroying the effect of the preliminary exposure. With silver iodide, as there is no reduction by the red rays, the reversed action is much more readily obtained.

A reversed image of the least refrangible end of the spectrum can thus be produced by using solutions of a certain strength, whilst if made more dilute an unreversed image is obtained. This throws a light on Draper's photographs of this region of the spectrum.

Geological Society, March 6.—Henry Clifton Sorby, F.R.S., president, in the chair.—Henry Edward Richard Bright, George James Cotton Broom, William James Farrer, George Scamell, and Joseph Fletcher White were elected Fellows of the Society.—The following communications were read:—On the geology of Gibraltar, by Prof. A. C. Ramsay, F.R.S., and James Geikie, F.R.S. In this paper the authors, after giving some account of the physical features of Gibraltar, described in detail the various rock-masses of which the peninsula is composed. The chief rock is a pale grey, bedded limestone, overlain by shales containing beds and bands of grit, mudstone, and limestone. Fossils are very rarely met with in the limestone, and have never as yet been found in the shales. The only recognisable fossil they obtained from the limestone was a *Rhynchonella*, which Messrs. Etheridge and Davidson think is most likely *Rh. concinna*. This would make the beds of Jurassic age. The limestone forms the great eastern escarpment, and dips west under the shales, which form the lower slopes upon which the town is built. The dips vary from 12° or 20° up to vertical. The connection of these strata with the rocks of the adjoining districts in Spain and the opposite coast of Africa was traced, and it was shown that the Gibraltar limestone reappears in Ape's Hill in Barbary, while the overlying shales and the sandstones of Queen of Spain's Chair form all the ground to the west of Ape's Hill up to Cape Spartel. The Jurassic strata of Gibraltar are overlain by various superficial accumulations, the oldest of which is a great mass of limestone agglomerate, which is unfossiliferous, and shows as a rule no trace of stratification. It is made up of angular blocks of limestone of all shapes and sizes, and rests upon an uneven surface of limestone: it also covers wide areas underneath which only shales are present. It is excessively denuded, being worn into ravines and gullies, and presents generally a highly honeycombed surface. Terraces of marine erosion have also been excavated in it. It is not now accreting, and could not have been formed under present conditions of climate and surface. The authors gave at length their reasons for believing it to have been the result of a severe climate. The blocks were wedged out by the action of frost, and the heaps of angular *débris* thus formed were saturated by water derived from melting snows, and so were caused to flow *en masse* down the mountain slopes and over the gently inclined ground at their base. The caves and fissures of Gibraltar were then described. It was shown that the true bone-breccias were confined to these. Many of these fossiliferous breccias are of later date than the great agglomerate, since they are met with in fissures and caves that intersect the limestone and limestone agglomerate alike. When the mammalia tenanted Gibraltar, Africa and Europe were united, and the climate was genial. All round the rock occur platforms, ledges, and plateaus, which are evidently the work of the sea. These erosion-terraces are covered in many places with calcareous sandstones containing recent species of Mediterranean shells. Such marine deposits occur up to a height of 700 feet. The movement of depression was interrupted by pauses of longer or shorter duration, and the climatic conditions were probably much the same as at present. After the rock had been re-elevated, the subaërial forces modified the surface of the marine sands that covered the limestone platforms, so that they came to form long sand slopes. The land at this period was of greater extent than it is now, and some grounds exist for believing Europe to have been again united to Africa, for mammalian remains occur here and there in the deposits that overlie the limestone platforms. These relics, however, it is just possible may be derivative. The climate was probably still genial like the present. Overlying the marine and subaërial deposits just referred to occurs an upper and younger accumulation of massive unfossiliferous limestone agglomerate. This deposit the authors believe to owe its origin to severe climatic conditions. After the marine deposits that cloak so much of the eastern side of the rock had been weathered into subaërial sand-slopes, large blocks were detached from the cliffs and steep slopes, and these dropped down upon the sand and were soon drifted over. By and by the blocks fell in such quantities that the sand-slopes in many places were completely buried under a talus of limestone *débris*. This was subsequently consolidated by infiltration into a solid agglomerate, in the same way as the underlying sands were hardened into sandstone. These sandstones contain a few blocks of limestone only in their upper portions. In their horizontally-bedded and lower-lying portions no limestone blocks occur. This later agglomerate bears every stamp of great antiquity, and could not have been formed under present geographical and climatic conditions. The surface is honeycombed and worn, just like that of the solid limestone and the older limestone agglomerate. Since its accumulation the climate has greatly changed, the present being characterised by the absence of frost. In concluding, the authors discussed at length the cause of the cold conditions that gave rise to the great limestone agglomerates, and argued that this cause could not have been *elevation of the land*. They also pointed out that a *submergence of the Sahara* would be equally incompetent to bring about the desiderated climatic conditions, and that even a former much greater elevation of the land, combined with the appearance of a Sahara sea, would fail to supply us with the severe winter climate that was necessary to produce the great agglomerates. They thought that the most probable explanation of the phenomena described is that the cold conditions referred to were contemporaneous with that general refrigeration of climate which took place over so vast an area in our hemisphere during pleistocene times. The limestone agglomerates they look upon as the equivalents of those glacial deposits that occur so plentifully in our own and other countries, and the bone breccias, which are intermediate in date between the lower and upper limestone agglomerates, are paralleled by the interglacial beds of the British Islands, Sweden, Switzerland,

[1] "Treatise on Photography," p. 225. Longmans.

&c.—Notes on the geology of Japan, by J. G. H. Godfrey, F.G.S.

Physical Society, March 16.—Prof. W. G. Adams, president, in the chair.—A special general meeting was held for the election, as an *ex officio* honorary member of the Society, of the President of the Physical Society of Paris.—The following candidates were then elected Members of the Society:—J. S. Bergheim, W. M. Hicks, M.A., Dr. J. Hopkinson, M.A., D.Sc., Miss E. Prance, and T. Wills.—The Secretary read a paper by Mr. W. J. Millar, C.E. on the transmission of vocal and other sounds by wires. The author was led, mainly by a consideration of the manner in which sounds are conveyed through walls and partitions, to make an extensive series of experiments on this subject, from which he concludes that conversation can be carried on at considerable distances by simply employing stretched wires provided with suitable vibrating discs. In one experiment two copper wires were attached to points on a telegraph wire 150 yards apart, and breathing, singing, &c., were distinctly audible; by stretched wires extending through a house and provided with mouth- and ear-pieces in the several rooms, conversation could be carried on without difficulty. The materials employed for terminals were very varied, and the vibrating disc, whether metal, wood, or india-rubber, &c., was generally formed as a drum-head, the wire being fastened at its centre. The volume of sound appears to be greater with a heavy wire, but in all cases it requires to be stretched.—The President referred to the experiments of Wheatstone on the conduction of sound by vibrating bodies, especially long wooden rods. He mentioned that in 1856 a performance was given at the Polytechnic at which numerous experiments connected with such conduction were exhibited. Some years ago M. Cornu, in conjunction with M. Mercadier, made experiments which showed that vibrations can be transmitted along a copper wire and rendered visible at the distant end on a rotating blackened drum. The free end of the wire was attached to a piece of copperfoil fixed at its base and provided with a point which left a clear trace on the drum when the distant end was attached to, say, a vibrating tuning-fork. By connecting such an arrangement with different instruments and varying the players also, M. Cornu has ascertained the form and extent of vibration corresponding to each. The arrangement adopted by him was exhibited by Prof. Adams, and in conclusion he referred to a passage in Hooke's "Micrographia," which clearly showed that he was aware of the facility with which sounds can be transmitted by solid bodies.—Mr. W. H. Preece described some experiments made in September of last year, by Mr. A. W. Heaviside and Mr. Nixon at Newcastle-on-Tyne on this subject, from which they conclude that the method might certainly be applied with success to the transmission of speech within a building. They find that a No. 4 wire gives the best results. The terminals were wooden discs about $\frac{1}{4}$ in. thick, and to these the wire was attached "end on," but speech could be distinctly heard by laying such a disc on any intermediate point of the wire. When the wire was particularly still speech was audible up to 200 yards.—Mr. G. W. von Tunzelmann then read a paper on the production of thermo-electric currents in wires subjected to mechanical strain. The wire, of iron, steel, or copper, was stretched vertically between two cans which could be maintained at different temperatures. It was fixed in the base of the lower can and held in the upper one by a clamp attached to the shorter arm of a lever, to the longer arm of which the stretching weight was applied. The free ends of the wire were joined to copper wires which led to the Thomson galvanometer, these junctions being covered with cotton wool. He has succeeded in reconciling the contradictory conclusions arrived at by Sir W. Thomson and M. Le Roux; whereas the former only used moderate strains, the latter worked near the breaking limit, and the author finds that if the weight be gradually increased the direction of the current changes, and hence these two authorities found the currents to flow in opposite directions. A great number of experiments were made, and from them it is evident that on applying a strain the deflection does not immediately attain a maximum, but it gradually rises for about eight minutes, and then gradually falls, attaining a stationary point at the end of about twelve minutes.—Prof. Adams then exhibited a simple arrangement for projecting Lissajous' figures on to the screen which has been made by his assistant, Mr. Furze. It consists of two strong straight steel springs, fixed in separate heavy iron frames, the one horizontally and the other vertically. The latter carries at its end a double convex lens and the former carries a black disc perforated with a small hole and is so mounted that its length may be varied as required. If now the disc be placed before the lamp and the point of light be focussed on the screen by means of the lens on the vertical spring, the two springs may be caused to vibrate and the spot will describe a figure corresponding to their relative rates.—Dr. Guthrie exhibited an experiment to show the behaviour of colloids and crystalloids in relation to electrolysis. A solution of gelatine was coloured with litmus, made acid and mixed with sulphate of soda; two platinum poles of a 6-cell Groves' battery were then immersed in it and the gelatine was allowed to set. The mass became comparatively clear round the positive pole and red and blue clouds were formed which met across a space of about $1\frac{1}{2}$ in. in three-quarters of an hour. The relative advance of the ions was indicated by the brightening of the litmus round one pole and by the blue coloration produced at the other.

Chemical Society, April 4.—Dr. Gladstone, president, in the chair.—A lecture "On the Application of the Microscope to some Special Branches of Chemistry" was delivered by Mr. H. C. Sorby, F.R.S. The lecturer confined his discourse to the application of the microscope for determining the refractive indices of liquids and solids. An object is placed on the stage of a microscope and the focus adjusted accurately; on covering the object with a plate of some refracting substance, the object will be invisible; to bring it again into focus the body of the microscope must be moved further out. If this distance be "d" and the thickness of the plate be T, then the index of refraction $= \frac{T}{T - d}$. This distance can be measured either by a scale and vernier attached to the body of the microscope or by graduating the head of the screw which works the fine adjustment. The lecturer then described the various methods by which the two quantities T and d could be practically measured to $\frac{1}{1000}$th of an inch; the curious and diversified images seen by observing with a microscope a circle or a grating through transparent plates of various substances were then explained. Minerals having no double refraction are unifocal, *i.e.*, both systems of lines in a grating can be seen at the same focus. Minerals having double refraction are bifocal, *i.e.*, only one system of lines can be seen at one focus, a new focus having to be found in order to see the lines at right angles to the first set. This method has enabled the author to identify various minerals in sections $\frac{1}{500}$th inch thick and $\frac{1}{100}$th inch in diameter. Thus in a dolerite $\frac{1}{400}$th inch thick, a zeolite, labradorite, calcite, and augite were identified with almost absolute certainty. In sections of shells $\frac{1}{1000}$th of an inch thick calcite can be easily distinguished from arragonite. In conclusion the lecturer referred to the connection between the indices of refraction and chemical composition; the data are defective at present, but several points have already been made out; thus of two minerals having similar compositions, but one containing calcium and the other one of the alkalies, the first has a higher index of refraction; a lime garnet on the other hand has a lower index than a precious garnet which contains iron instead of calcium.

Linnean Society, April 4.—W. Carruthers, F.R.S., vice-president, in the chair.—There was exhibited by Dr. H. Trimen the base of the stem of the Water Hemlock (*Cicuta virosa*, Linn.) in its floating winter state, obtained near Yarmouth. This was well figured in the *Phil. Trans.* last century, but since has seldom been referred to by botanists.—Mr. G. Murray showed under the microscope specimens of growing Saprolegnia, exhibiting terminal and interstitial oogonia.—A paper on some minute hymenopterous insects, by Prof. J. O. Westwood was, in his absence read by Mr. McLachlan. This contains descriptions of the following new forms: *Mymar taprobanicus*, *M. wolastonii*, *Alaptus excisus*, *Oligosita subfasciata*, *O. stanforthii*, *O. nodicornis*, and *Trichogramma erosicornis*. All singular insects alike interesting structurally and as regards habits, &c.—A short notice was made by Mr. M. C. Cooke on a collection of fungi from Texas, made by Mr. Ravenel. Adding all other recorded species the series shows that much yet remains unknown in the mycologic flora of what probably is one of the richest States of the Union.—The Secretary read some remarks on the peculiar properties ascribed to a fungus by the Samoans, by the Rev. Thos. Powell. The natives name it "Limamea"; specimens of which have been forwarded to the Rev. M. Berkeley for identification. It destroys their bread-fruit trees and the Chestnut (*Inocarpus edulis*). An antidote to its ravages is said to exist in the liliaceous plant *Crinum asiaticum*, which the natives grow

between the trees liable to be affected.—The following gentlemen were elected Fellows of the Society:—Frederick Manson Bailey, Dr. Archibald Hewan, George Payne, jun., and James R. Reid.

Zoological Society, April 2.—Prof. Newton, F.R.S., vice-president, in the chair.—A communication was read from the Marquis of Tweeddale, F.R.S., containing the seventh of his contributions to the ornithology of the Philippines. The present paper gave an account of the collection made by Mr. A. H. Everett in the Island of Panaon.—Mr. A. G. Butler, read descriptions of new Lepidoptera of the group Bombycites in the collection of the British Museum.—A communication was read from M. E. Oustelet, containing the description of a new species of cassowary, from New Guinea, proposed to be called *Casuarius edwardsi*.—A communication was read from Mr. F. Nicholson, F.Z.S., containing the description of an apparently new species of American pipit from Peru, which he proposed to call *Anthus peruvianus*.—Prof. A. H. Garrod, F.R.S., read some notes on the placentation of *Hyomoschus aquaticus* as observed in the pregnant uterus of a fresh specimen of this animal recently examined.

Victoria (Philosophical) Institute, April 1.—A paper on modern geology and its bearing on the antiquity of man, was read by Prof. Birks, of Cambridge.

Institution of Civil Engineers, April 9.—Mr. Bateman, president, in the chair.—The paper read was on the embankments of the River Thames, by Mr. Edward. Bazalgette, Assoc. Inst. C.E.

EDINBURGH

University Chemical Society, March 13.—Mr. W. Inglis Clarke, B.Sc., vice-president, in the chair.—A paper was read by Mr. Adrian Blaikie on the salts of trimethylsulphine, containing the results of a joint investigation carried on by Prof. Crum-Brown and himself. They find that the oxalate of trimethylsulphine crystallises in clear hydroscopic plates with one molecule of water of crystallisation, $\{(CH_3)_3S\}_2C_2O_4 + H_2O$. On heating, the salt at 110° C. gives off its water of crystallisation, and at 140° gives off sulphide of methyl, leaving pure oxalate of methyl, $\{(CH_3)_3S\}_2C_2O_4 = (CH_3)_2C_2O_4 + 2(CH_3)_2S$. The sulphide of trimethylsulphine, obtained by mixing equal quantities of sulphydrate and oxyhydrate of trimethylsulphine, can only be obtained in a solution which when evaporated over phosphoric anhydride in an atmosphere of coal gas, decomposes, after a certain strength of solution has been reached, into sulphide of methyl, $\{(CH_3)_3S\}_2S = 3(CH_3)_2S$. The hyposulphite of trimethylsulphine is obtained either by oxidation of the sulphide or polysulphide of trimethylsulphine. It crystallises in clear hydroscopic four-sided prisms with one molecule of water of crystallisation, $\{(CH_3)_3S\}_2S_2O_3 + H_2O$. This salt has all the characteristics of an alkaline hyposulphite. On drying over phosphoric anhydride it gives off its water of crystallisation, and on heating the anhydrous salt at 137° C. it gives off 23·5 per cent. sulphide of methyl, leaving a white crystalline substance, soluble in water, alcohol, and ether, which is at present under investigation.—A paper was also read by Mr. John Treharne, M.B., C.M., on some phenomena observed in the cooling of fats.

PARIS

Academy of Sciences, April 15.—M. Fizeau in the chair.—The following among other papers were read:—Sun-spots and magnetism, by M. Faye. Replying to Prof. Piazzi Smyth's question (NATURE, vol. xvii. p. 220), M. Faye says:—1. The periods 10·45 years for the needle, 11·11 for the spots, have been well determined by Mr. Broun and M. Wolf respectively. 2. The two phenomena are not related. 3. A combination of favourable circumstances, reproduced every 176 years, has led to belief in their connection. 4. These temporary concomitances are not absolutely rare in the history of sciences.—On a new compound of palladium, by MM. Sainte-Claire Deville and Debray. This relates to an ammoniacal sesquichloride of palladium, obtained by causing chlorine solution to act in the cold state on yellow chloride of palladamine. One analysis of it gave: palladium, 42·6; chlorine, 43·5; ammonia, 12·9.—Experiments tending to imitate various forms of foldings, contortions, and ruptures met with in the earth's crust (continued), by M. Daubrée. A thin layer of an adhesive colouring matter is applied to the surface of a distended balloon of vulcanised caoutchouc. On letting some of the air escape the coated portion forms a protuberance with regular and parallel wrinkles in certain directions; and M. Daubrée finds like phenomena in the earth's crust.—On the annual temperature of the air, the earth, and the water, in the Jardin des Plantes of Montpellier, according to twenty-six years of observations, by M. Martini. The mean annual temperature (of the air) is 13·42°; at Paris and Montsouris Observatories it is 10·67° for the same twenty-six years. The mean annual temperature at 0·10m. depth in unsodded ground is inferior to that of the air (about 2°) if only morning observations are taken; but from observations morning and evening they are nearly the same (ground, 14·65°, and air 14·11°, in the year 1863). The mean temperature of the subterranean sheet of water is 12·77°.—Report on a memoir by M. Jobert relating to aerial respiration of some Brazilian fishes. M. Jobert has found several fishes in the Upper Amazon, having two modes of respiration, one by the gills, the other by the alimentary canal, swallowing air and evacuating by the anus a gas which has more CO_2 and less O than air has. The intestine has a number of filiform appendices composed of blood vessels, which doubtless absorb some of the swallowed oxygen. In other fishes the gas returns by the mouth instead of the anus. In others the swimming-bladder (which has numerous blood vessels in its walls) takes the place of the lungs.—On the equivalent of gallium, by M. Lecoq de Boisbaudran. From calcination of the alum and calcination of the nitrate the mean obtained for the equivalent is 69·865. This agrees closely with a theoretical number got for a body between aluminium and indium.—On the mode of formation of the meteoritic breccia of Santa Catharina, Brazil, by M. Meunier. Four phenomena are traced:—1. Shattering of the metallic iron, and accumulation of the fragments with spaces between. 2. Penetration of sulphuretted hydrogen into these spaces, producing sulphur, and a mixture of pyrrhotine and graphite. 3. Mechanical crushing of the mass. 4. Production of new graphitous matter filling the fissures of the second formation.—On the dissociation of hydrate of chloral, by MM. Moitessier and Engel. From experiment they find that the tension of the vapour of the substance, when boiling, is superior to atmospheric pressure, hence they infer dissociation of the hydrate between 78° and 100° as affirmed by M. Wurtz.—On a rare form of the hepatic organ in worms, by M. Chatin. In a nematoid of the group of *Agamonema*, Dies, an exterior glandular mass is developed round the middle intestine.—Experiments proving that pure urea never causes convulsive disorders, by MM. Feltz and Ritter.—On two rainbows with opposite curvature, by M. Faraguet. This was observed at Agen, on April 8. The bows formed a figure like *x*.—M. Tommasi presented a new system of relays for long submarine cables.

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